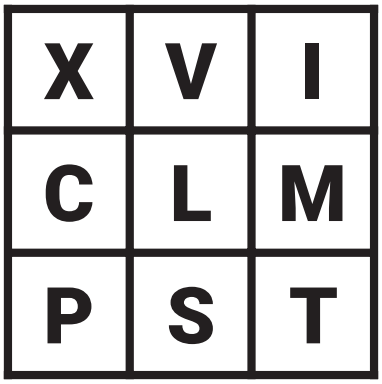
BOOK OF ABSTRACTS



16th INTERNATIONAL CONGRESS ON LOGIC, METHODOLOGY AND PHILOSOPHY OF SCIENCE AND TECHNOLOGY

BRIDGING ACROSS ACADEMIC CULTURES

Prague, 5–10 August 2019

CLMPST 2019

BOOK OF ABSTRACTS

SYMPOSIA

ACADEMIC MEANS–END KNOWLEDGE IN ENGINEERING, MEDICINE AND OTHER PRACTICAL SCIENCES. SYMPOSIUM OF THE DLMPST COMMISSION ON THE PHILOSOPHY OF TECHNOLOGY AND ENGINEERING SCIENCES

Organizer: Sjoerd Zwart

AMEK-1 Session 8D Chair: Sjoerd Zwart

AMEK-2 Session 9D Chair: Sjoerd Zwart

Congress section(s): C8

The difference between Theoretical and Practical reason has a long history in philosophy. Modern discussions concentrate on the relation between know-how and knowing-that, and ask whether one of two reduces to the other, or, if not, what the nature is of know-how. During the last decades, practical scientists in the information and social sciences (management, psychology, and law) have recognized the need to discern 'procedural or action means-end knowledge,' which may often be paraphrased as follows: 'if one wants to achieve goal G in (technical, medical, etc.) context C, perform action A.' This type of explicit (intersubjective—not tacit), or normative action knowledge seems hardly to be directly deducible from declarative scientific knowledge. Nevertheless, it prominently precipitates in countless patents and valuable academic research projects aiming at means-end or intervention knowledge. Despite its fundamental importance it has escaped the attention of most epistemologists. The purpose of this Symposium is to draw attention to, discuss and foster further interest in the production and results of academic (explicit, action) means-end knowledge in engineering, medicine, management or any other branch of practical science.

ADOLF GRÜNBAUM MEMORIAL SYMPOSIUM

Organizer: Sandra Mitchell

Grünbaum-1 Session 3C Chair: Sandra Mitchell

Grünbaum-2 Session 4C Chair: Sandra Mitchell

Congress section(s): B6

Adolf Grünbaum, former president of DLMPST and IUHPS, had an extraordinary impact on philosophy of science in the 20th century. He died November, 15, 2018 at the age of 95. This symposium honors Grünbaum by considering ideas he addressed in his work, spanning philosophy of physics, logic of scientific reasoning, Freud and psychiatry's status as a science and religion.

APPROACHING PROBABILISTIC TRUTHS, IN COMPARISON WITH APPROACHING DETERMINISTIC TRUTHS

Organizers: Theo Kuipers and Ilkka Niiniluoto

APT-1 Session 5D Chair: Theo Kuipers

APT-2 Session 6D Chair: Ilkka Niiniluoto

Congress section(s): B2

It is a widespread view among more-or-less realist philosophers of science that scientific progress consists in approach towards truth or increasing verisimilitude. This position has been elaborated within the fallibilist program of Karl Popper, who emphasized that scienti_c theories are always conjectural and corrigible, but still later theories may be "closer to the truth" than earlier ones. After the debunking of Popper's own definition of truthlikeness by David Miller and Pavel Tichý, a number of approaches have been developed in order to solve or to circumvent this problem (an early overview is found in Kuipers 1987). The logical problem of verisimilitude consists in finding an optimal definition of closer to the truth or the distance to the truth. The epistemic problem of verisimilitude consists in evaluating claims of truth approximation in the light of empirical evidence and non-empirical characteristics.

So far, post-Popperian theories of truth approximation have usually assumed, like Popper's own failing attempt, some kind of deterministic truth to be approached. This target could be descriptive or factual truth about some domain of reality, as expressed by universal laws, or the nomic truth about what is physically or biologically possible. These approaches, including most of the recent ones, are in agreement about the assumption that 'the truth' concerns a deterministic truth. However, they are deviating from each other in some other essential respects, especially concerning questions of logical reconstruction (qualitative vs. quantitative, syntactic vs. semantic, disjunction- vs. conjunction-based, content- vs. likeness-based) or concerning adequacy conditions for verisimilitude. Some useful overviews have been published about the state of the art (cf. Niiniluoto 1998, Oddie 2014).

In the symposium, adherents of such theories will now direct their attention to designing extensions to approaching probabilistic truths. Here the truth concerns a collection of statistical facts or the objective probabilities of some process, or probabilistic laws. Again the task is to find appropriate measures for the distance to such probabilistic truths and to evaluate claims about such distances on the basis of empirical evidence. Moreover, various well-known probabilistic enterprises can be (re-)construed as also dealing with truth approximation, if applied in such probabilistic contexts. For example, Carnapian inductive logic can be seen in this light (Festa, 1993). Similarly for straightforward Bayesian approaches, if applied in such contexts. Such reconstructions will also be addressed, including the interesting question whether these reconstructions can be seen as concretizations of deterministic truth approximation. In other words, one may ask whether deterministic measures of truthlikeness are special or limiting cases of probabilistic ones.

The main aim of this symposium is to bring together the search for such extensions and reconstructions. The significance is of course that the unified perspective on deterministic and probabilistic truth approximation will be illuminating and will stimulate further separate and comparative research.

ASSOCIATION FOR THE PHILOSOPHY OF MATHEMATICAL PRACTICE SYMPOSIUM

Organizers: Silvia De Toffoli and Andrew Arana

APMP-1 Session 5C Chair: Dirk Schlimm

APMP-2 Session 6C Chair: Dirk Schlimm

APMP-3 Session 7C Chair: Silvia De Toffoli

APMP-4 Session 8C Chair: Silvia De Toffoli

APMP-5 Session 9C Chair: Silvia De Toffoli

Congress section(s): C1

The philosophy of mathematics has experienced a very significant resurgence of activity during the last 20 years, much of it falling under the widely used label "philosophy of mathematical practice." This is a general term for a gamut of approaches which can also include interdisciplinary work. APMP members promote a broad, outward_looking approach to the philosophy of mathematics, which engages with mathematics in practice, including issues in history of mathematics, the applications of mathematics, and cognitive science. In 2009 the Association for the Philosophy of Mathematical Practice (APMP) was founded — for more information, see: http://philmathpractice.org/.

In this symposium, we aim at grouping twelve submission falling under the scope of APMP. The different contributions will put into focus different aspects of the philosophy of mathematical practice––both in term of topics and of methods––and with grouping them together we aim at promoting dialogue between them. We include studies of a wide variety of issues concerned with the way mathematics is done, evaluated, and applied, and in connection therewith, with historical episodes or traditions, applications, educational problems, cognitive questions, etc.

APMP aims to become a common forum that will stimulate research in philosophy of mathematics related to mathematical activity, past and present. It also aims to reach out to the wider community of philosophers of science and stimulate renewed attention to the very significant, and philosophically challenging, interactions between mathematics and science. Therefore, it is just natural that a symposium is being submitted to this Congress on behalf of APMP. We asked the members of APMP to submit a proposal for taking part in this meeting and we made an appropriate selection of submission so as to shape a one-day program. The aim of the meeting is to manifest the presence and activity of APMP within the larger community of

philosophers of science and logicians. In order to reach this aim we have opted for the format of twelve presentations that showcase the diversity of philosophical work done under the umbrella of APMP.

BOLZANO'S MATHEMATICS AND THE GENERAL METHODOLOGY OF THE SCIENCES

Organizer: Steve Russ

BMMS-1 Concepts and methodology Session 17B Chair: Steve Russ

BMMS-2 The mathematical infinite and other problems Session 18B Chair: Arianna Betti

BMMS-3 Mathematical manuscripts Session 19B Chair: Peter Simons

BMMS-4 Kinds of numbers Session 20B Chair: Steve Russ

Congress section(s): B6

The resources for study and scholarship on the thought and writings of Bernard Bolzano (Prague, 1781-1848) have been transformed by the ongoing publication of the Bernard Bolzano-Gesamtausgabe (Frommann-Holzboog, Stuttgart, 1969 -). This edition is projected to have 132 volumes, of which 99 have already appeared. (See https://www.frommann-holzboog.de/editionen/20.) The prodigious scale of the work testifies to the wide spectrum of Bolzano's interests and insights, ranging from his theology lectures and 'edifying discourses', through social, political and aesthetic themes, to his major works on philosophy, logic, mathematics and physics. In his thinking and his life he personified the congress theme of, 'Bridging across academic cultures'. The availability of so much previously unpublished, and significant, material has contributed to an increasing momentum in recent decades for Bolzano-related research, including: publications, PhD theses, translations, conferences, projects, reviews and grant awards. More than half of the Gesamtausgabe volumes, overall, are devoted to methodological or mathematical subjects.

The topic, and purpose, of this symposium is the presentation, and representation, of this thriving area of research which encompasses the history and philosophy of science and mathematics.

CAN THE HISTORY OF SCIENCE BE USED TO TEST PHILOSOPHY? SYMPOSIUM OF THE JOINT COMMISSION OF THE IUHPST 2

Organizer: Hasok Chang

JC2-1 Session 7A Chair: Jouni-Matti Kuukkanen

JC2-2 Session 8A Chair: Jouni-Matti Kuukkanen

Congress section(s): B1

This symposium examines the evidential relations between history and philosophy from various angles. Can the history of science show evidential support and falsifications for the philosophical theories about science? Or is it always a case of stalemate in which each reconstruction of history is only one possible reconstruction amongst several others? One suggestion has naturally been that the whole approach aimed at testing and comparing alternative philosophical models by recourse to historical data is misguided at worst, or in need of serious reformulation at best.

The tradition that looms large over this discussion is the attempt to turn philosophy of science into an empirically testable discipline. History and philosophy of science is then understood as a science of science in a close analogy to the natural sciences. One view is that philosophers provide theories to test and historians produce data by which these theories are tested. The most vocal and well-known representative of this approach is the VPI (Virginia Polytechnic Institute) project. The two most notable publications of this endeavour are "Scientific Change: Philosophical Models and Historical Research" and Scrutinizing Science: Empirical Studies of Scientific Change. A conference organised in 1986 preceded the latter publication. The key idea is testability; that historical case studies perform the role of empirical validation or falsification of the philosophical models of science. In this way, case studies were meant to provide 'a reality check for philosophy of science.' It is the role and status of case studies, and the rationale using case studies, that is brought back to the table and in the locus of this symposium. More generally, the authors are probing the appropriate evidential relationship between history

and philosophy. The symposium makes evident a new sticking point in the debate regarding the empirical accountability of philosophical theories: Should very recent science rather than the history of science function as a source of empirical information? Or should we rather focus on finding more sophisticated evidential modes for the history of science?

CLIMATE CHANGE: HISTORY AND PHILOSOPHY OF SCIENCE AND NATURE OF SCIENCE CHALLENGES. SYMPOSIUM OF THE INTER-DIVISIONAL TEACHING COMMISSION OF THE IUHPST

Organizer: Paulo Maurício

IDTC-1 Session 7B Chair: Jeroen Hopster

IDTC-2 Session 8B Chair: Ludovic Touze-Peiffer

Congress section(s): C2

The study of Climate Change as a philosophic subject was until recent times at very early stages (Winsberg 2018). The first entry related to 'Climate Science' in the Stanford Encyclopedia of Philosophy appear as late as 2018 (Parker, 2018). This is more awkward if we recall several of the main issues related to Climate Change and the scientific practice associated: epistemic trust, models, risk, uncertainty, probability, values, data, instruments, and complexity among many others.
Also, the bridge between research on Climate Change and policy and social spheres create problems that are not settled such as the epistemic trust or, in some other communities the relation between science and non-science.
At the same time, the development of the philosophical study of Climate Change can convey new educational insights to teach a diversity of philosophical topics. This is particularly relevant to the philosopher of science engaged in 'social relevant philosophy' but also to all the other philosophers of science.
This Symposium aims to bring together philosophers of science prone to shed light upon the above issues and correlated ones.
References:
Winsberg, Eric (2018) Philosophy and Climate Change. Cambridge, MA; Cambridge University Press
Katzav, Joel & Parker, Wendy S. (2018). Issues in the Theoretical Foundations of Climate Science. Studies in History and Philosophy of Modern Physics 63: 141-149.
Parker, Wendy, ""Climate Science"", The Stanford Encyclopedia of Philosophy (Summer 2018 Edition), Edward N. Zalta (ed.), URL https://plato.stanford.edu/archives/sum2018/entries/climate-science/.

COMMITMENTS OF FOUNDATIONAL THEORIES

Organizer: Mateusz Łełyk

CFT-1 Session 26J Chair: Mateusz Łełyk

CFT-2 Session 27I Chair: Mateusz Łełyk

Congress section(s): A1

The aim of our symposium is twofold. Firstly, we provide a unified approach to a number of contemporary logico-philosophical results and propose to see them as being about the commitments of various prominent foundational theories. Secondly, we give an overview of formal results obtained over the past few years which shed new light on commitments of both arithmetical theories and theories of sets.
The rough intuition is that commitments of a theory are all the restrictions on the ways the world might be, which are imposed on us given that we accept all the basic principles of the theory. For clarification, during the symposium we focus on the following two types of commitments of a given foundational theory Th:
1. Epistemic commitments are all the statements in the language of Th (or possibly, in the language of Th extended with the truth predicate) that we should accept given that we accept Th.
2. Semantic commitments are all the restrictions on the class of possible interpretations of Th generated by the acceptance of a theory of truth over Th.

In the context of epistemic commitments, several authors have claimed that a proper characterisation of a set of commitments of Th should take the form of an appropriate theory of truth built over Th (see, for example, [Feferman 91], [Ketland 2005] and [Nicolai,Piazza 18]). During the symposium we give an overview of the latest results concerning the Tarski Boundary - the line demarcating the truth theories which generate new implicit commitments of Peano Arithmetic (PA) from the ones which do not. Moreover, we investigate the role of a special kind of reducibility, feasible reducibility, in this context and prove some prominent theories of compositional truth to be feasibly reducible to their base theories.
A different approach to characterize the epistemic commitments of a foundational theory Th was given in [Cieśliński 2017]. Its basic philosophical motivation is to determine the scope of implicit commitments via an epistemic notion of believability. One of the symposium talks will be devoted to presenting this framework.
While investigating the epistemic commitments of Th, we look at the consequences of truth theories in the base truth-free language. Within this approach, a truth theory Th_1 is at least as committing as Th_2 if Th_1 proves all the theorems of Th_2 in the base language. In the semantic approach, one tries to understand every possible condition which truth theories impose on the class of models of Th, instead of looking only at the conditions which are expressible in the base language. A theory Th_1 is at least as semantically committing as Th_2 if for every condition which Th_2 can impose on models of PA, the same condition is imposed already by Th_1. During the symposium we present and compare the latest formal results concerning the semantical commitments of various truth theories extending two of the most distinguished foundational theories: PA and Zermelo-Fraenkel set theory (ZF). During the talks we discuss the philosophical meaning of these developments.
References:
[Cieśliński 2017] The Epistemic Lightness of Truth, Cambridge University Press.
[Feferman 1991] Reflecting on Incompleteness, Journal of Symbolic Logic, 56(1), 1-49.
[Ketland 2005] Deflationism and the Godel Phenomena: reply to Tennant, Mind, 114(453), 75-88.
[Nicolai, Piazza 2018] The Implicit Commitments of Arithmetical Theories and its Semantic Core, Erkenntins.

COMMUNICATION AND EXCHANGES AMONG SCIENTIFIC CULTURES. SYMPOSIUM OF THE IUHPST COMMISSION INTERNATIONAL ASSOCIATION FOR SCIENCE AND CULTURAL DIVERSITY

Organizers: Nina Atanasova, Karine Chemla, Vitaly Pronskikh and Peeter Müürsepp

CESC-1 Sharing, recycling, trading, and other forms of circulation Session 28J Chair: Nina Atanasova

CESC-2 Circulation of epistemological elements Session 29G Chair: Vitaly Pronskikh

CESC-3 Practices of communication Session 30G Chair: Peeter Müürsepp

CESC-4 Comparing modes of recycling Session 31G Chair: Karine Chemla

Congress section(s): B3

This symposium is predicated upon the assumption that one can distinguish between different scientific cultures. This is the founding hypothesis of the IASCUD commission. The distinction between these scientific cultures can be made on the basis of the bodies of knowledge actors uphold (which present differences depending on the culture) and the scientific practices they adopt; the distinct kinds of material environment that actors shaped to operate in these contexts and how they operate with them; and also on the basis of epistemological facets. Among the facets that appear to be useful to differentiate cultures, we include: epistemic and epistemological values; types of questions and answers that are expected; types of explanation and understanding that actors hope for. This approach to scientific cultures has the potential of allowing us to understand cultures as temporary formations and not as fixed entities.
The aim of this symposium is to focus on the types of circulation that can be identified between cultures conceived along these lines and also on how these various phenomena of circulation can help us approach the historicity of scientific cultures and of their relationship with one another. The issues we would like to address include the following:
• What can circulate between scientific cultures? We are interested in cases when knowledge and practices migrate from one culture to another. We are also interested in the borrowing of material elements and practices, as well as the adoption of epistemological choices and practices from one context into another. Events of this latter type have perhaps been studied to a lesser extent, but they seem to us to deserve specific attention.

• How does the circulation affect what is circulated? If we all agree that the adoption of an element of knowledge or practice in a different environment transforms this element, we lack a systematic approach to these phenomena of “recycling”.
• How does the circulation affect the adopting culture, and its relationship with the culture of origin? How can it elicit a reconfiguration of the scientific cultures in presence? The study of how actors revise their knowledge in the light of new elements falls for us under this broader category of questions. However, if we consider circulation in the wider perspective that we advocate, the issue of revision presents itself in a new light. In the symposium, we aim at promoting the study of revision more broadly.

DENIAL OF FACTS: INSTRUMENTATION OF SCIENCE, CRITICISM, AND FAKE NEWS. SYMPOSIUM ORGANIZED BY DLMPST/IUHPST AND THE INTERNATIONAL SCIENCE COUNCIL

Organizers: Benedikt Loewe and Daya Reddy

ISC-1 Session 5B Chair: Benedikt Loewe

ISC-2 Session 6B Chair: Benedikt Loewe

Congress section(s): B5

For scientists and rational thinkers, the increasing acceptance of positions that constitute outright denial of established scientific consensus is disconcerting. In recent years, science denial movements have become more vocal and widespread, from climate change deniers via vaccination opponents to politicians whose statements are directly and openly in contradiction with established facts. The phenomenon of denial of (scientific) facts used to be confined to the fringes of our societies, but now transformed to have relevant policy effects with long-term consequences for all people and the entire globe. Both logic and philosophy of science can contribute to our understanding of this phenomenon and possibly show paths to react to it and deal with it.
In this symposium, representatives of the International Science Council, the global umbrella organisation for all of the natural and social sciences, will engage with logicians and philosophers of science and discuss both the philosophical theories underlying the phenomenon of denial of facts and their potential consequences for science policy makers and other stakeholders.

EPISTEMIC AND ETHICAL INNOVATIONS IN BIOMEDICAL SCIENCES

Organizer: David Casacuberta

EAIBS-1 Session 30D Chair: David Casacuberta

EAIBS-2 Session 31D Chair: David Casacuberta

Congress section(s): C4

About 90% of the biomedical data accessible to researchers was created in the last two years. This certainly implies complex technical problems on how to store, analyze and distribute data, but it also brings relevant epistemological issues. In this symposium we will present some of such problems and discuss how epistemic innovation is key in order to tackle such issues.
Databases implied in biomedical research are so huge that they rise relevant questions about how scientific method is applied, such as what counts as evidence of a hypothesis when data can not be directly apprehended by humans, how to distinguish correlation from causation, or in which cases the provider of a database can be considered co-author of a research paper. To analyze such issue current characterizations of hypothesis formation, causal link, or authorship do not hold, and we need some innovation in the methodological and epistemic fields in order to revise these and other relevant concepts.
At the same time, due to the fact that a relevant deal of such biomedical data is linked to individual people, and how some knowledge from biomedical sciences can be used to predict and transform human behavior, there are ethical questions difficult to solve as they imply new challenges. Some of the them are in the awareness field, so patients and citizens understand these new ethical problems that didn’t arise before the development of big data; others relate to the way in which scientists can and can’t store, analyze and distribute information, and some others relate to the limits on which technologies are ethically safe and which bring erosion of basic human rights.
During the symposium we will present a coherent understanding on what is epistemic innovation, some of logical tools necessary for its development, and then we will discuss several cases on how epistemic innovation applies to different aspect of the biomedical sciences, also commenting its relevance when tackling ethical problems that arise in contemporary biomedical sciences.

FACTIVITY OF UNDERSTANDING: MOVING BEYOND THE CURRENT DEBATES. SYMPOSIUM OF THE EENPS

Organizer: Lilia Gurova

EENPS-1 Session 11K Chair: Lilia Gurova

EENPS-2 Session 12K Chair: Daniel Kostic

Congress section(s): B1

There are several camps in the recent debates on the nature of scientific understanding. There are factivists and quasi-factivists who argue that scientific representations provide understanding insofar as they capture some important aspects of the objects they represent. Representations, the (quasi-)factivists say, yield understanding only if they are at least partially or approximately true. The factivist position has been opposed by the non-factivists who insist that greatly inaccurate representations can provide understanding given that these representations are effective or exemplify the features of interest. Both camps face some serious challenges. The factivists need to say more about how exactly partially or approximately true representations, as well as nonpropositional representations, provide understanding. The non-factivists are expected to put more effort into the demonstration of the alleged independence of effectiveness and exemplification from the factivity condition. The aim of the proposed symposium is to discuss in detail some of these challenges and to ultimately defend the factivist camp.
One of the biggest challenges to factivisim, the existence of non-explanatory representations which do not possess propositional content but nevertheless provide understanding, is addressed in ‘Considering the Factivity of Non-explanatory Understanding’. This paper argues against the opposition between effectiveness and veridicality. Building on some cases of non-explanatory understanding, the author shows that effectiveness and veridicality are compatible and that we need both.
A different argument for the factivity of scientific understanding provided by models containing idealizations is presented in ‘Understanding Metabolic Regulation: A Case for the Factivists’. The central claim of this paper is that such models bring understanding if they capture correctly the causal relationships between the entities, which these models represent.
‘Effectiveness, Exemplification, and Factivity’ further explores the relation between the factivity condition and its suggested alternatives – effectiveness and exemplification. The author’s main claim is that the latter are not alternatives to factivity, strictly speaking, insofar as they could not be construed without any reference to truth conditions.
‘Scientific Explanation and Partial Understanding’ focuses on cases where the explanations consist of propositions, which are only partially true (in the sense of da Costa’s notion of partial truth). The author argues that such explanations bring partial understanding insofar as they allow for an inferential transfer of information from the explanans to the explanandum.
What happens, however, when understanding is provided by explanations which do not refer to any causal facts? This question is addressed in ‘Factivity of Understanding in Non-causal Explanations’. The author argues that the factivity of understanding could be analyzed and evaluated by using some modal concepts that capture “vertical” and “horizontal” counterfactual dependency relations which the explanation describes.

FORMALISM, FORMALIZATION, INTUITION AND UNDERSTANDING IN MATHEMATICS: FROM INFORMAL PRACTICE TO FORMAL SYSTEMS AND BACK AGAIN

Organizer: Mate Szabo

FFIUM-1 Session 28C Chair: Gerhard Heinzmann

FFIUM-2 Session 29C Chair: Gerhard Heinzmann

FFIUM-3 Session 30C Chair: Gerhard Heinzmann

Congress section(s): C1

This project investigates the interplay between informal mathematical theories and their formalization, and argues that this dynamism generates three different forms of understanding:
1. Different kinds of formalizations fix the boundaries and conceptual dependences between concepts in different ways, thus contributing to our understanding of the content of an informal mathematical theory. We argue that this form of understanding of an informal theory is achieved by recasting it as a formal theory, i.e. by transforming its expressive means.
2. Once a formal theory is available, it becomes an object of understanding. An essential contribution to this understanding is made by our recognition of the theory in question as a formalization of a particular corpus of informal mathematics. This form of understanding will be clarified by studying both singular intended models, and classes of models that reveal the underlying conceptual commonalities between objects in different areas of mathematics.
3. The third level concerns how the study of different formalizations of the same area of mathematics can lead to a transformation of the content of those areas, and a change in the geography of informal mathematics itself.
In investigating these forms of mathematical understanding, the project will draw on philosophical and logical analyses of case studies from the history of mathematical practice, in order to construct a compelling new picture of the relationship of formalization to informal mathematical practice. One of the main consequences of this investigation will be to show that the process of acquiring mathematical understanding is far more complex than current philosophical views allow us to account for.
While formalization is often thought to be negligible in terms of its impact on mathematical practice, we will defend the view that formalization is an epistemic tool, which not only enforces limits on the problems studied in the practice, but also produces new modes of reasoning that can augment the standard methods of proof in different areas of mathematics. Reflecting on the interplay between informal mathematical theories and their formalization means reflecting on mathematical practice and on what makes it rigorous, and how this dynamism generates different forms of understanding. We therefore also aim to investigate the connection between the three levels of understanding described above, and the notion of rigor in mathematics. The notion of formal rigor (in the proof theoretic sense) has been extensively investigated in philosophy and logic, though an account of the epistemic role of the process of formalization is currently missing. We argue that formal rigor is best understood as a dynamic abstraction from informally rigorous mathematical arguments. Such informally rigorous arguments will be studied by critically analyzing case studies from different subfields of mathematics, in order to identify patterns of rigorous reasoning.

FROM CONTRADICTION TO DEFECTIVENESS TO PLURALISM

Organizers: María Del Rosario Martínez Ordaz and Otávio Bueno

FCDP-1 Session 24D Chair: Maria Del Rosario Martinez Ordaz

FCDP-2 Session 25D Chair: Maria Del Rosario Martinez Ordaz

FCDP-3 Session 26D Chair: Maria Del Rosario Martinez Ordaz

FCDP-4 Session 27D Chair: Otávio Bueno

Congress section(s): B1

In their day-to-day practice, scientists make constant use of defective (false, imprecise, conflicting, incomplete, inconsistent etc.) information. The philosophical explanations of the toleration of defective information in the sciences are extremely varied, making philosophers struggle at identifying a single correct approach to this phenomenon. Given that, we adopt a pluralist perspective on this issue in order to achieve a broader understanding of the different roles that defective information plays (and could play) in the sciences.
This symposium is devoted to exploring the connections between scientific pluralism and the handling of inconsistent as well as other types of defective information in the sciences. The main objectives of this symposium are (a) to discuss the different ways in which defective information could be tolerated (or handled) in the different sciences (formal, empirical, social, health sciences, etc.) as well as (b) to analyze the different methodological tools that could be used to explain and handle such type of information.
The symposium is divided into two parts: the first tackles the issue of inconsistency and scientific pluralism. This part includes discussions of the possible connections between the different ways in which scientist tolerate contradictions in the sciences and particular kinds of scientific pluralism. This analysis is extremely interesting in itself as the phenomenon of inconsistency toleration in the science has often been linked to the development of a plurality of formal approaches, but not necessarily to logical or scientific pluralism. In fact, scientific pluralism is independent of inconsistency toleration.
The second part of the symposium is concerned with a pluralistic view on contradictions and other defects. This part is devoted to explore under which circumstances (if any) it is possible to use the same mechanisms for tolerating inconsistencies and for dealing with other types of defective information. This part includes reflections on the scope of different formal methodologies for handling defectiveness in the sciences as well as considerations on scientific communicative practices and their connections with the use of defective information and reflections on the different epistemic commitments that scientists have towards defective information.

IDENTITY IN COMPUTATIONAL FORMAL AND APPLIED SYSTEMS. HAPOC SYMPOSIUM

Organizers: Giuseppe Primiero and Nicola Angius

IdCFAS-1 Session 28A Chair: Raymond Turner

IdCFAS-2 Session 29A Chair: Raymond Turner

Congress section(s): C6

Defining identity between two objects is a fundamental problem in several philosophical disciplines, from logic to language and formal ontology. Since Frege, identity has been addressed in terms of formal constraints on definitional criteria which vary depending on context, application and aims. This symposium collects and compares current approaches to identity for computational systems in formal and applied contexts. Problems of interest include: definitional identity in arithmetics, intensional identity for proofs, the definition of replicas and the study of preservation of second-order properties for copied computational artefacts, and the identity over time of formally defined social institutions. All these contexts offer problematic interpretations and interesting questions for the notion of identity.
Arithmetics offers a precise formal interpretation of logical identity, but higher types display a tension between extensionality and equivalent term evaluation of identical functions: if the latter is accepted, then functions are co-definable but irreducible.
In proof-theoretical semantics a sentence is identified by the set of all its proofs with a common inferential structure. Accounting for intensional aspects of these objects means to uphold their identity, while investigating common meta-theoretical properties like harmony and stability.
From formal to implemented objects, the problem of identity resurfaces for computational artefacts. For these objects, by definition subject to replication, the notion of copy has started receiving formal treatment in the literature, while the notion of replica can be further analysed with respect to existing approaches for technical artefacts. Moreover, the problem of preservation of behavioural properties like safety and reliability is crucial.
Finally, these problems extend to applications in social ontology. In particular, identity criteria are at the basis of an ontological analysis of the persistence of organisations through time and changes, a problem which can be formulated both theoretically and formally.

The problem of defining formal identity criteria for natural and technical objects traces back to ancient philosophy and it characterises modern and contemporary analytic ontology from Leibniz to Frege. This symposium collects contemporary analyses of the logical accounts of identity in formal and applied contexts.

INTERNATIONAL YEAR OF THE PERIODIC TABLE

Organizers: Gisela Boeck and Benedikt Loewe

IYPT-1 Session 26G Chair: Gisela Boeck

IYPT-2 Session 27G Chair: Gisela Boeck

Congress section(s): B6

The year 2019 is the International Year of the Periodic Table (IYPT), celebrating the 150th anniversary of its year of discovery, and the International Union for History and Philosophy of Science and Technology (IUHPST) is one of the supporting institutions of IYPT.
With this event at CLMPST 2019, we aim to offer all participants of the congress, independent of whether they are working in philosophy of chemistry or not, an insight into the relevance and important of the Periodic Table. The event consists of talks for a general academic audience, with a non-technical historical introduction by Hasok Chang, two personal reflections by current or recent graduate students in philosophy of chemistry, and a local point of view by a expert from Prague, Soňa Štrbáňová.

KARL POPPER: HIS SCIENCE AND HIS PHILOSOPHY

Organizer: Zuzana Parusniková

KRP-1 Session 11C Chair: Zuzana Parusniková

KRP-2 Session 12C Chair: Zuzana Parusniková

KRP-3 Session 13C Chair: Zuzana Parusniková

KRP-4 Session 14C Chair: Zuzana Parusniková

KRP-5 Session 15C Chair: Zuzana Parusniková

Congress section(s): B6

Of all philosophers of the 20th century, few built more bridges between academic disciplines than did Karl Popper. For most of his life, Karl Popper made contributions to a wide variety of fields in addition to the epistemology and the theory of scientific method for which he is best known. Problems in quantum mechanics, and in the theory of probability, dominate the second half of Popper's Logik der Forschung (1934), and several of the earliest items recorded in §2 ('Speeches and Articles') of Volume 1 of The Writings of Karl Popper, such as item 2-5 on the quantum-mechanical uncertainty relations, item 2-14 on nebular red-shifts, and item 2-43 (and other articles) on the arrow of time, show his enthusiasm for substantive problems in modern physics and cosmology. Interspersed with these were a number of articles in the 1940s on mathematical logic, and in the 1950s on the axiomatization of the theory of probability (and on other technical problems in this area). Later he made significant contributions to discussions in evolutionary biology and on the problem of consciousness. All these interests (except perhaps his interest in formal logic) continued unabated throughout his life.
The aim of this symposium is to illustrate, and to evaluate, some of the interventions, both substantive and methodological, that Karl Popper made in the natural and mathematical sciences. An attempt will be made to pinpoint the connections between these contributions and his more centrally philosophical concerns, especially his scepticism, his realism, his opposition to subjectivism, and his indeterminism.
The fields that have been chosen for the symposium are quantum mechanics, evolutionary biology, cosmology, mathematical logic, statistics, and the brain-mind liaison.

LOGIC, AGENCY, AND RATIONALITY

Organizers: Valentin Goranko and Frederik Van De Putte

LoARa-1 Session 11B Chair: Valentin Goranko

LoARa-2 Session 12B Chair: Valentin Goranko

LoARa-3 Session 13B Chair: Valentin Goranko

LoARa-4 Session 14B Chair: Frederik Van De Putte

LoARa-5 Session 15B Chair: Frederik Van De Putte

LoARa-6 Session 16B Chair: Frederik Van De Putte

Congress section(s): A2

The concept of rational agency is broadly interdisciplinary, bringing together philosophy, social psychology, sociology, decision and game theory. The scope and impact of the area of rational agency has been steadily expanding in the past decades, also involving technical disciplines such as computer science and AI, where multi-agent systems of different kinds (e.g. robotic teams, computer and social networks, institutions, etc.) have become a focal point for modelling and analysis. Rational agency relates to a range of key concepts: knowledge, beliefs, knowledge and communication, norms, action and interaction, strategic ability, cooperation and competition, social choice etc. The use of formal models and logic-based methods for analysing these and other aspects of rational agency has become an increasingly popular and successful approach to dealing with their complex diversity and interaction.
This symposium will bring together different perspectives and approaches to the study of rational agency and rational interaction in the context of philosophical logic.
The symposium talks are divided into three thematic clusters, each consisting of 4-5 presentations, as follows.
I. Logic, Rationality, and Game-theoretic Semantics. Applying logic-based methods and formal logical systems to reasoning in decision and game theory is a major and increasingly popular approach to agency and rationality. Formal logical languages allow us to specify principles of strategic behaviour and interaction between agents, and essential game-theoretic notions, including solution concepts and rationality principles. Formal logical systems provide precise and unambiguous semantics and enable correct and reliable reasoning about these, while involving the concepts of knowledge, beliefs, intentions, ability, etc.
II. Deontic Logic, Agency, and Action. Logics of agency and interaction such as STIT and deontic logics have been very influential and generally appreciated approaches to normative reasoning and theory of actions. Active directions of research in this area include the normative status of actions vs. propositions, causality and responsibility, collective and group oughts and permissions, and further refinements of the STIT framework stemming from the works of Belnap, Horty and others.
III. Logic, Social Epistemology, and Collective Decision-making. Rational agency and interaction also presuppose an epistemological dimension, while intentional group agency is inextricably linked to social choice theory. In this thematic cluster, various logical and formal models are discussed that allow shedding light on these factors and processes.

MARIO BUNGE: APPRAISING HIS LONG-LIFE'S CONTRIBUTION TO PHILOSOPHY

Organizer: Michael Matthews

MBA-1 Session 9F Chair: Jean-Pierre Marquis

MBA-2 Session 10F Chair: Jean-Pierre Marquis

Congress section(s): B7

As Mario Bunge celebrates his 100th birthday, this symposium will appraise four different aspects of his life-long contribution to philosophy.
Bunge was born in Argentina on 21st September 1919. He has held chairs in physics and in philosophy at universities in Argentina, the USA, and since 1966 a philosophy chair at McGill University. He has published 70 books (many with revised editions) and 540 articles; with many translated into one or other of twelve languages.
Bunge has made substantial research contributions to an unequalled range of fields: physics, philosophy of physics, metaphysics, methodology and philosophy of science, philosophy of mathematics, logic, philosophy of psychology,

philosophy of social science, philosophy of biology, philosophy of technology, moral philosophy, social and political philosophy, management theory, medical philosophy, linguistics, criminology, legal philosophy, and education.
Bunge's remarkable corpus of scientific and philosophical writing is not inert; it has had significant disciplinary, cultural and social impact. In 1989 the American Journal of Physics asked its readers to vote for their favourite papers from the journal in the sixty years since its founding in 1933. Bunge's 1956 'Survey of the Interpretations of Quantum Mechanics' was among the 20 top voted papers. In 1993, the journal repeated the exercise this time Bunge's 1966 paper 'Mach's Critique of Newtonian Mechanics' – joined his first paper in the top 20.
Beyond breadth, Bunge's work is noteworthy for its coherence and systemicity. Through to the mid twentieth-century most significant Western philosophers were systematic philosophers. But in the past half-century and more, the pursuit of systemic philosophy, 'big pictures', 'grand narratives' or even cross-disciplinary understanding has considerably waned. Bunge has defied this trend. His philosophical system was laid out in detail in his monumental eight-volume Treatise on Basic Philosophy (1974-1989). Individual volumes were devoted to Semantics, Ontology, Epistemology, Systemism, Philosophy of Science, and Ethics. His Political Philosophy: Fact, Fiction and Vision (2009) was originally planned as its ninth volume.
Bunge has applied his systems approach to issues in logic, mathematics, physics, biology, psychology, social science, technology, medicine, legal studies, economics, and science policy.
Bunge's life-long commitment to Enlightenment-informed, socially-engaged, systemic philosophy is manifest in his being asked by the Academia Argentina de Ciencias Exactas, Físicas y Naturales to draft its response to the contemporary crisis of anthropogenic global warming. Bunge authored the Manifesto which was signed by numerous international associations. Guided by his own systematism he wrote: since climate is not regional but global, all the measures envisaged to control it should be systemic rather than sectoral, and they should alter the causes at play – mechanisms and inputs – rather than their effects.
Clearly Bunge is one of the most accomplished, informed, wide-ranging philosophers of the modern age. This symposium, held in the year that he, hopefully, celebrates his 100th birthday, is an opportunity for the international philosophical community to appraise his contribution to the discipline.

MESSY SCIENCE. SYMPOSIUM OF THE JOINT COMMISSION OF THE IUHPST 3

Organizer: Hasok Chang

JC3-1Session 3A Chair: Catherine Kendig

JC3-2 Session 4A Chair: Catherine Kendig

Congress section(s): B1

It is often said that science is "messy" and, because of this messiness, abstract philosophical thinking is only of limited use in analysing science. But in what ways is science messy, and how and why does this messiness surface? Is it an accidental or an integral feature of scientific practice? In this symposium, we try to understand some of the ways in which science is messy and draw out some of the philosophical consequences of taking seriously the notion that science is messy.
The first presenter discusses what scientists themselves say about messy science, and whether they see its messiness as a problem for its functioning. Examining scientists' reflections about "messy science" can fulfill two complementary purposes. Such an analysis helps to clarify in what ways science can be considered "messy" and thus improves philosophical understanding of everyday research practice. The analysis also points to specific pragmatic challenges in current research that philosophers of science can help address.
The second presenter discusses the implications of "messy science" for scientific epistemology, specifically for scientific justification. They show how this messiness plays itself out in a particular episode in nineteenth century medicine: the transition from mid-nineteenth-century miasma views to late nineteenth-century early germ views by examining different senses in which scientific epistemology may be said to be messy and lay out in what ways such messy scenarios differ from the idealized circumstances of traditional accounts of justification. They conclude by discussing some limits that taking these differences into account will impose on developing practice-based views of scientific justification, explaining how it is still possible for such views to retain epistemic normativity.
The third presenter explores how the messiness of eighteenth-century botanical practice, resulting from a constant lack of information, generated a culture of collaborative publishing. Given the amount of information required for an accurate plant description let alone a taxonomic attribution, eighteenth-century botanists and their readers were fully aware of the preliminary nature of their publications. They openly acknowledged the necessity of updating and correcting them, and developed collaborative strategies for doing so efficiently. Authors updated their own writings in cycles of iterative publishing, most famously Carl Linnaeus, but this could also be done by others, such as the consecutive editors of the unpublished manuscripts of the German botanist Paul Hermann (1646-1695), who became his co-authors in the process.
The fourth presenter investigates how biological classification can sometimes rely on messy metaphysics. Focusing on the lichen symbiont, they explore what grounds we might have for relying on overlapping and conflicting ontologies. Lichens have long been studied and defined as two-part systems composed of a fungus (mycobiont) and a photosynthetic partner (photobiont). This bipartite metaphysics underpins classificatory practices and determines the criteria for stability that rely on the fungus to name lichens despite the fact that some lichens are composed of three or more parts. The presenter investigates how reliable taxonomic information can be gleaned from metaphysics that makes it problematic to even count biological individuals or track lineages.

NEW DIRECTIONS IN CONNEXIVE LOGIC

Organizers: Hitoshi Omori and Heinrich Wansing

NDCXL-1 Session 25F Chair: Heinrich Wansing

NDCXL-2 Session 26F Chair: Heinrich Wansing

NDCXL-3 Session 27F Chair: Heinrich Wansing

Congress section(s): A2

Modern connexive logic started in the 1960s with seminal papers by Richard B. Angell and Storrs McCall. Connexive logics are orthogonal to classical logic insofar as they validate certain non-theorems of classical logic, namely
Aristotle's Theses: ~(~A→A), ~(A→~A)
Boethius' Theses: (A→B)→~(A→~B), (A→~B)→~(A→B)
Systems of connexive logic have been motivated by considerations on a content connection between the antecedent and succedent of valid implications and by applications that range from Aristotle's syllogistic to Categorial Grammar and the study of causal implications. Surveys of connexive logic can be found in:
Storrs McCall, "A History of Connexivity", in D.M. Gabbay et al. (eds.), Handbook of the History of Logic. Volume 11. Logic: A History of its Central Concepts, Amsterdam, Elsevier, 2012, pp. 415-449.
Heinrich Wansing, "Connexive Logic", in Edward N. Zalta (ed.), The Stanford Encyclopedia of Philosophy (Fall 2014 Edition).
There is also a special issue on connexive logics in the IfCoLog Journal of Logics and their Applications. The entire issue is available at: http://collegepublications.co.uk/ifcolog/?00007
As we are observing some growing interests in topics related to connexive logics, collecting attention from researchers working on different areas within philosophical logic, the symposium aims at discussing directions for future research in connexive logics. More specifically, we will have talks related to modal logic, many-valued logic, probabilistic logic, relevant (or relevance) logic and conditional logic, among others. There will also be some connections to experimental philosophy and philosophy of logic.

OBSERVATION TO CAUSATION

Organizer: Frederick Eberhardt

O2C-1 Session 3B Chair: Christopher Hitchcock

O2C-2 Session 4B Chair: Christopher Hitchcock

Congress section(s): B1

Over the past few years, the Causal Bayes net framework – developed by Spirtes et. al. (2000) and Pearl (2000), and given philosophical expression in Woodward (2004) – has been successfully spun off into the sciences. From medicine to neuro- and climate-science, there is a resurgence of interest in the methods of causal discovery. The framework offers a perspicuous

representation of causal relations, and enables development of methods for inferring causal relations from observational data. These methods are reliable so long as one accepts background assumptions about how underlying causal structure is expressed in observational data. The exact nature and justification of these background assumptions has been a matter of debate from the outset. For example, the causal Markov condition is widely seen as more than a convenient assumption, and rather as encapsulating something essential about causation. In contrast, the causal faithfulness assumption is seen as more akin to a simplicity assumption, saying roughly that the causal world is, in a sense, not too complex. There are other assumptions that have been treated as annoying necessities to get methods of causal discovery off the ground, such as the causal sufficiency assumption (which says roughly that every common cause is measured) and the acyclicity (which implies, for example, that there is no case in which X causes Y, Y causes Z, and Z causes X, forming a cycle). Each of these assumptions has been subject to analysis and methods have been developed to enable causal discovery even when these assumptions are not satisfied. But controversies remain, and we are confronted with some long standing questions: What exactly is the nature of each of those assumptions? Can any of those assumptions be justified? If so, which? How do the question of justification and the question of nature relate to each other?
This symposium aims to address those questions. It brings together a group of researchers all trained in the causal Bayes nets framework, but who have each taken different routes to exploring how we can address the connection between the underlying causal system and the observational data that we use as basis to infer something about that system. In particular, we will discuss a variety of different approaches that go beyond the traditional causal Bayes net framework, such as the discovery of dynamical systems, and the connection between causal and constitutive relations. While the approaches are largely driven by methodological considerations, we expect these contributions to have implications for several other philosophical debates in the foundations of epistemology, the metaphysics of causation, and on natural kinds.

PARTICLES, FIELDS, OR BOTH?

Organizer: Charles Sebens

PFB-1 Session 19I Chair: Christopher Hitchcock

PFB-2 Session 20I Chair: Christopher Hitchcock

Congress section(s): C2

One of the primary tasks of philosophers of physics is to determine what our best physical theories tell us about the nature of reality. Our best theories of particle physics are quantum field theories. Are these theories of particles, fields, or both? In this colloquium we will debate this question in the context of quantum field theory and in an earlier and closely related context: classical electromagnetism. We believe that the contemporary debate between particle and field interpretations of quantum field theory should be informed by a close analysis of classical electromagnetism and seek to demonstrate the fruitfulness of such a dialogue in this session.
Our first speaker will start the session by discussing the debate between Einstein and Ritz in the early 20th century over whether classical electromagnetism should be formulated as theory of particles interacting directly with one another or interacting via fields. They will discuss the technical challenges facing each approach as well as the role that philosophical and methodological presuppositions play in deciding which approach is to be preferred.
Our second speaker will defend a dual ontology of particles and fields in classical electromagnetism. They argue that the singularities which arise in the standard Maxwell-Lorentz formulation of electromagnetism are unacceptable. However, the standard equations of electromagnetism can be modified (as is done in the Born-Infeld and Bopp-Podolsky formulations).
Our third speaker will recount the problems of self-interaction that arise for a dual ontology of particles and fields in the context of classical electromagnetism and defend point particle ontology. They will go on to argue that attempts to formulate quantum field theory as a theory of fields have failed. They believe that it too should be interpreted as a theory of particles.
Our final speaker will defend a pure field ontology for quantum field theory. They will argue that quantum theories where the photon is treated as a particle are unacceptable. On the other hand, treating the electron as a field yields significant improvements over the ordinary particle interpretation.

PHILOSOPHY OF BIG DATA. HAPOC SYMPOSIUM

Organizer: Paula Quinon

Big Data-1 Session 26A Chair: Giuseppe Primiero

Big Data-2 Session 27A Chair: Giuseppe Primiero

Congress section(s): C6

The HaPoC symposium “Philosophy of Big Data” is submitted on behalf of the DLMPST, History and Philosophy of Computing division
The symposium devoted to a discussion of philosophical problems related to Big Data, an increasingly important topic within philosophy of computing. Big Data are worth studying from an academic perspective for several reasons. First of all, ontological questions are central: what Big Data are, whether we can speak of them as separate ontological entity, and what their mereological status is. Second, epistemological ones: what kind of knowledge do they induce, and what methods do they require for accessing valuable information.
These general questions have also very specific counterparts raising series of methodological questions. Should data accumulation and analysis follow the same general patterns for all Sciences, or should those be relativized to particular domains? For instance, shall medical doctors and businessmen focus on the same issues related to gathering of information? Is the quality of information similarly important in all the contexts? Can one community be inspired by experience of another? To which extent human factors influence information that we issue from Big Data?
In addition to these theoretical academic issues, Big Data represents also a social phenomenon. “Big Data” is nowadays a fancy business buzzword, which - together with “AI” and “Machine Learning” – shapes business projects and the R&D job market, with data analysts among the most attractive job titles. It is believed that “Big Data” analysis opens up unknown opportunities and generates additional profits. However, it is not clear what counts as Big Data in the industry and critical reflection about it seems necessary.
The proposed symposium gathers philosophers, scientists and experts in commercial Big Data analysis to reflect on these questions. We believe that the possibility to exchange ideas, methodologies and experiences gathered from different perspectives and with divergent objectives, will enrich not only academic philosophical reflection, but will also prove useful for practical - scientific or business - applications.

PROOF AND TRANSLATION: GLIVENKO’S THEOREM 90 YEARS AFTER

Organizers: Sara Negri and Peter Schuster

Glivenko-1 Session 24C Chair: Sara Negri

Glivenko-2 Session 25C Chair: Sara Negri

Glivenko-3 Session 26C Chair: Sara Negri

Glivenko-4 Session 27C Chair: Sara Negri

Congress section(s): A1

Glivenko’s theorem from 1929 says that if a propositional formula is provable in classical logic, then its double negation is provable within intutionistic logic. Soon after, Gödel extended this to predicate logic, which requires the double negation shift. As is well-known, with the Gödel-Gentzen negative translation in place of double negation one can even get by with minimal logic. Several related proof translations saw the light of the day, such as Kolmogorov’s and Kuroda’s.
Glivenko’s theorem thus stood right at the beginning of a fundamental change of perspective: that classical logic can be embedded into intuitionistic or minimal logic, rather than the latter being a diluted version of the former. Together with the revision of Hilbert Programme ascribed to Kreisel and Feferman, this has led to the quest for the computational content of classical proofs, today culminating in agile areas such as proof analysis, dynamical algebra, program extraction from proofs and proof mining. The considerable success of these approaches suggests that classical mathematics will eventually prove much more constructive than widely thought still today.
Important threads of current research include the following:
1. Exploring the limits of Barr’s theorem about geometric logic

2. Program extraction in abstract structures characterised by axioms
3. Constructive content of classical proofs with Zorn's Lemma
4. The algorithmic meaning of programs extracted from proofs

SCIENCE AS A PROFESSION AND VOCATION. ON STS'S INTERDISCIPLINARY CROSSROADS

Organizers: Ilya Kasavin, Alexandre Antonovskiy, Liana Tukhvatulina, Anton Dolmatov, Eugenia Samostienko, Svetlana Shibarshina, Elena Chebotareva and Lada Shipovalova

WEBPROVOC-1 Session 28B Chair: Ilya Kasavin

WEBPROVOC-2 Session 29B Chair: Ilya Kasavin

WEBPROVOC-3 Session 30B Chair: Ilya Kasavin

Congress section(s): B1, B5

The topic of the special symposium is inspired by Max Weber's lecture on "Science as a Vocation" [Wissenschaft als Beruf], which will be celebrating the 100th anniversary of its publication in 2019. The ambivalence of the German term Beruf [occupation, job, vocation] plays a crucial role in Weber's text, making it possible, on the one hand, to view science as a highly specialized activity, and on the other hand, to uncover its openness, its communicative nature, and its ethical dimension. In particular, the essay's focus on the communicative dimension of science, and its relation to ideas of social progress, brings to light the significance of human meaning and practice in the conduct of science, but also the reliability of scientific knowledge and its perceived status in society. Weber's lecture clearly remains relevant today, since it interrogates the possibility of history and philosophy of science to be both a specialized and an open project, designed to bridge the disciplinary gaps between various approaches to study science. More broadly, his essay thus presents a timely attempt to address the problem of integrating different academic cultures: philosophy and the sciences; ethics and methodology.
The call for epistemic openness should be complemented by a renewed methodological focus, including an emphasis on detailed historical and sociological research, and the development of educational practices that foster the creation of new "trading zones" (Peter Galison), in which cross-disciplinary discussions of science, technology and human values can take place. With our call, we thus invite scholars to re-engage Weber's text, from the perspective of 21st century Science and Technology Studies (STS), to help forge new forms of interdisciplinary interaction and expertise.

SOME RECENT DIRECTIONS IN MODEL THEORY

Organizer: John Baldwin

clmpsmod-1 Session 17A Chair: John Baldwin

clmpsmod-2 Session 18A Chair: John Baldwin

clmpsmod-3 Session 19A Chair: John Baldwin

Congress section(s): A1

This symposium builds on the proposed Authors and Critics session on Baldwin's book: Model Theory and the Philosophy of Mathematical Practice: Formalization without Foundationalism. A key thesis of that book asserts: Contemporary model theory enables systematic comparison of local formalizations for distinct mathematical areas in order to organize and do mathematics, and to analyze mathematical practice.
Part I: Appropriate formalization for different areas of mathematics.
Part II: Abstract elementary classes and accessible categories.

STYLES IN MATHEMATICS

Organizers: Erich Reck and Georg Schiemer

SIM-1 Session 18D Chair: Karine Chemla

SIM-2 Session 19D Chair: Erich Reck

SIM-3 Session 20D Chair: Erich Reck

Congress section(s): C1

While philosophers of mathematics usually focus on notions such as proof, theorem, concept, definition, calculation, and formalization, some historians of mathematics have also used the notion of "style" to characterize the works of various mathematicians (from Euclid and Archimedes through Riemann and Brouwer to Bourbaki and Grothendieck). One question is, then, whether that notion can have significance from a philosophical point of view, and especially, for epistemological purposes. The notion of "style" is quite ambiguous, however, both in general and as applied to mathematics. In the present context, it is often used in a sense close to "characteristic approach" or "methodology", i.e., a distinctive way of investigating, organizing, and presenting mathematical ideas (geometric, algebraic, conceptual, computational, axiomatic, intuitive, etc.); but it has also been used in a personal/psychological sense (individual style), a sociological/political sense (e.g., national styles), a literary or more broadly aesthetic sense (writing style, stylish), and as indicating a "brand" (an easily recognizable, influential, and visible style).
The seven talks in this session will explore this topic in a broad and multi-faceted way. They will investigate differences in style in ancient and medieval mathematics (not just in Greece but also in China), in early and late modern mathematics (into the 19th and 20th centuries, including Dedekind, Kronecker, Klein, and Hilbert, later also Chevalley and Grothendieck), and in current mathematics (category theory and beyond). A particular focus will be the "structuralist" style that has dominated much of mathematics since the second half of the 19th century. But "stylistic" issues concerning logic (Boole, Jevons, DeMorgan, Frege, etc.), on the one hand, and concerning more popular presentations of mathematics (e.g., in the journal The Monist), on the other, are also considered. In addition, several more general discussions of the notion of style in science, e.g., by Ludwig Fleck, G.-G. Granger, and Ian Hacking, are addressed and related to mathematics; as are questions about the dynamics of styles, i.e., the ways in which they change and get transformed over time.
Overall, it will become evident that the notion of "style" as applied to mathematics should, indeed, be seen as significant philosophically, but also as in need of further disambiguation and sharpening.

SUBSTRUCTURAL EPISTEMOLOGY

Organizers: Dominik Klein, Soroush Rafiee Rad and Ondrej Majer

SubStrE-1 Session 28D Chair: Ondrej Majer

SubStrE-2 Session 29D Chair: Ondrej Majer

Congress section(s): A2

Epistemic and Doxastic logic, on one hand, and probabilistic logics on the other, are the two main formal apparatus used in the representation of knowledge and (graded) belief. Both are striving fields that have allowed for many fruitful applications in philosophy and AI. In representing knowledge and belief, classic epistemic and deontic logic rely on a number of minimal assumptions. Agents are, for instance, usually taken to be logically omniscient and their informational states are assumed closed under logical equivalence. Within dynamic logic, also informational updates are often assumed to be correct, i.e. truthful. In the same manner in the probabilistic approach the agents beliefs are assumed to satisfy the Kolmogorov axioms for probabilities which in turn impose strong rationality and consistency conditions on these beliefs.
These assumptions are, of course, idealizations. Newly learned information can often be false or misleading and rarely satisfies classic strong consistency criteria. What is more, real agents frequently behave in ways that are incompatible with orthodox assumptions of logical and probability theory. To establish more comprehensive positive or normative theories about agents, there is hence a need for theories that are able to deal with weaker contexts where some standard assumptions are violated.
This workshop brings together a number of approaches for weak, substructural epistemic logic. The approaches discussed apply to the merging of possibly contradictory information, probabilistic assignments based on contradictory or inconclusive

information, intensional and hyper-intensional beliefs, i.e. belief states that are not closed under logical equivalence and collective epistemic states such as group knowledge among groups of weaker-than-classic agents.

SYMPOSIUM OF THE DLMPST COMMISSION ON ARABIC LOGIC

Organizer: Wilfrid Hodges

Session 5M Chair: Saloua Chatti

Congress section(s): A4

The symposium will consist of two talks that provide introductions to two areas of logic where medieval Arabic-speaking logicians made advances. One of these, presented by Wilfrid Hodges, is the use of diagrams for solving logical problems; it was only recently realised that this was achieved both accurately and insightfully in twelfth-century Baghdad. The other, presented by Saloua Chatti, is hypothetical logic, a distinctive branch of formal logic of great interest to several leading Arabic logicians, with some features of propositional logic and some of temporal logic.

SYMPOSIUM OF THE SPANISH SOCIETY OF LOGIC, METHODOLOGY AND PHILOSOPHY OF SCIENCE

Organizer: Cristina Corredor

SLMFCE 1 Session 15L Chair: Jose Martinez Fernandez

SLMFCE 2 Session 16L Chair: Jose Martinez Fernandez

The Spanish Society of Logic, Methodology and Philosophy of Science (SLMFCE in its Spanish acronym) is a scientific association formed by specialists working in these and other closely related fields. Its aims and scope cover also those of analytic philosophy in a broad sense and of argumentation theory. It is worth mentioning that among its priorities is the support and promotion of young researchers. To this aim, the Society has developed a policy of grants and awards for its younger members.

The objectives of the SLMFCE are to encourage, promote and disseminate study and research in the fields above mentioned, as well as to foster contacts and interrelations among specialists and with other similar societies and institutions. The symposium is intended to present the work carried out by prominent researchers and research groups linked to the Society. It will include four contributions in different subfields of specialization, allowing the audience at the CLMPST 2019 to form an idea of the plural research interests and relevant outcomes of our members.

SYMPOSIUM ON HIGHER BAIRE SPACES

Organizers: Lorenzo Galeotti and Philipp Lücke

SGBS-1 Session 24B Chair: Philipp Lücke

SGBS-2 Session 25B Chair: Philipp Lücke

SGBS-3 Session 26B Chair: Philipp Lücke

Congress section(s): A1

The study of sets of real numbers and their structural properties is one of the central topics of contemporary set theory and the focus of the set-theoretic disciplines of descriptive set theory and set theory of the reals. The Baire space consists of all functions from the set of natural numbers to itself. Since this space is Borel-isomorphic to the real line and has a very accessible structure, it is one of the main tools of descriptive set theory. Because a great variety of mathematical objects can be canonically represented as subsets of Baire space, techniques from descriptive set theory and set theory of the reals can be applied throughout mathematics. These applications are limited to the study of objects of cardinality at most the size of the continuum. Therefore, the question whether similar methods can be applied in the analysis of larger objects arose naturally in several areas of mathematics and led to a strongly increasing interest in the study of higher Baire spaces, i.e., higher analogues of Baire space which consist of all functions from a given uncountable cardinal to itself.

In the recent years, an active and steadily growing community of researches has initiated the development of higher analogues of descriptive set theory and set theory of the reals for higher Baire spaces, turning this area of research into one of the hot topics of set theory. Results in this area provide a rich and independent theory that differs significantly from the classical setting and gives new insight into the nature of higher cardinals. The proofs of these results combine concepts and techniques from different areas of set theory: combinatorics, forcing, large cardinals, inner models and classical descriptive set theory. Moreover, they also use methods from other branches of mathematical logic, like model theory and the study of strong logics. In the other direction, these results have been applied to problems in other fields of mathematical logic and pure mathematics, like the classification of non-separable topological spaces, the study of large cardinals and Shelah's classification theory in model theory.

These developments have been strongly supported by regular meetings of the research community. The community met first at the Amsterdam Set Theory Workshop in 2014, then at a satellite workshop to the German mathematics congress in Hamburg in 2015, at a workshop at the Hausdorff Center for Mathematics in Bonn in 2016, and at the KNAW Academy Colloquium in Amsterdam in 2018.

The increased significance of the study of higher Baire spaces has been reflected through these meetings by both strongly growing numbers of attendees and a steadily increasing percentage of participants from other fields of set theory. The Symposium on higher Baire spaces will provide the opportunity to reunite this community a year after the last meeting.

SYMPOSIUM ON JOHN BALDWIN'S MODEL THEORY AND THE PHILOSOPHY OF MATHEMATICAL PRACTICE

Organizer: John Baldwin

Baldwin-1 Session 30A Chair: Juliette Kennedy

Baldwin-2 Session 31A Chair: Juliette Kennedy

Congress section(s): A1, A4, B1, C1

This book serves both as a contribution to the general philosophy of mathematical practice and as a specific case study of one area of mathematics: model theory. It deals with the role of formal methods in mathematics, arguing that introduction of formal logic around the turn of the last century is important, not merely for the foundations of mathematics, but for direct impact in such standard areas of traditional mathematics as number theory, algebraic geometry, and even differential equations. Finding informative axiomatizations of specific areas of mathematics, rather than a foundation which is impervious to the needs of particular areas drives this impact. Some of the many uses of the tools of modern model theory are described for non-specialists.

The book outlines the history of 20th century model theory, stressing a paradigm shift from the study of logic as abstract reasoning to a useful tool for investigating issues in mathematics and the philosophy of mathematics. The book supports the following four theses that elaborate on this shift.

Theses

1. Contemporary model theory makes formalization of specific mathematical areas a powerful tool to investigate both mathematical problems and issues in the philosophy of mathematics (e.g. methodology, axiomatization, purity, categoricity and completeness).
2. Contemporary model theory enables systematic comparison of local formalizations for distinct mathematical areas in order to organize and do mathematics, and to analyze mathematical practice.
3. The choice of vocabulary and logic appropriate to the particular topic are central to the success of a formalization. The technical developments of first order logic have been more important in other areas of modern mathematics than such developments for other logics.
4. The study of geometry is not only the source of the idea of axiomatization and many of the fundamental concepts of model theory, but geometry itself (through the medium of geometric stability theory) plays a fundamental role in analyzing the models of tame theories and solving problems in other areas of mathematics.

The book emphasizes the importance in formalization of the choice of both the basic notions of a topic and the appropriate logic, be it first order, second order, or infinitary logic. Geometry is studied in two ways: the analysis of the formalization of geometry from Euclid to Hilbert to Tarski and by describing the role of combinatorial geometries (matroids) in classifying models. The analysis

of Shelah's method of dividing lines for classifying first order theories provides a new look into the methodology of mathematics. A discussion of the connections between model theory and axiomatic and combinatorial set theory fills out the historical study.

SYMPOSIUM ON THE PHILOSOPHY OF THE HISTORICAL SCIENCES

Organizer: Aviezer Tucker

PHS-1 Session 11A Chair: Giuseppina D'Oro

PHS-2 Session 12A Chair: Aviezer Tucker

Congress section(s): C7

Philosophers have attempted to distinguish the Historical Sciences at least since the Neo-Kantians. The Historical Sciences attempt to infer rigorously descriptions of past events, processes, and their relations from their information preserving effects. Historical sciences infer token common causes or origins: phylogeny and evolutionary biology infer the origins of species from information preserving similarities between species, DNAs and fossils; comparative historical linguistics infers the origins of languages from information preserving aspects of exiting languages and theories about the mutation and preservation of languages in time; archaeology infers the common causes of present material remains; Critical Historiography infers the human past from testimonies from the past and materials remains, and Cosmology infers the origins of the universe. By contrast, the Theoretical Sciences are not interested in any particular token event, but in types of events: Physics is interested in the atom, not in this or that atom at a particular space and time; Biology is interested in the cell, or in types of cells, not in this or that token cell; Economics is interested in modeling recessions, not in this recession; and Generative Linguistics studies "Language" not any particular language that existed in a particular time and was spoken by a particular group of people. The distinctions between realms of nature and academic disciplines may be epistemically and methodologically arbitrary. If from an epistemic and methodological perspectives, historiography, the study of the human past, has more in common with Geology than with the Social Sciences that have more in common with Agronomy than with historiography, we need to redraw the boundaries of philosophies of the special disciplines. This is of course highly controversial and runs counter to attempts to distinguish the historical sciences by the use of narrative explanations, reenactment or emphatic understanding.
The Historical Sciences may be distinguished from the Theoretical Sciences according to their objects of study, tokens vs. types; from Experimental Sciences according to their methodologies, inference from evidence vs. experimenting with it; and from natural sciences according to the realm of nature they occupy. The criteria philosophers proposed for these distinctions were related to larger issues in epistemology: Do the Historical Sciences and offer different kinds of knowledge? Do the Historical and Theoretical sciences support each other's claims for knowledge, and if so, how?; metaphysics and ontology: Do the types of objects the Historical and Theoretical Sciences attempt to study, represent, describe, or explain differ, and if so, how does it affect their methodologies?; and the philosophy of science: What is science and how do the Historical and Theoretical Sciences relate to this ideal?

TEXT-DRIVEN APPROACHES TO THE PHILOSOPHY OF MATHEMATICS

Organizers: Carolin Antos, Deborah Kant and Deniz Sarikaya

TDPhiMa-1 Session 13A Chair: Carolin Antos

TDPhiMa-2 Session 14A Chair: Deniz Sarikaya

TDPhiMa-3 Session 15A Chair: Deborah Kant

TDPhiMa-4 Session 16A Chair: Deborah Kant

Congress section(s): C1

Text is a crucial medium to transfer mathematical ideas, agendas and results among the scientific community and in educational context. This makes the focus on mathematical texts a natural and important part of the philosophical study of mathematics. Moreover, it opens up the possibility to apply a huge corpus of knowledge available from the study of texts in other disciplines to problems in the philosophy of mathematics.

This symposium aims to bring together and build bridges between researchers from different methodological backgrounds to tackle questions concerning the philosophy of mathematics. This includes approaches from philosophical analysis, linguistics (e.g., corpus studies) and literature studies, but also methods from computer science (e.g., big data approaches and natural language processing), artificial intelligence, cognitive sciences and mathematics education. (cf. Fisseni et al. to appear; Giaquinto 2007; Mancosu et al. 2005; Schlimm 2008; Pease et al. 2013).
The right understanding of mathematical texts might also become crucial due to the fast successes in natural language processing on one side and automated theorem proving on the other side. Mathematics as a technical jargon or as natural language, which quite reach structure, and semantic labeling (via LaTeX) is from the other perspective an important test-case for practical and theoretical study of language.
Hereby we understand text in a broad sense, including informal communication, textbooks and research articles.

THE GENDER GAP IN THE SCIENCES AND PHILOSOPHY OF SCIENCE. SYMPOSIUM ORGANISED BY THE DLMPST/IUHPST AND THE GENDER GAP PROJECT

Organizers: Benedikt Loewe and Helena Mihaljevic

Session 3D Chair: Helena Mihaljevic

Congress section(s): B5

The project "A Global Approach to the Gender Gap in Mathematical, Computing, and Natural Sciences: How to Measure It, How to Reduce It?" is an international and interdisciplinary effort to better understand the manifestation of the Gender Gap in the named scientific fields, and to help overcome barriers for women in their education and career. The collaboration between eleven partners including various scientific unions allows for a comprehensive consideration of gender-related effects in these fields, yielding the opportunity to elaborate common grounds as well as discipline-specific differences. Currently, existing data on participation of women in the mathematical and natural sciences is scattered, outdated, and inconsistent across regions and research fields. The project approaches this issue mainly from two different perspectives. Through a survey, scientists and mathematicians worldwide have the opportunity to confidentially share information about their own experiences and views on various aspects of education and work in their disciplines and countries. At the same time, we statistically analyze large data collections on scientific publications in order to understand effects of gender and geographical region on publication and collaboration practices. Moreover, the project aims to provide easy access to materials proven to be useful in encouraging girls and young women to study and pursue education in mathematics and natural sciences.
In this symposium, methods and findings of the Gender Gap project will be presented by Helena Mihaljevic, connected to and contrasted with similar issues in philosophy of science. After three presentations, there will be a panel discussion.

THE HISTORY AND ONTOLOGY OF CHEMISTRY. SYMPOSIUM OF THE JOINT COMMISSION OF THE IUHPST 4

Organizer: Hasok Chang

JC4-1 Session 5A Chair: Hasok Chang

JC4-2 Session 6A Chair: Hasok Chang

Congress section(s): B1

In this proposed symposium we make a historical–philosophical examination of chemical ontology. Philosophers thinking about the metaphysics of science would do well to scrutinize the history of the concepts involved carefully. The idea of "cutting nature at its joints" does not offer much practical help to the scientists, who have to seek and craft the taxonomic and ontological notions according to the usual messy procedures of scientific investigation. And we philosophers of science need to understand the nature of such procedures. In this session we showcase various attempts to do such historical–philosophical work, with a focus on chemistry.
Robin Hendry will provide a general framing of the issue. The International Union of Pure and Applied Chemistry (IUPAC) has developed different systems of nomenclature for inorganic and organic substances. These systems reflect both

chemistry's historical development and particular metaphysical views about the reality of chemical substances. Looking back into the history, we recognize the contingent decisions taken by past chemists that led to our present conceptions, and the possible paths-not-taken that might have led to different ontological conceptions. Such decisions were, and will continue to be, influenced by various types of forces that shape science. If the history of chemistry is a garden of forking paths, then so is the metaphysics of chemistry.

This presentation will be followed by three concrete studies. Marina Paola Banchetti-Robino will discuss the shift from vitalism to mechanicism that took place in early modern investigations of matter. This was a gradual and complex process, with corpuscularianism as an important commonality shared by the competing perspectives. She argues that aspects of vitalism and mechanicism co-existed in interesting ways in the chemical ontology of the early modern period, and that the gradual demise of vitalism resulted not from reductionism but from a physicalistic and naturalistic rationalization of chemical qualities.

Sarah Hijmans will address the history of the concept of chemical element. She starts by noting that there are two IUPAC definitions that loosely correspond to Lavoisier's operational concept and Mendedeev's more metaphysical concept. Little has been said about the evolution of the concept of element between the times of these two great chemists. She argues that the change in the conception of the element was part of a broader evolution of chemical practice. A view very similar to Mendeleev's was already present in early 19th-century chemical atomism, and developed in a rather continuous way through the century.

Karoliina Pulkkinen will examine the history of the late 19th-century attempts to find periodic regularities among the chemical elements. While Meyer saw it likely that all elements were comprised of the same primordial matter, Mendeleev saw each element as a distinct, individual, autonomous entity and refrained from making representations of periodicity that suggested otherwise. Following Andrea Woody's discussion of the law of periodicity as a theoretical practice, this paper explores how Meyer's and Mendeleev's ontological views on primordial matter shaped their ideas on how to represent periodicity.

THEORIES AND FORMALIZATION. SYMPOSIUM OF THE ITALIAN SOCIETY FOR LOGIC AND PHILOSOPHY OF SCIENCE

Organizers: Giovanni Valente and Roberto Giuntini

SILFS-1 Session 19F Chair: Gustavo Cevolani

SILFS-2 Session 20M Chair: Gustavo Cevolani

SILFS (Società Italiana di Logica e Filosofia della Scienza) is the Italian national organization devoted to fostering research and teaching in the fields of logic, general philosophy of science and philosophy of the special sciences. It comprises a large number of academics working in such areas, who are based in Italy as well as in other countries. This symposium proposes to explore philosophical and methodological issues concerning the foundations of our best scientific theories, with the aim of bridging across the diverse research trends characterizing the Italian community of logicians and philosophers of science. Specifically, the symposium focuses on the formal status of successful theories developed in various fields of science, most notably the life-sciences, the mathematical sciences and the social sciences. For this purpose, it brings together experts on the logic and philosophy of medicine, physics, computation and socio-economics, so as to jointly investigate from different perspectives a host of inter-connected questions that arise when facing the outstanding problem of how to formalize scientific theories.

More to the point, we plan to deal with the following issues: (1) how to provide a formal treatment of empirical evidence in medical research; (2) how to elaborate a computational notion of trust that can be applied to socio-economical contexts; (3) how to construct a rigorous framework for the logic of physical theories, with particular focus on the transition from classical to quantum mechanics; (4) how to develop a mathematical foundation for the concept of reduction between different theoretical systems. By addressing such specific questions with a systematic and inter-disciplinary approach, the symposium wishes to advance our general understanding of the relation between theories and formalization.

TOWARD THE RECONSTRUCTION OF LINKAGE BETWEEN BAYESIAN PHILOSOPHY AND STATISTICS

Organizer: Masahiro Matsuo

TRLBPS-1 Session 30F Chair: Masahiro Matsuo

TRLBPS-2 Session 31F Chair: Masahiro Matsuo

Congress section(s): C1

In philosophy of science, Bayesianism has long been tied to subjective interpretation of probability, or probability as a degree of belief. Although several attempts have been made to construct an objective kind of Bayesianism, most of the core issues and controversies concerning Bayesianism have been biased to this subjectivity, particularly to the subjective priors. Along this line of argument, philosophers currently seem to implicitly assume that Bayesian statistics, which is increasingly getting popular in many fields of science, can be treated legitimately as a branch of subjective Bayesianism.

Despite this comprehensive view, which could be partly traced back to the interpretation of Savage's 'Likelihood Principle', how subjectivity is involved in Bayesian statistics is not so obvious. On the contrary, scientists who use Bayesian statistics are inclined to think of it rather as based on an objective methodology, or else merely as a mathematical technique, without even knowing much of arguments of philosophical Bayesianism. These suggest that there is a considerable gap between typically discussed Bayesianism in philosophy and Bayesian statistical method used in science. The problem is no longer simply the distinction about subjective or objective but more importantly, the present situation where this linkage is almost neglected by both philosophers and statisticians despite the common use of the term "Bayesian". Bayesian philosophy without statistics and Bayesian statistics without philosophy are both epistemically unsound, and undoubtedly philosophers of science should have responsibility for the restoration of this linkage.

In this symposium, we present some perspectives which could presumably help this restoration. Although an approach trying to examie the history of Bayesianism minutely is certainly necessary in some part of the analysis to achieve this goal, there must be a risk of losing our way if we focus too much attention on this, because the history of it, particularly of the rise of Bayesian statistics, is tremendously complicated to unravel. In order to grasp appropriately the relation between current Bayesian philosophy and statistics, it seems a more plausible way to start from the current situation we are placed in and to investigate it from multiple philosophical and statistical perspectives available, with some help of historical ones when in need. This is the basic strategy we have in this symposium. Accordingly, our focus is not just upon restoration, but rather on (in a positive sense) reconstruction of the linkage between the two Bayesian camps. The perspectives we present are: a parallelism found between Bayesianism and inductive logic; a complementary relation between Bayesian philosophy and statistics; a solution to the conflict between Bayesian philosophy and frequentism through Bayesian statistics; and a linkage between Bayesian philosophy and statistics through statistical theories based on both Bayesianism and frequentism. In this symposium, we have time to discuss after each speaker's presentation.

WHAT IS THE VALUE OF HISTORY OF SCIENCE FOR PHILOSOPHY OF SCIENCE? JOINT COMMISSION OF THE IUHPST 1

Organizer: Hasok Chang

JC1 Session 10A Chair: Hasok Chang

Congress section(s): B1

This special session is devoted to the presentation of the 2019 IUHPST Essay Prize in History and Philosophy of Science. The prize question for this round of competition was: "What is the value of history of science for philosophy of science?" This question was intended as a counterpart to the question for the inaugural run of the prize in 2017, which asked about the value of philosophy of science for history of science. The session will include the presentation of the prize, a lecture by the prize-winners (60 minutes), by the author of the runner-up essay, and a period of discussion with members of the audience.

This session is offered as part of the set of symposia organized by the Joint Commission, which serves as a link between the historical and the philosophical Divisions of the IUHPST.

WHAT METHOD FOR CONCEPTUAL ENGINEERING

Organizer: Manuel Gustavo Isaac

MET4CE-1 Session 17C Chair: Manuel Gustavo Isaac

MET4CE-2 Session 18C Chair: Manuel Gustavo Isaac

MET4CE-3 Session 19C Chair: Manuel Gustavo Isaac

MET4CE-4 Session 20C Chair: Manuel Gustavo Isaac

Congress section(s): B1

Conceptual engineering is a fast-moving research program in the field of philosophical methodology. Considering concepts as cognitive devices that we use in our cognitive activities, it basically assumes that the quality of our conceptual apparatuses crucially determines the quality of our corresponding cognitive activities. On these grounds, conceptual engineering adopts a normative standpoint that means to prescribe which concepts we should have, instead of describing the concepts we do have as a matter of fact. And its ultimate goal as a research program is thus to develop a method to assess and improve the quality of any of our concepts working as such cognitive devices—that is, for the identification of improvable conceptual features (e.g. conceptual deficiencies) and the elaboration of correlated ameliorative strategies (e.g. for fixing the identified conceptual deficiencies). Given the ubiquity of deficient and improvable concepts, the potential outreach of conceptual engineering is arguably unlimited. But conceptual engineering is still a very young research program and little has been said so far as to how its method should be devised. The purpose of the MET4CE Symposium is to contribute to filling this theoretical gap. Its main aim will then be to propose critical reflections on the very possibility—whether and why (or why not)? how? to what extent?—of developing an adaptable set of step-by-step instructions for the cognitive optimization of our conceptual apparatuses. With this in mind, the common background of the symposium will be made of the Carnapian method of explication rebooted as an ameliorative project for (re-)engineering concepts. Against this background, a first objective of the symposium will be to present ways to procedurally recast Carnapian explication with complementary frameworks (e.g. via reflective equilibrium, metrological naturalism, formalization, or conceptual modeling) for the purposes of conceptual engineering. A second objective will next be to present ways to extend the scope Carnapian explication as a template method with alternative frameworks (e.g. via conceptual history/genealogy, experimental philosophy, or constructionism in philosophy of information), again, for the purposes of conceptual engineering. And finally, a third objective of the symposium will be to evaluate these upgraded methodological frameworks for (re-)engineering concepts by comparison with competing theories of conceptual engineering that reject the very possibility of developing any template procedural methods for (re-)engineering concepts (such as Cappelen's 'Austerity framework'). The expected outcome of the MET4CE Symposium is thereby to provide conceptual engineering with proven guidelines for making it an actionable program for the cognitive optimization of our conceptual apparatuses.

CLMPST 2019

BOOK OF ABSTRACTS

TALKS

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VIRTUES, ARGUMENTS, AND MATHEMATICAL PRACTICE

Session 5C

Congress section(s): C1

Several authors have proposed argumentation theory as a methodology for the study of mathematical practice [2]. Formal logic serves the traditional purposes of philosophy of mathematics very well. However, the philosophy of mathematical practice is concerned not just with formal derivation but with the social processes whereby mathematicians gain assent for their conjectures. Since formal logic is less well-adapted to the analysis of arguments in this wider sense, it is natural to look beyond it to argumentation theory, a discipline concerned with the analysis of natural language argument.
Several authors have proposed virtue theory as an approach to argumentation theory [1]. Virtue theories of argument shift the focus away from arguments as abstractions onto the interpersonal nature of argumentation, stressing the importance of arguers, respondents, and audiences, and especially the character of these participants.
Despite some overlap amongst their advocates, these two trends have never been addressed together. In doing so, it is natural to ask if their conjunction entails a virtue theoretic approach to mathematical practice: must the virtue theorist of argument also be a virtue theorist of mathematical practice? A negative answer to this question is not impossible. It could be held that those aspects of mathematical practice that lend themselves best to analysis in terms of argument do not correspond to features of argumentation theory where a virtue approach is of most value. In particular, some virtue theorists of argument deny that theirs is an all-embracing account, insisting that some issues, notably the appraisal of arguments, must be handed over to another theory [3].
Nonetheless, this paper defends a virtue argumentation theory of mathematical practice. It does so on two grounds. Firstly, there are significant but neglected areas of both argumentation theory and the study of mathematical practice where a shared virtue approach is potentially salutary. For example, conventional approaches in each discipline pay little attention to the contribution the respective practice makes to human flourishing [4]. Secondly, mathematical practice is potentially a valuable testbed for the ambitious varieties of virtue argumentation theory. Virtue accounts have already been proposed for aspects of mathematical practice corresponding to argument appraisal, such as the social acceptance of proofs [5]. The success of such accounts would suggest that virtue approaches can be of comparable utility within argumentation in general.
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DYNAMICS AND CHRONOGEOMETRY IN SPACETIME THEORIES

Session 29L

Congress section(s): C2

A recent debate on the foundations of special relativity (SR) concerns the direction of an alleged arrow of explanation between Minkowski structure and Lorentz covariant dynamics. Harvey Brown (2005) argues that the latter explains the former, whereas Michel Janssen (2009) argues that the arrow points in the opposite direction.
I propose a different view concerning the relation between dynamics and spacetime structure, drawing a lesson from Helmholtz's (1977) work on the epistemology of geometry. Helmholtz's insight was that for the question of the geometric structure of physical space to make sense at all, dynamical considerations must be involved from the outset. If the notions of congruence and rigidity are not previously defined and operationalized in terms of dynamical principles, the measurements that can tell about the geometric structure of physical space are neither defined nor possible. Geometric structure cannot refer to the physical world unless dynamical principles define congruence and rigidity. The converse is also true: dynamics

makes definite sense only on a geometric structure background. This is why measurements with rigid bodies constitute empirical evidence for a certain geometric structure in the first place. If the dynamics of measuring rods were geometrically neutral, measurements would be idle with respect to the geometric structure of physical space.

I illustrate this point comparing SR and Lorentz's ether theory. The mathematical form of the dynamical laws in both theories is the same, but they have a different meaning. In Lorentz's theory, $\Delta x' = \Delta x/\gamma$ refers to the longitudinal contraction of an object that moves with respect to the ether with velocity v. In SR the formula refers to different measurements of the length of the same object in two frames in relative motion. This difference is grounded in that $\Delta x' = \Delta x/\gamma$ is setup on different chronogeometric structures. For the ether theory to be able to pick a privileged ether-rest frame, Galilean spacetime must be the chronogeometric background. On the other hand, in SR the formula is about kinematics in different frames since the background chronogeometric structure is Minkowski spacetime. If the law were chronogeometrically neutral we could not assign it any of the two meanings—or any physical meaning at all.

In conclusion, for a chronogeometric structure to have a physical meaning, dynamical principles that operationalize it are necessary, and if dynamical laws are to have a definite physical meaning, they must be setup on a chronogeometric structure background. Thus, the connection between them cannot be explanatory, and then the debate mentioned above gets dissolved. This thesis is a development of the argument in (Acuña 2016): there it is argued that in SR Minkowski spacetime and Lorentz covariance are inextricably connected. Here I argue that the same relation holds between spacetime structure and dynamics in spacetime theories in general.

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COLLINGWOOD, THE NARRATIVE TURN, AND THE COOKIE CUTTER CONCEPTION OF HISTORICAL KNOWLEDGE

Session 12A

Congress section(s): B1, C7

The narrative turn in the philosophy of historiography relies on a constructivist epistemology motivated by the rejection of the view that there is any such thing as immediate knowledge of the past. As there is no such thing as knowledge of things as they are in themselves generally speaking, so there is no knowledge of the past in itself. Some narrativists characterise the temporal distance between the agents and the historian in positive terms and present it as an enabling condition of historical knowledge, because, so they argue, the significance of an historical event is better grasped retrospectively, in the light of the chain of events it set in motion. Others, on the other hand, see the retrospective nature of historical knowing as a sort of distorting mirror which reflects the historian's own zeitgeist. Historical knowledge, so the argument goes, requires conceptual mediation, but since the mediating concepts are those of the historian, each generation of historians necessarily re-writes the past from their own perspective, and there can never be anything as "the past as it always was" (Dray). To use a rather old analogy one might say that as the form of the cookie cutter changes, so does the shape of the cookie cut out of the dough.

This paper argues that there is a better way of preserving the central narrativist claim that the past cannot be known in-itself, one which does not require biting the bullet that the past needs to be continuously re-written from the standpoint of the present. To do so one needs to rethink the notion of mediacy in historical knowledge. We present this alternative conception of mediacy through an explication and reconstruction of Collingwood's philosophy of history. According to Collingwood the past is known historically when it is known through the eyes of historical agents, as mediated by their own zeitgeist. The past is therefore not an ever-changing projection from different future "nows". While human self-understanding changes over time (the norms which govern how a medieval serf should relate to his lord are not the same as those which govern the relation between landlord and tenant in contemporary London) the norms which governed the Greek, the Romans, the Egyptian or the Mesopotamian civilization remain what they always were. It is the task of the historian to understand events as they would have been perceived by the historical agents, not in the light of legal, epistemic or moral norms that are alien to them. Fr example, understanding Caesar's crossing of the Rubicon as challenging the authority of the senate (rather than, say, simply talking a walk with horses and men) involves understanding the Roman legal system and what Republican law entailed. This is a kind of conceptual knowledge that is not altered either by the future course of events or by changes in human self-understanding. Although Collingwood's account of the nature of mediacy in historical knowledge would disallow that later historians should/could retrospectively change the self-understanding of the Romans (or the Egyptians, or the Greeks), the claim that historians can know the past as the Egyptians, the Romans or the Mesopotamians did, is not tantamount to claiming that the past can be known in itself. It is rather the assertion that the past is known historically when it is known through the eyes of the historical agent, not those of the historian. This conception of mediacy takes the past to be always-already mediated (by the conceptual framework of the agent) and, unlike the cookie-cutter conception of knowledge, does not lead to the sceptical implications which go hand in hand with the narrativist conception of mediacy.

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STRUCTURAL MODALITY AS THE CRITERION FOR NATURALISTIC INVOLVEMENT IN SCIENTIFIC METAPHYSICS

Session 20H

Congress section(s): B4

Ladyman & Berenstain 2012 argue that Ontic Structural Realism (OSR) attributes rich natural necessities to the world whereas Epistemic Structural Realism (ESR) developed by Worrall 1989 and Constructive Empiricism do not. Although it is undoubtedly clear that the world is intrinsically de-modalized according to Constructive Empiricism, I think there is a place for natural necessities in ESR since the negative argument about our epistemic access to the basic furniture of the world and attributing structural natural necessities to it can be held at the same time. The reason for that is an argument due to Stanford (Stanford et al. 2010). He has shown that modality claims in OSR do not say anything more than endorsing No Miracle Argument (NMA). Since ESR, just like OSR, endorses NMA, modality can similarly be attributed to structures in ESR; hence modality cannot be introduced as a distinctive feature for OSR. After showing that introducing modality supports OSR and ESR equally, I will try to understand the significance of natural necessities in a plausible version of naturalized metaphysics that would be suitable for structuralism. I will argue, following Wilson (forthcoming), that modality is the key component of naturalistic involvement. Since natural necessities have the same strength in OSR and ESR, I will conclude that naturalized metaphysics developed by Ladyman & Ross could easily be adapted for ESR. Although I endorse Wilson's argument that modality should be taken as the criteria for naturalisticness, I reject his claim that his position provides the strongest version of scientific modality since it reconciles Lewisian modal realism with Everettian quantum mechanics.

For, post-positivist concept analysis driven analytic metaphysics mainly populated with Lewis is the primary target for most scientific metaphysicians today; hence combining modal realism with current fundamental physics would not strike most of the naturalized metaphysicians as a successful reconciliation. I conclude by arguing that an account for structural modalities, including structural necessities in ESR, is the most convenient locus for a plausible version of naturalized metaphysics.

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ON THE DEFINITIONS OF SOCIAL SCIENCE AND WHY THEY MATTER

Session 15E

Congress section(s): C7

What sort of category is ‘social science’? Is it theoretical, that is, reflecting a genuine specialness of social sciences’ subject matter or method? Or merely institutional, that is, denoting the activities and the body of knowledge of those recognised as practicing economics, sociology, anthropology, etc.? The field of philosophy of social science has traditionally assumed the former and sought to articulate ways in which social sciences are unlike the natural ones. I trace the history and the motivations behind this exceptionalism and evaluate its viability in this age of interdisciplinarity and data-driven methods.

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LIABILITY WITHOUT CONSCIOUSNESS? THE CASE OF A ROBOT

Session 14F

Congress section(s): C8, C9

It is well known that the law punishes those who caused harm to someone else. However, the criteria for punishment becomes complicated when applied to non-human agents. When talking about non-human agency we primarily have in mind robot agents. Robot agency could be reasonably defended in terms of liability, the mental state of being liable. The roots of the problem should be looked for when defining robots’ ability to have mental states but even when we put this particular problem aside the question of liability seems to be of a crucial value while discussing a harm-causing technology.
Since the question of liability requires special attention to the domain of mental states, we argue that it is crucial for the legal domain to define the legal personhood of a robot. We should try to answer the question – what constitutes a legal person in terms of non-human agency? If legal personhood is the ability to have legal rights and obligations, how can we ascribe these human qualities to a non-human agent? Are computing paradigms able to limit robots’ ability to cause harm? If so, can legal personhood still be ascribed (having in mind that computing could be limiting the free will)?
These questions are of the highest importance when thinking about if we should punish a robot, and how this punishment could function having in mind the non-human personhood.

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A PLURALITY OF METHODS IN THE PHILOSOPHY OF SCIENCE: HOW IS THAT POSSIBLE?

Session 14E

Congress section(s): B1

In this talk, the place of logical and computational methods in the philosophy of science shall be reviewed in connection to the emergence of the cognitive sciences. While the interaction of several disciplines was breeding ground for diverse methodologies, the challenge to methodologists of science to provide a general framework, still remains.
As is well-known, the distinction between the context of discovery and the context of justification, which served as a basis for logical positivism, left out of its research agenda –specially from a formal perspective-- a very important part of scientific practice, that which includes issues related to the generation of new theories and scientific explanations, concept formation as well as other aspects of discovery in science.
Some time later, in the seventies of last century, cognitive scientists revived some of the forgotten questions related to discovery in science within research topics such as mental patterns of discovery and via computational models, like those found for hypothesis formation. This helped to bring scientific discovery to the fore as a central problem in the philosophy of science. Further developments in computer science, cognitive science and logic itself, provided a new set of tools of a logical and computational nature. The rather limited logic used by the Vienna Circle was now augmented by the discovery of quite new systems of logic, giving place to a research context in which computer science, logic and philosophy of science interacted, each of them providing its own methodological tools to the service of philosophy of science.
A further interaction arised in the eighties of last century, in this case between logic and history, giving place to computational philosophy of science, a space in which history and computing met on a par. This created the possibility of a partial synthesis between the logical and the historical approaches with a new computational element added to the mix.
The present setting, in which we have all logical, historical and computational approaches to philosophy of science, fosters the view that what we need is a balanced philosophy of science, one in which we take advantage of a variety of methodologies, all together giving a broad view of science. However, it is not at all clear how is that such plurality of methods can be successfully integrated.

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CONCEPTUAL ENGINEERING IN THE PHILOSOPHY OF INFORMATION

Session 19C

Congress section(s): B1

Conceptual engineering is not only a growing topic of methodological interest in contemporary, primarily analytically oriented, philosophy. It also occupies a central place within the philosophy of information (Floridi 2011b). Yet, despite the agreement that conceptual engineering is the philosophical task (Floridi 2011a, Cappelen 2018) par excellence, we have two intellectual endeavours that have developed independently of each other, have shown little interest in each other, and whose interdependencies remain unclear. For the sake of terminological clarity, I will reserve the term conceptual engineering to refer to the project of Cappelen and others (the primary project targeted in this symposium), while I will use constructionism to refer to the importance that is accorded to making / engineering / designing in the philosophy of information.
My goal in this paper is to clarify how constructionism relates to conceptual engineering; both as a means to situate the philosophy of information vis-à-vis the mainstream debate and identify the defining differences, and as a means to identify fruitful convergences and opportunities for mutual influence.
As a starting point, I would like to present the constructionism within the philosophy of information as a convergence of three conceptual shifts:
1. A focus on a maker’s conception of knowledge as an alternative to the more traditional focus on user’s conceptions of knowledge (Floridi 2018). The key idea here is the view that we only know what we make. Here, constructionism is an epistemological thesis about what we can know and about the kind of knowledge we ought to pursue.
2. An account of philosophical questions as open questions whose resolution require the development of new conceptual resources (Floridi 2013). Here, constructionism is first and foremost a meta-philosophical thesis that addresses the question which kind of inquiries philosophers should engage in.
3. A view about the nature of and our responsibilities towards the infosphere and the project involved in “construction, conceptualization, semanticization, and finally the moral stewardship of reality” (Floridi 2011b: 23); especially in mature information-societies (Floridi 2014). Here, constructionism becomes a ethical and political thesis.
Once this stage is set, we can begin to identify a number of notable divergences between the project of conceptual engineering and constructionism. Here, I propose to focus on only three such divergences.
First, constructionism is best described as a pluralist project; not as a meliorative or revisionist project.
Second, constructionism is best understood relative to a restricted domain. Its focus is on the conceptual resources we need for specific purposes; tasks or questions that are always relative to a purpose, context, and level of abstraction. Global

changes of our language are not an immediate concern; even if specific goals may often require the development of new terminologies or demand the re-purposing of existing terms for novel uses.
Third, constructionism does not engage in the design of concepts for representational purposes.
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ON THE SIGNIFICANCE OF ARGUMENTATION IN DISCOVERY PROOF-EVENTS

Session 27E

Congress section(s): B2

Many researchers claim that the role of argumentation is central in mathematics. Mathematicians do much more, than simply prove theorems. Most of their proving activity might be understood as kinds of argumentation. Lakatos' Proofs and Refutations is an enduring classic that highlights the role of dialogue between agents (a teacher and some students) by attempts at proofs and critiques of these attempts. The comparison between argumentation supporting an assumption or a purported proof and its proof is based on the case that proof can be regarded as a specific argumentation in mathematics. Thus, argumentation theory can be used to explore certain aspects of development of discovery proof-events in time. The concept of proof-event was introduced by Joseph Goguen, [2001], who understood mathematical proof, not as a purely syntactic object, but as a social event, that takes place in specific place and time and involves agents or communities of agents. Proof-events are sufficiently general concepts that can be used to study besides the "traditional" formal proofs, or other proving activities, such as incomplete proofs, purported proofs or attempts to verify a conjecture.
Since argumentation is inseparable from the process of searching for a mathematical proof, we suggest a modified model of the proof-events calculus [Stefaneas and Vandoulakis 2015] that was used to represent discovery proof-events and their sequences, based on the versions of argumentation theories advanced by Pollock [1992], Toulmin [1993] and Kakas and Loizos [2016].
We claim that the exchange of arguments and counterarguments aimed at clarifying possible gaps or implicit assumptions that occur during a proof, can be formally represented within this integrated framework. We illustrate our approach on the historical case of the sequence of proof-events leading to the proof of Fermat's Last Theorem.
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SPLICING LOGICS: HOW TO COMBINE HYBRID AND EPISTEMIC LOGIC TO FORMALIZE HUMAN REASONING

Session 30J

Congress section(s): A2, A3

We advocate in this paper for splicing Hybrid and Epistemic Logic to properly model human reasoning.
Suppose Wittgenstein wants to meet Russell to discuss some (possibly weird) philosophical issues at 2 a.m. of April 26th. Wittgenstein knows p ("we meet to discuss at 2 a.m. of April 26th") (symbolically: Kwp, where K is the knowledge operator) at the morning of April 25th, whereas Russell does not: ¬Krp. And at the same time Wittgenstein knows that Russell does not know it: Kw¬Krp.
At the afternoon of April 25th Wittgenstein types Russell to communicate him p. But Russell does not reply his message. Thus, at that time we have Kwp ∧ ¬KwKrp ∧ ¬Kw¬Krp ∧ KwKr¬KwKrp, for Wittgenstein does not know whether Russell has read his message but knows that, if he has indeed read it, Russell knows Wittgenstein does not know it.
Being considerate, Russell would resolve this situation by answering Wittgenstein´s message. Let us suppose that he does it at night. Then at that moment we have Krp, but it cannot be assumed that KrKwKrp for Wittgenstein could not have read the reply, so ¬KrKwKrp ∧ ¬KrKwKrKwp. Tired of so much ambiguity, Russell phones Wittgenstein to set up the meeting, and then Kwp ∧ Krp ∧ KwKrp ∧ KrKwp ∧...
By means of Epistemic Logic we have been able to formalize both Wittgenstein and Russell´s knowledge and their communication. And if we spliced it with Temporal Logic (see [2] and [4]), we could reflect how their knowledge change over time.
Nevertheless, even splicing that two systems it is not possible to properly model the whole situation. Neither of them are able to name points (inside a model). They cannot formalize and evaluate, for instance, that a formula such as Kwp is true at exactly the morning of April 25th, i.e., at the point which stands for that moment. So actually, just by means of Temporal-Epistemic Logic none of the formulae which come into play in Wittgenstein-Russell communication can be interpreted. To accomplish it it is necessary to turn to Hybrid Logic.
This paper aims at introducing a Hybrid-Epistemic Logic (resulting from splicing both logical systems) in order to improve Temporal-Epistemic Logic expressivity. A proper semantic will be provided, and it will be showed that the only way of accurately modeling how we talk about our knowledge and beliefs, and how they change over time, is via Hybrid-Epistemic Logic.
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EXPLANATORY CONDITIONALS

Session 13G

Congress section(s): B2

The present paper aims to complement causal model approaches to causal explanation by Woodward (2003), Halpern and Pearl (2005), and Strevens (2004). It does so by carrying on a conditional analysis of the word 'because' in natural language by Andreas and Günther (2018). This analysis centres on a strengthened Ramsey Test of conditionals:

α ≫ γ iff, after suspending judgment about α and γ, an agent can infer γ from the supposition of α (in the context of further beliefs in the background).
Using this conditional, we can give a logical analysis of because:
Because α,γ (relative to K) iff α≫γ ∈ K and α,γ ∈ K
where K designates the belief set of the agent. In what follows, we shall refine this analysis by further conditions so as to yield a fully-fledged analysis of (deterministic) causal explanations. The logical foundations of the belief changes that define the conditional ≫ are explicated using AGM-style belief revision theory.
Why do we think that causal model approaches to causal explanation are incomplete? Halpern and Pearl (2005) have devised a precise semantics of causal models that centres on structural equations. Such an equation represents causal dependencies between variables in a causal model. In the corresponding definition of causation, however, there is no explanation of what it is for a variable to causally depend directly on certain other variables. This approach merely defines complex causal relations in terms of elementary causal dependencies, just as truth-conditional semantics defines the semantic values of complex sentences in terms of a truth-value assignment to the atomic formulas. And the corresponding account of causal explanation in Halpern and Pearl (2005) inherits the reliance on elementary causal dependencies (which are assumed to be antecedently given) from the analysis of causation. Woodward (2003) explains the notion of a direct cause in terms of interventions, but the notion of an intervention is always relative to a causal graph so that some knowledge about elementary causal dependencies must be antecedently given as well.
The kairetic account of explanation by Strevens (2004) makes essential use of causal models as well, but works with a more liberal notion of such a model. In his account, a set of propositions entail an explanandum E in a causal model only if this entailment corresponds to a "real causal process by which E is causally produced" (2004, p. 165). But the kairetic account is conceptually incomplete in a manner akin to the approaches by Halpern and Pearl (2005) and Woodward (2003). For, it leaves open what the distinctive properties of causal relations of logical entailment are. In what follows, we aim to give a precise characterization of logical entailment with a causal meaning. For this characterization, we define an explanatory conditional ≫, but impose also non-logical conditions on the explanans and the explanandum. Here is a semiformal exposition of our final analysis:
Definition 1. Causal Explanation. Let S be an epistemic state that is represented by a prioritised belief base. K(S) is the set of beliefs of S, extended by the strengthened Ramsey Test. The set A of antecedent conditions and the set G of generalisations explain the fact F - for an epistemic state S - iff
(E1) For all α ∈ A, all γ ∈ G, and all β ∈ F: α,γ,β ∈ K(S).
(E2) For all non-empty A′ ⊆ A, A′≫F ∈ K(S).
(E3) For any α ∈ A and any β ∈ F, (i) the event designated by α temporally precedes the event designated by β, or (ii) the concepts of α are higher up in the hierarchy of theoreticity of S than the concepts of β.
(E4) For any γ ∈ G, γ is non-redundant in the set of all generalisations of S.

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LOGICAL APPROACHES TO VAGUENESS AND SORITES PARADOXES

Session 7M

Congress section(s): A2

In this talk, I will regard some logical approaches to the problem of vagueness and respectively to sorites paradoxes and will analyse their advantages and disadvantages. In this connection, I will outline the main characteristics of subvaluationism, supervaluationism, fuzzy logic, relevant logic and fuzzy relevant logic and will point out which of them, according to me, is the most appropriate for the clarification of the above problem and for solving the sorites paradoxes.
The analysis will begin with consideration of some paraconsistent and paracomplete approaches as subvaluationism and supervaluationism. I will stress on their inadequacy concerning the discrepancy between the preliminary considerations on the problem of vagueness, which these logics attempt to solve and the desideratum to retain most aspects of classical semantics, which in their case do not make a good job. From first sight, it seems that subvaluationism and supervaluationism would propose good solutions for vague cases since they deny the principle of bivalence and assess the vague propositions as having the both truth values (true and false) for subvaluationism and, respectively, as a lack of truth values for supervaluationism. However, although the failure of bivalence these logics retain the law of non-contradiction and, respectively, the law of excluded middle; the conjunction and respectively disjunction are not truth-functional - they are sometimes truth-functional and sometimes they are not. For these reasons, as Hyde shows [1, p.73-95], subvaluationism is only weakly paraconsistent and supervaluationism is only weakly paracomplete. To me these two logics have used extra-logical prerequisites without giving a clear mechanism how to assess the formulas in the different cases and they propose informal arguments to solve a formal issue (the sorites paradox) without clear principles and rules. I will give arguments for this thesis.
After that the focus of my presentation will be directed to the intuitions accommodated by fuzzy logic, relevant logic (that is strongly paraconsistent and paracomplete) and Priest's fuzzy relevant logic [4] as well as to their advantages and shortcomings.
As a whole, I will stress on the virtues of the last two logics and will regard which interpretations of their terms are sufficient to accommodate vague predicates and propositions.
I will also point out the inadequacy of the two forms of sorites paradoxes: as a sequence, using modus ponens and as mathematical induction. It is mainly due to the usage of a mixture of different sort of notions, different kind of dependences and different levels, which I will try to analyze.
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SECOND ORDER PROPERTIES OF COPIED COMPUTATIONAL ARTEFACTS

Session 29A

Congress section(s): A2, A3, C6, C8

This paper provides a logical analysis of second order properties of computational artefacts preserved under copies as defined in (Angius and Primiero 2018). Properties like safety or liveness assume special significance in computer security and for the notion of malfunctioning. While definition and model checking of these properties are extensively investigated (Kupferman and Vardi 2001, Padon et al. 2018), the study of their preservation under copies is less considered in the literature. Another context of application is in the computer ethics debate on software property rights (Johnson 1998), translated to the question of which formal characteristics are or should be preserved by copies.
Copies for computational artefacts are defined in (Angius and Primiero 2018) as set-theoretic relations holding between abstract machines x and y. For exact copies, behaviours prescribed by y are all and only the behaviours prescribed by x; for inexact copies, the behaviours of y are all, but not only, the behaviours of x; for approximate copies, the behaviours of y are some, but not only, the behaviours of x. Bisimulation and simulation are used to provide formal definitions of the three copy relations (Fokkink 2013). In this paper, we introduce CTL* temporal logic (Kröger and Merz 2008) for formulas of the form EX, EG, and EU (respectively: existential next, existential global and existential until) to which any other CTL* formula can be reduced using equivalence rules. We then analyse whether EX, EG, and EU formulas are preserved by exact, inexact, and approximate copies. We prove that any second order property is preserved by exact copies, since bisimulation implies CTL* equivalence. EX, EG, and EU formulas in positive form are satisfied by inexact copies, including liveness EU, provided that they admit infinite paths. They can or cannot satisfy any negation thereof, including safety ¬E¬U, so the copying machines need to be model checked against those formulas to determine whether they satisfy properties of interest of the copied machines. If y is an approximate copy of x, we prove that a definable subset of the behaviours prescribed by y preserves EX, EG, and EU properties expressed in negative form, provided that x and y only allow for finite paths. And a definable subset

of EX, EG, and EU formulas in positive form satisfied by x is also satisfied by y, provided that both machines admit infinite paths.

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ORGANISMS AS SITUATED MODELS

Session 17D

Congress section(s): C3

The philosophical literature now abounds on the use of organisms as model organisms (for an early contribution, see Ankeny & Leonelli 2011), and draws heavily on historical and sociological work which tends to focus on the standardisation of organisms as a critical stage in related research processes. This paper builds on this literature while taking up a new philosophical angle, namely that the environment, experimental set-ups, and other conditions in which the organism is situated are critical to its use as a model organism (for an early discussion of this approach in historical context, see Ankeny et al. 2014; cf. Nelson 2017 on scaffolds). In such cases, the organism itself is only one component in a more complex model. Hence we explore how material systems can ground inferences, extrapolations, and representations made using these organisms, using a series of examples based on recent historical cases including the use of cages and other housing systems with rodents and farm animals (Ramsden 2009; Kirk & Ramsden 2018), and various types of field experiments and stations (drawing on Kohler 2002; de Bont 2015; Raby 2015 among other historical work).

We argue that this type of situatedness is a critical part of what constitutes the repertoires connected with model organisms and other uses of experimental organisms (Ankeny & Leonelli 2016), and that the materiality of this situatedness is an essential feature which shapes the way in which such organisms are used in scientific research practices. In addition, this analysis assists us in making sense of the variety of modelling activities associated with the use of organisms in laboratories and elsewhere across various fields within biology (e.g., Meunier 2012; Love & Travisano 2013; Germain 2014; Gross & Green 2017 among many others), while at the same time clarifying why organisms themselves have such an important 'anchoring' role for these practices (see especially Rheinberger 2006).

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COUNTERFACTUALS AND REASONING ABOUT ACTION

Session 27L

Congress section(s): A2

A rational agent creates counterfactual alternatives when they act on actions concerning "if..then", "what if" and constantly assess how the past actions could have been different. For instance, we create counterfactual alternatives that change an undesirable action to be like a desirable one. For instance, if she had put up her usual effort she would have done better in the exam. The capacity to extract causal knowledge from the environment allows us to predict future events and to use those predictions to decide on a future course of action. Counterfactual conditionals are the special kind of subjunctive conditionals that facilitate us to explore other alternatives, in which the antecedent is typically considered to be false.

Since counterfactuals are one of the best means of talking about unrealized possibilities, we find its prominence in the understanding of causation, laws, planning, and the reasoning about action. Analysis of various kinds of counterfactuals and its role in the common sense reasoning, mainly the semantics of counterfactuals have attracted the attention of philosophers from antiquity. In this paper, we restrict our self to counterfactuals involving action. We argue that the prominent approach concerning semantics for counterfactuals due to David Lewis has been challenged on the basis of unlikely, or impossible, events. The present study will be an attempt to link the metaphysical conception of possible-world semantics, and the role of action in the evaluation of counterfactuals. We present an extended version of Lewis-Stalnaker semantics that deals with action counterfactuals.

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SYMPLETIC BATTLEFRONTS. PHASE SPACE ARGUMENTS FOR (AND AGAINST) THE PHYSICS OF COMPUTATION

Session 9H

Congress section(s): B4, C2

In this talk, I will defend the idea that we can exploit phase geometrical resources instead of physically meaningless information-theoretical concepts to gain insight on the so-called physics computation (encompassing quantum and statistical analyses of informational processes), which for many authors is "not a science at all" (Norton; 2005). By assessing all the phase arguments existent in the literature defending or attacking the physics of computation, it can be concluded that the possibility of a Maxwell's demon (namely, system's entropy can be diminished by manipulating its constituent particles) and Landauer's principles (i.e. erasing information entails an increase of entropy) depends ultimately on "blending" (Hemmo and Shenker; 2010) and "squeezing" volume-preserving transformations of macrostatistical phase volumes, respectively. Notice that within Boltzmannian statistical mechanics the phase volume of a macrostate corresponds to the thermodynamical entropy of that macrostate.
Given this "Macrostatistical Representational Underdetermination", wherein the validity of either Maxwell's demon or Landauer's principle depends not on how the world is but moreover on the choice of phase space partition or macrostatistical settings, I posit a threefold criterion to select the Best Representational System for the statistical mechanics of computation: namely, the one that can infer that maximal quantity of macrostatistical deterministic data (individual particle's position, momentum, etc.) at a minimum of entropic cost for a given probabilistic fitness. I will suggest that the macrostatistical system having the most optimal combination of (i) entropic efficiency, (ii) inferential power and (iii) dynamical fit is the one mechanically compatible not with Maxwell's demon (as defended by Albert; 2000) but otherwise the one affine to Landauer's principle.
Additionally, I will also defend that the received view on Landauer's principle, mostly inherited from Bennett (1982), must be reconsidered (i) by avoiding physically insignificant information-theoretical approach, (ii) without relying on Szilard-Bennett method (focused on toy model cases) and (iii) based on a rigorous phase spatial framework. In this line, I will follow both Norton's fluctuation-centric position concerning Maxwell's demon exorcisms (Norton; 2018) and Ladyman's naturalistic modal epistemological reinterpretation of Landauer's principle. Under these strong assumptions, I provide a simple but robust phase spatial argument for a reinterpreted Landauer's principle (Ladyman; 2018). Then, I will conclude that, in spite of not being a universal principle of physics, Landauer's principle can tell us something physically meaningful about how much microstatistical dynamical information it can be extracted from a particular macrostatistical apparatus, without any use of exotic information-theoretical concepts.

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MAX WEBER'S DISTINCTION TRUTH/VALUE AND 'OLD-EUROPEAN' SEMANTICS

Session 29B

Congress section(s): B1, B5

In his paper Alexander Antonovskiy considers the communicative and social conditions of modern science as interpreted by Max Weber. Weber associates the modernity of science (unlike art) with the fundamental inaccessibility of "true being" and, as a consequence, with the transitory nature of any scientific achievement. As a result, Weber – partly explicitly and partly implicitly – formulated the meaning of modern science: Why does a scientist need science under (1) external alienation and (2) the inaccessibility of a scientific object? By analyzing the ideal-typical conditions specified by Weber, the concept of "the invariant modernity" of science is formulated. A substantiation is provided to the fact that the concept of scientific cognition in Weber's interpretation bundles conceptually together all other basic concepts of social philosophy, primarily time, (scientific) object, sociality, truth, and values. In this work, Weber proposed the concept of modern science, resting on a certain temporary logic of human life, as it was formulated by Leo Tolstoy. He explicitly refers to the ideas of the Russian thinker, and this is rewarding because it supplements well a general multicultural perspective, characterizing the regional cultural specifics of his contemporary science: American science as an unpleasant but inevitable prospect of transforming a university into a state-run capitalist corporation, capable to produce only temporarily relevant products; French science with its unjustified claim to "eternal" truths and with its "immortal body" (the French Academy of Sciences). In addition, within the context of scientific cognition, the very concept of modernity obtains a definition and loses disposition associated with the perspective of a speaker, relative to whom the past, future, and modernity, as their boundary, obtained their situational definiteness. Now it is possible to speak objectively about modernity.
This article by Weber has always been used as a methodological program for purification and demarcation of science, which, as is known, starts in parallel to be discussed by the Vienna Circle. However, there is a second plane, and, maybe, it can be considered as the main one: Weber's article is valuable as a diagnosis of modern German and in part American science of his time (P. Duhem makes a parallel diagnosis, strangely not even mentioning Weber in his work [Duhem 1991]. It is "American science," controlled by bureaucracy and business (recall Habermas here), that is closest, in Weber's opinion, to the ideal type of modern science. This, if you prefer, is his empirical illustration. On the contrary, French science is farthest from this ideal type of modernity, which Weber develops with the help of Leo Tolstoy.
This regionally differentiated diagnosis and hypothetical prospects for science in various countries allow us to see from the height of our time how and to what extent the trends that Weber noticed relative to German and partially to global science have been accomplished. It seems to me that this work has not been done yet. In his paper Antonovskiy considers the components of this diagnosis–prognosis in detail.

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FORMALISATION AND PROOF-THEORETIC REDUCTIONS

Session 20M

Congress section(s): B2

Proof-theoretic reductions from a prima facie stronger theory to a prima facie weaker one have been used for both epistemological ends (conceptual reduction, reduction of the levels of explanation, relative consistency proofs) and ontological ones (chiefly, ontological parsimony). In this talk I will argue that what a proof-theoretic reduction can achieve depends on whether the proof transformation function meets certain intensional constraints (e.g. preservation of meaning, theoremhood, etc.) that are determined locally, by the aims of the reduction. In order to make this point more precise, I will use Feferman [1979]'s terminology of a formalisation being faithful or adequate, and direct or indirect, and I will consider two case studies:
(I) The proof-theoretic reduction of the prima facie impredicative theory Δ1-1-CA to the predicative theory ACA$<\varepsilon_0$. The aim of this reduction was not to dispense with the assumptions of the former theory in favour of those of the latter, but rather to sanction the results obtained in the former theory from a predicativist standpoint. Even though the patterns of reasoning carried out in Δ1-1-CA are not faithfully represented by proofs in ACA$<\varepsilon_0$, the proof-theoretic reduction yields a conservativity result for Π1-2 sentences, important from a predicative perspective because they define arithmetical (and thus predicative) closure conditions on the powerset of the natural numbers. Using Feferman's terminology, this reduction demonstrates that ACA$<\varepsilon_0$ is an indirectly adequate formalisation of the mathematics that can be directly formalised within Δ1-1-CA, but not an indirectly faithful one, because the methods of proof available within Δ1-1-CA do go beyond those available in ACA$<\varepsilon_0$.
(II) The proof-theoretic reduction of subsystems of second order arithmetic S to first order axiomatic theories of truth T, conservative over arithmetical sentences. The aim of such reductions is to obtain a more parsimonious ontology, thus showing that even though reasoning carried out in S can be epistemically more advantageous (shorter proofs, closeness to informal analytical reasoning, etc.), the existential assumptions of S can be ultimately eliminated in favour of the leaner assumptions of T. I will argue that since the aim of this reduction is the elimination of a part of the ontology of S, we should demand that the proof theoretic reduction is not only indirectly adequate, but also indirectly faithful––that is, that it does not

merely preserve the arithmetical theorems of S, but also that it preserves (under some appropriate translation) the second-order theorems of S.
In the last part of the talk, I will apply these arguments to the debate on theoretical equivalence in the philosophy of science and I will argue that by imposing additional intensional criteria on the proof-theoretic translation function between two theories that are coherent with the aims of the idealisation, we can obtain locally satisfactory syntactical criteria of theoretical equivalence.

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MUTUALLY INCONSISTENT SET THEORETIC-UNIVERSES: AN ANALYSIS OF UNIVERSIST AND MULTIVERSIST STRATEGIES

Session 25D

Congress section(s): B1

Modern set theory presents us with the very curious case of a mathematical discipline whose practice has been decidedly pluralistic in flavor during the last decades. The typical work of a set theorist today consists of starting from a model of set theory and building up different models which not only can be, but usually are, incompatible with one another in the sense of fulfilling mutually inconsistent mathematical statements. This practice is so mathematically fruitful that nowadays there is a whole cosmos of set-theoretic models that are very well researched and worked on, but which contradict each other to the point that they seem to represent different “kinds of set theories”. Recent programs in the philosophy of set theory try to resolve this situation in the most diverse ways, from claiming a pluralistic platonism to trying to return to the picture of single, “true” model of mathematics.
In this talk we want to explain how such a pluralistic practice evolved in the 1960’s; why and how it has been not only tolerated but pursued until now; and we want to analyze various strategies that have been proposed to deal with the scientific pluralism implicit in this practice.

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DOES SCIENTIFIC LITERACY REQUIRE A THEORY OF TRUTH?

Session 14D

Congress section(s): B1, B7

From “flat earthers“ to “anti-vaxxers“, to the hoax cures and diets in social media, the importance of scientific literacy cannot be emphasized enough. On the one hand, this informs one of the challenges of those in science education. Changes in teaching approaches and addressing deficiencies in the curriculum may be done. On the other hand, this opens the discussion to epistemological questions of truth and knowledge. Easily one can go to “The earth is flat is false”, or “Baking soda is not a treatment for cancer” for these kinds of discussions that would involve scientific literacy and epistemology.
This paper aims to show that while scientific literacy may benefit from discussions of epistemological issues, it does not require a theory of truth. This appears counterintuitive since there is a view that epistemology needs to account for the success of science in terms of its being truth conducive. This is the view that Elgin (2017) calls veritism. Following Elgin, some of the problems with veritism in science will be discussed in terms of their relevance to scientific literacy.
Popularizers of science would also probably object to this position, that a theory of truth is not required for scientific literacy. Especially since this paper, would also look back at Rorty’s (1991) views on science to further buttress its position. Indeed, Rorty’s views on science may prove more relevant to issues in scientific literacy than to science itself.
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SYMPOSIUM ON JOHN BALDWIN’S MODEL THEORY AND THE PHILOSOPHY OF MATHEMATICAL PRACTICE

Session 30A

Congress section(s): A1, C1

I will be commenting on Baldwin’s book from my point of view as a philosopher of mathematics. I will concentrate on the question of how model theory sheds light on what is geometry. While geometry was once identified with the study of visualizable figures, since the nineteenth century it has been generalized to a study of space and its properties, where these properties are not necessarily visualizable. One thinks for example of complex projective space, where both points at infinity and imaginary points seem to elude visual representation. Model theorists, starting from the brilliant insights of Zilber, have suggested that logical features of our study of space may be connected to---indeed, may account for---what geometry is and has been become. Our logical aims to study uncountably categorical theories seems to lead inexorably to classically geometrical structures. Can one then say that at its heart, geometry as we study it is constrained by the logics we prefer? If so, then one here finds a kind of neo-Kantianism, where one replaces forms of intuition by logical forms. Baldwin’s book frames this logical work in a fruitful way, and in my discussion I will make more explicit these tantalizing themes.

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THE UNIVERSALISM OF LOGIC AND THE THEORY OF TYPES

Session 5K

Congress section(s): A4

In his famous essay “Logic as a Calculus and Logic as a Language”, van Heijenoort (1967) describes a widespread picture of logic which is called the “Universalistic Conception of Logic”. According to him, this picture emerges from Frege’s and Russell’s writings as opposed to the Algebra of Logic, represented by Boole, Löwenheim, etc. A formal system is “universal” iff (1) quantifiers range over unrestricted variables; (2) the universe consists of all that exists and it is fixed; and (3) there is not an external standpoint, so nothing can be said outside the system. From these premises, van Heijenoort concludes that no metatheoretical question could be meaningfully posed by Frege and Russell, on account of their conception of Logic.
This interpretation of logicism has been developed by young students who attended, during the 1960s, to Dreben’s lectures and seminars in Harvard. Among others, stand out Goldfarb (1979, 2001 and 2005) and Ricketts (1985 and 1986). In their opinion, logicism and metatheory were mutually exclusive. However, several commentators have recently pointed out the necessity of revisiting Frege’s and Russell’s contributions to provide a more balanced interpretation. Tappenden (1997), Proops (2007) and Heck (2010) rejected this account of logicism by adducing new textual evidence against the Universalistic Conception of Logic.
In this talk, I will critically examine the claims (1)-(3) in the light of Russell’s conception of Logic. First of all, I will discuss Russell’s ontology to assess the plausibility of (1). Secondly, I will argue that (2) is not a sufficient condition for the impossibility of metatheory, since some logicians have posed metatheoretical questions in a fixed domain (for example, Carnap and Tarski). Finally, I will show that (3) is false, because Russell seemed to be worried about certain metatheoretical issues in his Principia Mathematica. Furthermore, he was fully aware of Bernays’s investigations on independence, so there was no intrinsic incompatibility between these kind of results and his conception of Logic.
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SOURCES OF PEANO’S LINGUISTICS

Session 4K

Congress section(s): A4

Guiseppe Peano’s axiomatizing effort in Formulario went hand in hand with the design of an innovative mathematical notation that allowed to write any formula without recourse to ordinary language. Alongside his contemporaries such as Frege, Boole, Schröder and Peirce, Peano stressed the importance of a rigorous language for mathematics as an alternative to local, ambiguous and cumbersome formulations in ordinary language. His project was greatly influenced by Leibniz, as much in its encyclopedic scope as in its use of a universally readable ideography. Leibniz’ ambitious philosophical project had also a more modest linguistic counterpart that inspired attempts at creating an international auxiliary language (IAL), including Peano’s. Following Leibniz’ instructions for a Latin-based interlanguage, Peano created Latino sine flexione (LSF). With other mathematicians such as Louis Couturat and Léopold Léau, Peano advocated for the worldwide adoption of an IAL. Not only he used LSF in his own writings, including the fifth edition of Formulario, but he took a leading role in the IAL movement by setting up and presiding Academia pro Interlingua, where he promoted LSF as a solution to the problem of multilingualism in an age of competing nationalisms.
In its grammatical structure, LSF owes significantly to Leibniz’ project of a universal characteristics. Peano’s major intervention in the Latin language was the elimination of elements that he considered logically superfluous: inflection, grammatical gender, and redundant expression of number. He systematically replaced inflected forms of Latin words by their simplest form accompanied by separate prepositions and adverbs, in the aim of reaching a transparent syntax where all words occur only in their form found in the dictionary. For Peano, there is a good reason to get rid of inflections in Latin, since they add to its difficulty and, consequently, make it unfit for international communication in the modern world, despite the familiarity of much of its vocabulary. For Peano, eliminating inflections leads to a “minimal grammar” found also in Chinese (a model for Leibniz, as well as Frege’s Begriffsschrift), Modern English (compared to its older forms), and the symbolic language of modern mathematics.
In its analytic structure, LSF appears as a linguistic application of Peano’s ideographical endeavor that produced Formulario. Referring to Max Müller and Michel Bréal, Peano used the distinction logic/linguistics to question established linguistic forms and suggest more “rational” ones. Yet, as much as Peano owes to the idea of conceptual writing, formulated by Leibniz and carried further by Frege, other factors determined the emergence and principles of LSF; namely, advances in the historical and comparative linguistics of his time, and – not the least – the turn-of-the-century organization of scientists into a transnational community with the help of a growing number of international institutions and events.

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ARE LOGICAL EXPRESSIONS AMBIGUOUS AND WHY?

Session 24M

Congress section(s): A2

The relation of logical expressions from natural languages and those of formal logics has been an object of much study and many opinions. Already Frege used an interesting simile, comparing formal logic to a microscopce and everyday reasoning to bare eye. New challenges, though, appear in face of plurality of logics. Are some logics constructed in such a way that their constants are more faithful to their counterparts in natural language? And is it even a virtue, given Frege´s metaphor? I will examine what might seem to be the most natural way of coping with this issue, namely that logical expressions of everyday language are ambiguous and the formal systems spell out their more definite and exact shapes (this view is present in works of many authors, for example in Logical Pluralism by Beall and Restall). As this view has a lot of appeal, I want to highlight the ways in which it can be misleading and should at the very least be ammended, if not abandoned.
The view I just sketched and which I want to criticize can be rendered more precise by saying that logical expressions of natural language are in fact disjunctions of various meanings. Thus the word ‚not‘ is on some occasions used to mean classical negation, sometimes to mean the intuitionistic one, sometimes others yet. The fact, though, is that new logical systems keep being developed and therefore if this view were right, we could never be sure which possibilities there are in the mentioned disjunctions of meanings. Furthermore, questions arise about which of the disjuncts of the purported meaning one must know in order to be called a competent user of the given expression.
I therefore propose a different view of meaning in general, which will have interesting consequences for the meanings of logical expressions. I regard meaning as constituted by rules regulating the use of a given expression and these rules are constituted by an interplay of normative attitudes of members of a given community of speakers (this understanding is originally due to Brandom and can be also found, for example, in Peregrin). These attitudes are continuous and never in fact accomplished. What we regard as right has to be retold anew in contexts we happen to find ourselves in and thefore the meanings of our expressions keep being developed. Regarding meaning as something static in a given state thus means distorting it in a significant way. Understanding a given expression esssentially involves partaking in its further developement, not just storaging it in one’s memory.
Applying this approach to logical expressions means seeing them rather as the very movement between the various shapes we know from formal logics, not as a conglomerate or disjunction of these shapes. Formal logics thus also attain a different role than the one usually ascribed to them. They are here to modify, rather than to capture our usage of logical expressions.
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POPPER ON THE MIND-BRAIN PROBLEM

Session 11C

Congress section(s): B6

Popper’s influence on science can be traced within many branches. It ranges from direct contributions, such as suggestions of experiments in quantum mechanics (e.g. the so-called Popper experiment, testing the Copenhagen interpretation) to mere inspiration, waking up scientists from their dogmatic slumber. Especially his criticism of instrumentalism and his advocacy of realism has been an eye-opener for many. As an illustration a case from the field of neuroscience is discussed in the paper. It relates to the development of theories about mechanisms underlying the nerve impulse. The central question after the pioneering studies by Hodgkin and Huxley was how the critical ions permeate the nerve cell membrane (Hille, 2001). Some experimentalists adopted a realistic view and tried to understand the process by constructing mechanistic models, almost in a Cartesian fashion. Others adopted an instrumentalistic, positivistic and allegedly more scientific view and settled

for a mathematical black box description. When it finally was possible to experimentally determine the molecular details, they were found to fit the realistic, mechanistic attempts well, while the instrumentalistic attempts had not led far, thus supporting the Popperian view.
An important part of Popper's philosophy concerns the mind-brain problem. The present paper discusses two aspects of his philosophy of mind. One aspect relates to the ontology of mind and the theory of biological evolution. During the years Popper's interest in evolution steadily grew; from an almost negative patronizing view to giving it a central role in many of his later studies. In the theory of evolution Popper found support for his interactionistic view on the mind-brain problem. This, as will be discussed, is a view that for many philosophers is difficult to accept. Another aspect discussed is Popper's observation that mind has similarities with forces and fields of forces. As Popper points out, the introduction of forces as such (in the dynamism of Newton) could have been used by Descartes' adherents to avoid inconsistencies of the Cartesian dualism. But Popper has developed this idea further, comparing properties of mind with those of forces and fields of forces (Popper, Lindahl and Århem, 1993). His view has renewed the interest in force fields as a model for consciousness and the present paper discusses some recent hypotheses that claim to solve problems that attach to the dominant present-day mind-brain theories. Several authors even identify consciousness with an electromagnetic field (Jones, 2013). In contrast, Popper proposes that consciousness works via electromagnetic forces. (Lindahl and Århem, 1994). This has been criticized as violating thermodynamic conservation laws. The present paper discusses Popper's defence against this argument. The paper also discusses a related hypothesis that consciousness act on fields of probability amplitudes rather than on electromagnetic fields. Such an idea has been proposed by Friedrich Beck in response to Popper's hypothesis (Beck, 1996). The present paper argues that such models, based on quantum mechanical ideas, often are in conflict with Poppers propensity interpretation of quantum mechanics (Popper, 1982).

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THE HISTORICAL BASIS FOR ALGORITHMIC TRANSPARENCY AS CENTRAL TO AI ETHICS

Session 11F

Congress section(s): C6

This paper embeds the concern for algorithmic transparency in artificial intelligence within the history of technology and ethics. The value of transparency in AI, according to this history, is not unique to AI. Rather, black box AI is just the latest development in the 200-year history of industrial and post-industrial technology that narrows the scope of practical reason. Studying these historical precedents provides guidance as to the possible directions of AI technology, towards either the narrowing or the expansion of practical reason, and the social consequences to be expected from each.
The paper first establishes the connection between technology and practical reason, and the concern among philosophers of ethics and politics about the impact of technology in the ethical and political realms. The first generation of such philosophers, influenced by Weber and Heidegger, traced the connection between changes in means of production and the use of practical reason for ethical and political reasoning, and advocated in turn a protection of practical reasoning – of phronesis – from the instrumental and technical rationality valued most by modern production. More recently, philosophers within the postphenomenological tradition have identified techne within phronesis as its initial step of formation, and thus call for a more empirical investigation of particular technologies and their enablement or hindering of phronetic reasoning.

This sets the stage for a subsequent empirical look at the history of industrial technology from the perspective of technology as an enabler or hindrance to the use of practical reasoning and judgment. This critical approach to the history of technology reveals numerous precedents of significant relevance to AI that from a conventional approach to the history of technology focusing on technical description appear to be very different from AI – such as the division of labor, assembly lines, power machine tools and computer-aided machinery. What is revealed is the effect of most industrial technology, often quite intentional, in deskilling of workers by narrowing the scope of their judgment, whereas other innovations have the potential to expand the scope of workers' judgment. In particular, this section looks like the use of statistics in industrial production, as it is the site of a nearly century-long tension between approaches explicitly designed to narrow or expand the judgment of workers.
Finally, the paper extends this history to contemporary AI – where statistics is the product, rather than a control on the means of production – and presents the debate on explainable AI as an extension of this history. This final section explores the arguments for and against transparency in AI. Equipped with the guidance of 200 years of precedents, the possible paths forward for AI are much clearer, as are the effects of each path for ethics and political reasoning more broadly.

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INCOMPLETENESS-BASED FORMAL MODELS FOR THE EPISTEMOLOGY OF COMPLEX SYSTEMS

Session 16I

Congress section(s): B2, C5

The thesis I intend to argue is that formal approaches to epistemology deriving from Goedel incompleteness theorem, as developed for instance by Chaitin, Doria and da Costa (see [3]), even if originally conceived to solve decision problems in physical and social sciences (e.g. the decision problem for chaotic systems), could also be used to adress problems regarding consistency and incompleteness of sets of beliefs, and to define formal models for epistemology of complex systems and for the "classical" systemic-relational epistemology of psychology, such as Gregory Bateson's epistemology (see [2]) and Piaget's Genetic Epistemology (see for instance [4]). More specifically, following systemic epistemology of psychology, there are of two different classes of learning and change processes for cognitive sytems: a "quantitative learning" (the cognitive system adquires information without changing the rules of reasoning) and a "qualitative" learning (an adaptation process which leads the system to a re-organization). Therefore, as in Incompleteness theorems the emergence of an undecidable sentence in a logical formal system lead to the definition of a chain of formal systems, obtained by adjoining as axioms propositions that are undecidable at previous levels, in the same way, the emergence of an undecidable situation for a cognitive system could lead to the emergence of "new ways of thinking". Thus, a (systemic) process of change (process of "deuterolearning"), could be interpreted as a process that leads the cognitive organization of the subject to a different level of complexity by the creation of a hierarchy of abstract relations between concepts, or by the creation of new sets of rules of reasoning and behaving (where the process of learning is represented by a sequence of learning-stages, e.g. by sequences of type-theoretically ordered sets, representing information/proposition and rules of reasoning/rules of inference).
I will propose two formal models for qualitative change processes in cognitive systems and complex systems:
The first, form set set theory, is based on Barwise's notion of partial model and model of Liar-like sentences (see [1]).
The second, from proof theory and algebraic logic, is based on the idea that a psychological-change process (the development on new epistemic strategies), is a process starting from a cognitive state s_0 and arriving to a cognitive state s_n, possibly assuming intermediate cognitive states s_1 , . . . , s_(n−1) : developing some researches contained in [5] and [6], I will propose a model of these processes based on the notion of paraconsistent consequence operator.
I will show that these two different formal models are deeply connected and mutually translatable.
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MULTICAUSALITY AND MANIPULATION IN MEDICINE

Session 12J

Congress section(s): C4

The objectivity of causality in its observable aspects is generally characterized by the reference to the concrete alteration of the effects due to the alteration in a cause. One of the ways of making a causal relationship takes place is precisely by human intervention in the factor that is considered the cause. This type of deliberate intervention, which an agent can produce with manipulable factors, is absolutely intrinsic to medicine. My interest here is to present how medicine, as a practical science, articulates the multiple factors and phenomena that act on an organism in order to understand cause and effect relationships. To that end, I associate JL Mackie's and Kenneth Rothman's theories about the necessary and sufficient conditions for the cause-effect relation to the theory of manipulability. This theory, in general, identifies the causal relation as that in which some kind of intervention in the cause gives rise to the effect. Medical science is distinguished exactly by the practices it performs, without which it would lose its own meaning. In this way, medicine is one of the sciences in which the relation between cause and effect can be evaluated objectively.
Despite these observable aspects, a problem rises. Faced with the complexity of an organism, where several factors act together to produce an effect, how to delimit the cause on which to intervene? The proper functioning of the organism is not based on the functioning of isolated causes. If, on the one hand, the analysis of causality from a singularist perspective in sciences like medicine is impracticable, on the other hand, this analysis becomes more complicated if we add the fact that some physiological mechanisms are absolutely unknown. That is to say, in treating the organism, medicine depends fundamentally on intervention in cause-effect relationships, in a complex system with some mechanisms that are not absolutely clear. Nonetheless all these difficulties, medicine is recognized for succeeding in the various activities that concern it. In this context, both Mackie's and Rothman's conceptions on cause-effect relationship helps us to understand the role of intervention in medicine and its consequences for the general conception of causality.
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MEDIA MEMORY AS THE OBJECT OF HISTORICAL EPISTEMOLOGY

Session 11H

Congress section(s): B1, B3, B6, C7

Introduction. Under modern conditions the influence of electronic media on social construction of historical memory is huge. Historical information is transferred to a digital format, not only archives and libraries accumulate the knowledge of the Past, but also electronic storages of databases. Written memory gives way to electronic memory, and development of Internet technologies provides access of a massive number of users to it.
Today the idea of the Past is formed not only by the efforts of professional historians, but also Internet users. The set of individual images of history creates collective memory. Modern society is going through the memory boom which is connected with the ability of users to make knowledge of the Past and to transmit it through new media. Thus, the memory from personal and cultural space moves to the sphere of public media. This process is about the emergence of media memory.
Methods. The research of influence of media on individual and collective memory is based on M. McLuhan's works. Studying of social memory is carried out within M. Halbwachs's theory about «a social framework of memory», the theory of cultural memory of J. Assmann and the theory of «places of memory» P. Nora. The analysis of ideas of the Past is based on the methods of historical epistemology presented in H. White and A. Megill's works.
Discussion. A small number of studies is devoted to the influence of media on social memory. One of such works is the collective monograph "Silence, Screen, and Spectacle: Rethinking Social Memory in the Age of Information and New Media", edited by L. Freeman, B. Nyenas and R. Daniel (2014). The authors note that new social media change the nature of perception of the Present and Past, revealing the Past through metaphors «silence», «screen», and «performance».
The mediatization of society hasproduceda special mechanism of storage, conversion and transmission of information which changed the nature of production of historical knowledge and practice of oblivion. Also, the periods of storage of social information have changed.
According to theabove mentioned the author defines media memory as the digital system of storage, transformation, production and dissemination of information about the Past. Historical memory of individuals and communities is formedon the basis of media memory. Media memory can be considered as the virtual social mechanism of storing and oblivion, it has an opportunity to provide various forms of representation of history in daily occurrence space, to expand practice of representation of the Past and a commemoration and also to increase quantity creating and consuming memorial content.
Standing on the position of historical epistemology we can observe the emergence of new ways of the cognition of the Past. Media memory selects historical knowledge, including relevant information about the Past in the agenda, and subjecting to oblivion the Past with no social need. Also, there is segmentation of historical knowledge between various elements of the media sphere. It is embodied in a variety of historical Internet resources available to users belonging to different target audiences.
Media memory is democratic. It is created on the basis of free expression of thoughts and feelings by available language means. Photos and documentary evidence play equally important roles in the formation of ideas of the Past alongside with subjective perception of reality and estimating statements. Attempts to hide any historical information or withdraw it from public access lead to its greater distribution.
Conclusion. Media memory as a form of collective memory is set within the concept of the post-truth when the personal history and personal experience of reality replace objective dataforaparticular person.The knowledge of history gains new meanings, methods and forms, this in its turn makes researchers look for new approaches within historical epistemology.

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DOING WITHOUT STRUCTURAL REPRESENTATIONS

Session 18H

Congress section(s): C5

Despite the fact that the notion of a representation is a cornerstone of cognitive science, a definition of this central concept remains elusive. In this paper I would like to concentrate on the notion of 'Structural representation' (or 'S-representation'), which has become a recent focus of attention in the specialized literature. In a nuthsell, a particular cognitive mechanism M is a structural representation of S iff (1) there is a homomorphism between M and S (roughly, a mapping between a relations in M and relation in S) and (2) some cognitive mechanism uses this homomorphism to deal better with the evironment (Cummins, 1989; O'Brien, 2015; Ramsey, 2007; Gladziejewski and Milkowski 2017). Crucially, the notion of S-representation has recently been employed to distinguish the set of genuine representations from the category of receptors, that is, those internal mechanisms that reliably correlate with certain evironmental features but which, according to these authors, should not qualify as proper representations. Thus, the concept of S-representation plays a fundamental role in recent attempts to defend a form of representationalism that can escape the objection of being too liberal, i.e. attributing representations to many states that intuitively lack them.

The goal of this paper is to argue that the notion of S-representation cannot fulfil this theoretical role. First of all, I will argue that the above definition can be undestood in at last two ways. On the one hand, if this notion is understood as merely requiring that there is a homorphism between a cognitive mechanism M and a structure S and that M is exploited to behave appropriately, then mere receptors seem to satisfy this requrement (Morgan, 2014). A mechanism that can be in two states, which reliably covary with certain world events exemplifies a homomorphism that the system employs to deal with the environment.

To avoid this result, one could provide a more restrictive interpretaton of the notion of S-representation, according to which the use of the homomorphism mentioned in (2) above necessarily implies using the relations between M off-line in order to learn about S. The problem with this interpretation, however, is that it is too narrow, since processes that clearly should be classified as representations (even as 'structural representations', in an important sense of the term) would be excluded, such as waggle dances produced by bees (Shea, 2014) or certain kinds of cognitive maps. As a conclusion, then, a broad understanding of S-representation is too liberal because it does not allow this notion to exclude mere receptors and a more restrictive interpretation is too narrow since it excludes clear cases of representations. Consequently, this notion cannot play the job it is supposed to perform.

This negative result, however, should not be taken to imply that there are no interesting differences between mere receptors and more complex forms of representation. The final part of the paper seeks to sketch some notions that, in combination, are in a better position the play this role. The suggested analysis will include the notions of 'lexical productivity' (the capacity to produce new meaningful tokens), structural productivity and off-line use. I will defend that these concepts are much more useful for cutting the nature of representation a its joints and that they can be employed to make fruitful and illuminating distinctions between different kinds of representational phenomena.

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ELIMINATING PAIN

Session 30L

Congress section(s): C5

I defend pain eliminativism against two recent challenges, Corns (2016) and van Rysewyk (2017). Both challenge Dennett's (1978) and Hardcastle's (1999) critiques of the common-sense notion of pain and its inadequacy for scientific study of pain. The two converge in their interpretation of eliminativism as a prediction about the replacement of folk-psychological vocabulary by the vocabulary of a mature neuroscience (of pain in particular). They differ in that Corns admits that eliminativism for pain has had success in science but not in everyday contexts, whereas van Rysewyk shows that contemporary pain research still makes use of the folk-psychological notion of pain for the purposes of research and treatment of pain. Both conclude that this falsifies the radical claims of pain eliminativism.

I will show that both Corns' and van Rysewyk's positions are in fact compatible with eliminative materialism as originally articulated by Churchland (1981, 1985). I argue that Churchland's version of eliminativism makes the case for the necessity of mature neuroscience vocabulary, but not its sufficiency, for eliminating folk psychology. Corns' and van Rysewyk's arguments only go against the sufficiency of mature neuroscience for the replacement of folk psychology.

Following Machery (2009), Corns distinguishes between "scientific eliminativism" and "traditional eliminativism" for pain. Her main claim is that Dennett's and Hardcastle's theories provide, at best, sufficient reasons to accept scientific eliminativism. Therefore, it is justified to expect scientists to abandon the folk-psychological notion of pain. However, Corns argues, scientific eliminativism does not entail traditional eliminativism. In other words, mature neuroscience of pain may not eliminate "pain" from everyday discourse.

I will not take issue with Corns' account of the relative success of scientific eliminativism. I will argue, however, that her account of the relevance of scientific eliminativism, or rather lack thereof, to everyday use of "pain" is oversimplified.

Van Rysewyk, on the other hand, argues that the folk-psychological notion of pain is compatible with mature neuroscience of pain. He shows that it has been and is still used in psychological and clinical research. His example is of the success of educational neurophysiological and pain-management programs that provide accurate neurophysiological information to trained patients by means of metaphors (p. 79).

In my interpretation, however, the success of these programs is due to the presentation of accurate neuroscientific information and this is consistent with eliminativism. The usefulness of the folk-psychological vocabulary about pain does not undermine the conceptual replacement of an inadequate theory with a better theory about pain.

Traditional eliminativism for pain is a viable option. We have good reasons to maintain that the folk-psychological notion of pain as a subjective and private experience is in fact eliminated from neuroscientific vocabulary and we have good reasons to expect that the folk-psychological notion of pain will be replaced with a more refined notion. Even if this prediction is not fulfilled, pragmatic considerations for preserving folk-psychological vocabulary about pain don't prove that the purported referents of the terms in that vocabulary exist. Folk talk about ghosts and demons. This does not make them real.

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IN DEFENCE OF THE EVIDENTIAL ROLE OF MECHANISTIC REASONING

Session 11I

Congress section(s): C4

Mechanistic reasoning involves a process of inferring that a medical intervention will have a particular effect on the basis of evidence of underlying biological mechanisms. The principles of evidence-based medicine (EBM) maintain that the best evidence for the effectiveness of medical interventions is obtained from comparative clinical studies. Evidence of the underlying mechanisms typically does not provide evidence for the effectiveness of medical interventions. Miriam Solomon (2015) is a philosopher who supports this view. Her argument appeals to the unreliability of mechanistic reasoning in medicine. There are numerous examples of treatments proposed where we had good evidence of mechanism, which then turned out to be ineffective. Jeremy Howick (2011) is a philosopher who argues that mechanistic reasoning can provide evidence of effectiveness, while still acknowledging that it is rare that this is the case. A problem for mechanistic reasoning raised by both Howick and Solomon is that mechanisms are complex and our knowledge of mechanisms is almost always incomplete. Moreover, it is hard to know when a mechanism is complete as there is always the possibility of counteracting mechanisms.

In this paper I argue that mechanistic reasoning is not the only way that evidence of mechanisms might provide evidence of effectiveness. A more reliable type of reasoning may be distinguished by appealing to recent work on evidential pluralism in the epistemology of medicine (Clarke et al. 2014). In an instance of so-called reinforced reasoning, evidence of mechanisms can provide evidence for the effectiveness of a medical intervention. This is only the case when mechanistic reasoning is combined with correlational reasoning, where correlational reasoning involves a process of inferring that an intervention will have a particular effect on the basis of evidence of a correlation (typically obtained from clinical studies). A case study from virology provides an example of reinforced reasoning in medicine. This case study involves putative treatments for middle east respiratory syndrome (MERS). One potential treatment is a combination therapy of ribavirin and interferon. The rationale for this treatment is based on evidence of a mechanism linking combination therapy and survival in MERS patients, and limited clinical evidence. Mechanistic or correlational reasoning alone cannot determine whether combination therapy is effective. Reinforced reasoning however can because the strengths of correlational reasoning complement the weaknesses of mechanistic reasoning - e.g. if there is a correlation then it is less likely that the mechanism is counteracted - and the weaknesses of correlational reasoning are complemented by the strengths of mechanistic reasoning - e.g.

correlations can be confounded, and mechanisms can rule out the presence of confounders. I show that in the case study only a combination of clinical and mechanistic evidence can provide evidence of combination therapy's effectiveness.
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INFORMAL RIGOROUS MATHEMATICS AND ITS LOGIC

Session 25D

Congress section(s): A1, A2, B1

Mathematical practice in all its forms, and despite formidable technicalities, is a natural-language practice. Thus, the logic(s) of that practice is implicit; and, in turn—like claims about the logic(s) of natural language—what logic or logics are operative in mathematics is an empirical question. There is a normative question in the neighborhood. Regardless of what the descriptive situation vis-à-vis the logic(s) of mathematics is discovered to be, we can nevertheless ask the further question: what logic(s) should mathematics be governed by? This further question requires clarity about the function(s) of mathematics. It's important to realize that although mathematics is a natural-language practice, the answers to both the descriptive and the normative questions about mathematics and natural languages, generally, needn't be the same.
The gold standard for logic, I suggest, is Frege's. If we compare some form of syllogistic logic, or one place-predicate logic, with the standard predicate logic, we find an enormous advance in terms of formalizing the reasoning of mathematical proof: the project of characterizing classical mathematical reasoning seems solved by the use of this logic. This is because predicate relations can be represented in the standard predicate calculus and they are crucial to mathematical reasoning. In making this claim, it needs to be shown that classical higher-order logics don't provide characterizations of mathematical theorizing that go beyond what's available in the first-predicate calculus—but this can be shown. In particular, categoricity results in a first-order logical system are always available even without those results being true of such systems.
The important question is whether a similar advance is possible, taking us beyond the standard predicate calculus. A tempting possibility is afforded by the many examples in the history of mathematics where apparently inconsistent principles were employed. (Euler's free and easy ways with infinite series are often cited; infinitesimal reasoning is another example.) I claim that there isn't anything here that motivates characterizing mathematical practice according to non-classical logics that allow true contradictions, or the like.
Pluralist conceptions of the logic(s) of mathematics, however, can be motivated by considerations independent of the foregoing. This is because of the global reach of mathematics. One can simply study in mathematics any subject area subject to any logic one imagines: set theory in a multivalued logic, intuitionistic real analysis, and so on. Furthermore, although it's not uncommon to study these subject areas from a "classical" viewpoint, that isn't required. To speak somewhat picturesquely, one can use an intuitionistic metalanguage to derive results about intuitionistic set theory.
Applying mathematics to empirical sciences—I claim—is a different matter. Here the mathematics must function under one logical umbrella: whichever logic is operative in the sciences. I claim that the holism typical of the sciences—that results from any area may be applied to any other—requires the over-arching logic to be one, and (as of now, at last) to be classical. Pluralism in mathematics itself is a truism; pluralism in applied mathematics is forbidden.

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A GENERALIZED OMITTING TYPE THEOREM IN MATHEMATICAL FUZZY LOGIC

Session 14K

Congress section(s): A1

Mathematical fuzzy logic (MFL) studies graded logics as particular kinds of many-valued inference systems in several formalisms, including first-order predicate languages. Models of such first-order graded logics are variations of classical structures in which predicates are evaluated over wide classes of algebras of truth degrees, beyond the classical two-valued Boolean algebra. Such models are relevant for recent computer science developments in which they are studied as weighted structures.
The study of such models is based on the corresponding strong completeness theorems [CN,HN] and has already addressed several crucial topics such as: characterization of completeness properties w.r.t. models based on particular classes of algebras [CEGGMN], models of logics with evaluated syntax [NPM,MN], study of mappings and diagrams [D1], ultraproduct constructions [D2], characterization of elementary equivalence in terms of elementary mappings [DE], characterization of elementary classes as those closed under elementary equivalence and ultraproducts [DE3], Löwenheim-Skolem theorems [DGN1], and back-and-forth systems for elementary equivalence [DGN2]. A related stream of research is that of continuous model theory [CK,C].
Another important item in the classical agenda is that of omitting types, that is, the construction of models (of a given theory) where certain properties of elements are never satisfied. In continuous model theory the construction of models omitting many types is well known [YBWU], but in MFL has only been addressed in particular settings [CD,MN]. The goal of the talk is establish a new omitting types theorem, generalizing the previous results to the wider notion of tableaux (pairs of sets of formulas, which codify the properties that are meant to be preserved and those that will be falsified).
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KARL POPPER'S THREE INTERPRETATIONS OF THE EPISTEMOLOGICAL PECULIARITIES OF THE SOCIAL SCIENCES

Session 5G

Congress section(s): B1, B6, C7

In this paper, I will show that Karl Popper's philosophical oeuvre contains three different interpretations of the epistemological peculiarities of the social sciences, and more specifically of economics. At first, and most notably in his "Poverty of Historicism", he relied on the epistemological insights that he had developed thanks to his works on the epistemology of physics to propose an epistemological account of the social sciences very similar to that of physics. This epistemological account, which makes extensive use of the criterion of falsifiability, is the most famous one. It is, for example, this account that considerably influenced Milton Friedman's highly influential epistemological treatise, "The Methodology of Positive Economics", written in 1953. However, it is often overlooked that Popper later on changed his position. Already in "The Open Society and its Enemies", we find the general contours of a second interpretation of the epistemology of the social sciences that is very different from the previous one. This second account develops a distinctly normative epistemology for the social sciences, but its presence in the book is not adequately taken note of because of the concomitant presence of Popper's first epistemological position in the book. It will be shown that Popper maintained these two different positions because he could not, at the time, make up his mind about the exact nature of the relationship between theoretical and historical social sciences. It was only in two later texts, "The Logic of Social Sciences" and "Models, Instruments, and Truth", that he abandoned the distinction between theoretical and historical social sciences, in order to argue in favor of the fundamental role played by history in all social sciences. This then led him to extend the logic of the situation, the form of logic that he had previously developed for historical social sciences, to all social sciences, thereby coming up with his third account of the epistemology of the social sciences. It will be shown, moreover, that by taking this latter development into account, we can give more accuracy to the second position that he had developed in "The Open Society and its Enemies". I will try to argue that it is this second position, which is about developing a normative epistemology for the social sciences, that constitutes Popper's most original contribution to the epistemological debates on the social sciences.

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MATHEMATICAL AND PHILOSOPHICAL PROBLEMS ARISING IN THE CONTEXT OF THE BOOK

Session 31A

Congress section(s): A1, A4, C1

Here are some examples of the kinds of philosophical/mathematical questions that arise in the framework of the book.
1) Zilber wrote in 2000: 'The initial hope of this author that any uncountably categorical structure comes from a classical context (the trichotomy conjecture), was based on the belief that logically perfect structures could not be overlooked in the natural progression of mathematics.' In more concrete terms Zilber proposed that all 'logically perfect' (categorical in all uncountable powers) structures will naturally arise as 'canonical structures' in mathematics. The natural examples were the integers under successor, vector spaces, and algebraically closed fields. Hrushovski's construction destroyed his precise conjecture. But the conjecture raises philosophical issues. While 'classical' might be construed as a descriptive historical term, logically perfect and canonical are normative philosophical terms. What can they mean? Here is a related mathematical issue. Shelah's dividing line methodology led to a striking solution of the problem of classifying first order theories into those which admit countable trees of invariants and those which have the maximal number of modelsinalluncountablecardinals. But, the counter example to Zilber's conjecture shows that the basic building blocks of this classification, the strongly minimal sets, themselves admit immense diversity. Can one find finer dividing lines to better understand these atoms of the classification?
2) The ordinary mathematician considers a structure as a unique object defined in set theory (if he considers the issue at all). But then he treats all structures isomorphic to the given one as equals and regards the isomorphism type by neglecting, as Dedekind says, 'the special character of the elements, simply retaining their distinguishability and taking into account only the relations to one another in which they are placed by the order-setting mapping'. The mathematicians work continues by making clear when a representative is chosen from the isomorphism type. Structuralism insists on reifying in one way or another the isomorphism type as object of study. Can a philosopher explain to mathematicians, why he is not content with the usual approach which can be carried out in (a weak subsystem of) ZFC?
3) Angus Macintyre pointed out an historical fact with philosophical implications. By the time Hilbert wrote his Geometry, it had been seventy years since the independence of the parallel postulate was proved and in that time the meanings of 'geometry' had multiplied. A single description of space that was seen as a foundation for all mathematics had been replaced by not only Riemannian, hyperbolic, and elliptic variants of the description but such orthogonal notions as projective, algebraic, and differential geometry. Despite a few stirrings, there is not a similar situation in set theory, a hundred years after Zermelo's formulation of the axioms. How does this fact impact the argument for Hamkin's multiverse? It appears that the varieties of geometry I have recounted are different in kind from the various ex tensions of ZFC that have been proved consistent. Is there a cogent way to express this distinction?

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LEARNING PROBABILITIES: A LOGIC OF STATISTICAL LEARNING

Session 14B

Congress section(s): A2

We propose a new model for forming beliefs and learning about unknown probabilities (such as the probability of picking a red marble from a bag with an unknown distribution of coloured marbles). The most widespread model for such situations

of “radical uncertainty” is in terms of imprecise probabilities, i.e. representing the agent’s knowledge as a set of probability measures. We add to this model a plausibility map, associating to each measure a plausibility number, as a way to go beyond what is known with certainty and represent the agent’s beliefs about probability. There are a number of standard examples: Shannon Entropy, Centre of Mass etc. We then consider learning of two types of information: (1) learning by repeated sampling from the unknown distribution (e.g. picking marbles from the bag); and (2) learning higher-order information about the distribution (in the shape of linear inequalities, e.g. we are told there are more red marbles than green marbles). The first changes only the plausibility map (via a “plausibilistic” version of Bayes’ Rule), but leaves the given set of measures unchanged; the second shrinks the set of measures, without changing their plausibility. Beliefs are defined as in Belief Revision Theory, in terms of truth in the most plausible worlds. But our belief change does not comply with standard AGM axioms, since the revision induced by (1) is of a non-AGM type. This is essential, as it allows our agents to learn the true probability: we prove that the beliefs obtained by repeated sampling converge almost surely to the correct belief (in the true probability). We end by sketching the contours of a dynamic doxastic logic for statistical learning.

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EARLY MODERN CHEMICAL ONTOLOGIES AND THE SHIFT FROM VITALISM TO MECHANICISM

Session 5A

Congress section(s): B1

From a philosophical point of view, one of the more significant changes that occurred in chemical philosophy from the late 16th to the 17th century is the shift from the vitalistic metaphysics that had dominated Renaissance natural philosophy to the mechanistic theory of matter championed by the Cartesians and Newtonians.
The shift away from vitalism and toward mechanicism was gradual rather than abrupt, and aspects of vitalism and of mechanicism coexisted in interesting ways within the chemical ontologies of many early modern chymists. In spite of the tensions between these two opposing metaphysical paradigms, one important thread that connects early modern chymical theories, whether vitalistic or mechanistic, is their ontological commitment to corpuscular theories of matter.
The historical process whereby ancient Democritean atomism was revived in the 16th century is quite complex, but it would be a mistake to assume that particulate theories of matter need imply a commitment to physicalism and mechanicism. In fact, although the atomism of such natural philosophers as Gassendi and Charleton was indeed mechanistic, one finds many examples of medieval, Renaissance, and early modern that embraced vitalistic metaphysics while endorsing a corpuscularian theory of matter.
As it happens, there is strong evidence to show that, for much of the 17th century, chemical philosophers adopted a view of matter that was both ontologically corpuscularian and metaphysically vitalistic. In other words, these chemical philosophers adhered to a particulate matter theory while also embracing the idea that chemical qualities and operations involved the action of vital spirits and ferments.
This essay will examine these ideas by focusing on some of the more significant transitional chemical philosophies of the 16th and 17th centuries, in order to establish how chymists at this time adhered to complex corpuscularian ontologies that could not be subsumed under either a purely vitalistic or a purely mechanistic metaphysical framework.
To this end, I will focus on the chemical philosophies of Jan Baptista van Helmont, Daniel Sennert, Sebastian Basso, and Pierre Gassendi and the contributions that each of these important figures made to the subtle and graduate shift from vitalism to mechanicism.
I also hope to show that the demise of vitalistic metaphysics did not result from the victory of reductionistic mechanicism but, rather, from the physicalistic and naturalistic rationalization of chemical qualities and processes that opened the door for Lavoisier to articulate his quantitative and operational conception of simple substances.
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EXPERTS AND EXPERTISE IN NORTH-SOUTH CIRCULATION IN MID-TWENTIETH CENTURY MATHEMATICS

Session 31G

Congress section(s): B6, C1

Mid-twentieth century mathematics was decisively shaped by a variety of high-profile efforts to move mathematicians and their textual productions between global hemispheres, driven and funded by philanthropies such as the Rockefeller and Guggenheim Foundations, government efforts such as the United States’ Office of Inter-American Affairs and Fulbright program, and international organizations such as UNESCO. I shall examine the implicit and explicit cultural logics and assumptions of fellowship, exchange, and technical assistance programs linking South America to North America and Europe. In their different formulations and contexts, these programs each relied on ideas about how circulation--including both departure from and, crucially, return to one’s home country--enabled virtuous developments in local research cultures and beneficial ties between such cultures. These assumptions inflected both the high ideals and the routine practices of the various programs, evident in programmatic statements, pamphlets and advertisements, routines of intelligencing and administration, and supporting bureaucratic infrastructures. At stake were the inward- and outward-facing roles of experts and the cultural functions of expertise in the context of emerging institutional formations. Visiting experts were both producers and observers of scientific cultures in the venues they visited, both North and South, and the conditions of their circulation relied upon consequential models of convergence and exchange on the basis of both scientific and institutional expertise. This admixture of scientific and institutional knowledge reinforced funding and administrative agents’ interventionist and diffusionist understandings of scientific culture, reinforcing elite-driven models of national and international science in both old and emerging institutional establishments.

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SCHEMATISM OF HISTORICAL REALITY

Session 12L

Congress section(s): C7

The philosophy of history and the methodology of historical knowledge - traditional themes within the framework of continental philosophy. A person, reasoning about history, seeks to clarify his position history, to define his presence in it. History is not only a reality in which humanity finds itself, understands and interprets itself, but also a professional sphere on the professional sphere of acquiring and transmitting knowledge. In the 20-th century, a kind of «emancipation» of concrete historical knowledge from the conceptual complexes of the “classical” philosophy of history and from metaphysical grounds took place. In the 20-th century there was a rejection of the main ideas of modern philosophy regarding the philosophy of history: the idea of a rational world order, the idea of the progressive development of mankind, the idea of transcendental power responsible for what is happening in history, etc. Anthropologists, sociologists, historians, ethnographers played important role in the process of «emancipation» of concrete historical knowledge.

However, many questions did not receive any answer: «What is history?», «What is the historical meaning (and is there any at all)?», «What are the problems of interpretation of history and how can they be overcome?», «What are the general and special features of different types of history?».

One of the ways of contemporary understanding of history is to coordinate the schematism of historical knowledge and the structure of historical being. According to the type of co-presence described in the event communication, three schematic dimensions of historical reality are possible: spatial, situational, temporal.

The spatial schematic is presented in M. Foucault's «Words and Things». According to it, the historical is found there, and only where the spatial structure and the description of the typical method of communication of the elements of this structure are deployed.

The situational schematic of the historical takes place where a specific (moral, political, legal) nature of the connection between historical events is realized. The most important element of the situational schematic is the generation that has received an education that has left the fruits and results of its labor. Attractive in the history described in this way is the representation of historical reality as a process: historical formations, historical types, historical characters.

The temporal schematics of the historical, exemplified by M. Heidegger's phenomenological construction in «Being and Time», is found where the temporal measure of the existence of the historical being is explicated, that is, historicity is understood as the temporality of the existence of the real, and the temporality of the historical understanding itself.

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USING NORMS TO JUSTIFY THEORIES WITHIN DEFINITIONS OF SCIENTIFIC CONCEPTS

Session 14J

Congress section(s): B1, B4, B5, C3

This paper is about scientific concepts that are often thought to correspond to categories in nature. These range from widely known concepts such as SPECIES, to more specialized concepts like 2,4-DIHYDROXYBUTYRIC ACID METABOLIC PATHWAY, thought to correspond to a category to which certain synthetic metabolic pathways belong.

A typical definition of such a concept summarizes or otherwise suggests a theory about the conditions that constitute belong to the corresponding category. So these are theories that make constitution claims.

For several decades most philosophical discussions about such concepts have been metaphysical. This paper instead helps defend an epistemic thesis:

Normative conventionalism: If an agent is justified in endorsing the constitution claims from a definition of the concept C, then this stems at least in part from the constitution claims being in accord with norms about how agents ought to categorize things, and this contribution to justification is independent of any degree of correspondence between the constitution claims and supposed modal facts. (cf. Thomasson 2013)

To allow for detailed (rather than complete) defense, the paper restricts its focus to one concept, the persistent BIOLOGICAL SPECIES CONCEPT (BSC). The paper first uncovers how the BSC's typical definition (e.g., Mayr 2000) is more profoundly ambiguous than others have noted. From the definition, one can infer several extensionally non-equivalent and complex sets of constitution claims. Next the paper interprets the practices of relevant species biologists (e.g., Coyne and Orr 2004) as implicitly appealing to what have been called classificatory norms (Slater 2017) when selecting between competing BSC constitution claims. Finally, the paper argues this is wise because modal facts cannot alone tell biologists which constitution claims to endorse, and classificatory norms should help take up that slack. The conventionalism thus supported is interesting because it differs from others. It is about how to specify constitution claims for a given concept, rather than about selecting between multiple concepts (Dupré 1993; Kitcher 2001) or about when constitutive conditions are satisfied (Barker and Velasco 2013).

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INTEGRATING HPS: WHAT'S IN IT FOR A PHILOSOPHER OF SCIENCE?

Session 19L

Congress section(s): B1, B3, B6

Since the historical turn, there has been a great deal of anxiety surrounding the relationship between the history of science and the philosophy of science. Surprisingly, despite six decades of scholarship on this topic, we are no closer to achieving a consensus on how these two fields may be integrated. However, recent work has begun to identify the crucial issues facing a full-fledged account of integrated HPS (Domski & Dickson (Eds.) 2010; Schickore 2011; Mauskopf & Schamltz (Eds.) 2011). We contend that the inability to deliver on a model of integrated HPS is partly due to an insufficient appreciation of the distinction between normative and descriptive accounts of science, an over-emphasis on individual case studies, and the lack of a general theory of science to mediate between historical data and philosophical conceptions of science.

In this paper, we provide a novel solution to this conundrum. We claim that the emerging field of scientonomy provides a promising avenue for how the philosophy of science may benefit from the history of science. We begin by showing that much of contemporary philosophy of science is ambiguous as to whether it attempts to answer normative or descriptive questions. We go on to argue that this ambiguity has led to many attempts to cherry-pick case studies and hastily draw normative methodological conclusions. Against this, we claim that these two features of our reasoning should be clearly separated so that we may show how descriptive history of science may benefit normative philosophy of science. Specifically, we show that a general theory of scientific change is required to mediate between individual findings of the history of science and normative considerations of the philosophy of science. Such a general descriptive theory is necessary to avoid the problem of cherry-picking and avoid the temptation to shoehorn historical episodes into the normative confines of a certain methodology. The main aim of scientonomy is to understand the mechanism that underlies the process of changes in both theories and the methods of their evaluation. We demonstrate how a gradual advancement of this general descriptive theory can have substantial input for the normative philosophy of science and turn scientonomy into a link between the descriptive history of science and the normative philosophy of science.

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HANDLING OF DEFECTIVENESS IN A CONTENT-GUIDED MANNER

Session 26D

Congress section(s): B1

The following position will be described and defended. It implicitly answers most questions from the symposium CFP. Cases of straightforward falsification will be neglected in order to concentrate on some more difficult matters.

Two types of substantial defects may be distinguished in theories, conceptual ones and empirical ones. Concepts may be defective because they are ambiguous or vague, or because they have false or unsuitable presuppositions. Thus the concept presupposing that, at each point in time, an object is at rest in a spatial position is defective and leads to Zeno's paradoxes. Empirical defects are evoked by the fact that the involved empirical criteria are ambiguous or vague or by the presence of multiple criteria for the same predicate.
All substantial defects of theories surface as inconsistencies, a feature which may mask the nature of the defect. There is no general methodology to eliminate defects and it is possible that, in a given historical situation and even at the proverbial end of time, the best theories are defective, even unavoidably so.
Although the sources of the defects may be wildly diverse, an approach in terms of adaptive logics [1] facilitates locating the potential defects as described in the previous paragraphs. The approach considers the different potential formal logical defects: gluts or gaps in logical symbols or ambiguities in non-logical symbols. It was shown [2] that there is a recursive method for obtaining those formal logical defects, which next are minimized. The result is a minimally abnormal "interpretation" of the defective theory. Each of these interpretations connects a substantial defect to certain linguistic entities. The described method is recursive, but only after certain choices have been made. Such choices may be justified by former experience with removing defects and by properties of the involved theory.
While the described process may be considered as a general method, further steps towards removing defects require substantial and material investigation: choosing between the interpretations, determining whether the defect is conceptual or empirical, and modifying concepts or empirical criteria. So the methodology is content guided in that we "learn how to learn" [3].
The content-guided character will be underpinned by features of actual adaptive (viz. minimally abnormal) theories. These are mathematical theories, but the conclusion to be drawn is more general as well as perfectly transparent.
Pluralism enters the picture in that every defect has several potential solutions, each of which may result in a more or less viable theory. So the approach furthers a pluralism of alternatives. This is different from the epistemological pluralism that is typical for problem solving processes.
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A DISCUSSION OF BI-LOGIC AND FREUD'S REPRESENTATION THEORY IN FORMAL LOGIC

Session 9I

Congress section(s): C4

Matte Blanco introduced the Bi-logic theory, describing the symmetric and the bivalent mode, in order to explicit the logical features of the Freudian primary and secondary processes respectively. It allows for a comprehensive description of the properties of the conscious and unconscious mental representations as well.
It is possible to interpret Bi-logic in predicate logic. The idea is that variables and closed terms of first order language allow different readings of the domain of predicates, which correspond to the symmetric and bivalent mode respectively. Furthermore, we recall Freud's first characterization of representation and its manifestations: the "open" one, that is, the thing-presentation versus the "closed" one, namely the word-presentation. The interpretation in predicate logic allows for a clear illustration of the subsequent theoretical grounding of the primary and secondary process on the two aforementioned types of representations.
Since, formally, our interpretation can be conceived rethinking the notion of term, logical formalisations aiming to overcome the limitations given by such a notion could be significant for a further analysis. Very important proposals in this sense are: Gödel's modal system S4 and Girard's linear logic. In our view, the modality of S4 allows to convert the abstract element which is contained in the representation into a normative element, allowing for a logical reinterpretation of the notion of Super-ego; linear logic with its modalities would allow to describe some of the features of the unconscious thinking (such as displacement). As a further development, the features of linear logic could offer the opportunity to consider the quantitative aspects of drives.
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NUMERICAL COGNITION IN THE PERSPECTIVE OF THE KANTIAN PROGRAM IN MODERN NEUROSCIENCE

Session 28L

Congress section(s): C5

Hundreds - if not thousands - of works are devoted to the nature of the number. Meanwhile, no generally accepted and therefore acceptable understanding of the phenomenon of number and numerical cognition so far achieved. For instance, in the current Russian philosophy and psychology the concept of "numerical cognition" is virtually absent, as well as studies directly related to this kind of cognitive activity. However, the intensive development of neuroscience opens up prospects for analyzing the nature of number and the mechanisms of "numerical cognition" from the point of view of the Kantian program in neuroscience. This program stimulates this analysis by combining the principles of naturalism and sociocentrism, and allows us to look at the number as a cultural phenomenon due to the ontogenetic features of the human brain.
What are the most important features of the modern Kantian program in neuroscience? What are the (neuro) biological prerequisites for the implementation of this program? What is the "sense of number" (or numerosity) and what is the role of this "feeling" in the genesis of ideas about number and numerical cognition? What are the features of the representation of digital information in symbolic and non-symbolic form, and what is the role of language here? When and under what circumstances did the ordering of a set of numbers occur with the help of a horizontal number axis and what was the meaning of culture for this process? What are "conceptual metaphors" and what is their role in numerical cognition? Finally, how do the ontogenetic foundations of the "sense of number" and the successes (or failures) in the education of children and their future career correlate?
These questions are supposed to give some answers in the presentation.
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SEMILATTICES OF NUMBERINGS

Session 31M

Congress section(s): A1

Abstract. Uniform computations for families of mathematical objects constitute a classical line of research in computability theory. Formal methods for studying such computations are provided by the theory of numberings. The theory goes back to the seminal article of Gödel, where an effective numbering of first-order formulae was used in the proof of the incompleteness theorems. To name only a few, computable numberings were studied by Badaev, Ershov, Friedberg, Goncharov, Kleene, Kolmogorov, Lachlan, Mal'tsev, Rogers, Uspenskii, and many other researchers. Let S be a countable set. A numbering of S is a surjective map ν from the set of natural numbers ω onto S. A standard tool for measuring the algorithmic complexity of numberings is provided by the notion of reducibility between numberings: A numbering ν is reducible to another numbering μ if there is total computable function $f(x)$ such that $\nu(x) = \mu(f(x))$ for all $x \in \omega$. In other words, there is an effective procedure which, given a ν-index of an object from S, computes a μ-index for the same object. In a standard recursion-theoretical way, the notion of reducibility between numberings gives rise to an upper semilattice, which is usually called the Rogers semilattice. Rogers semilattices allow one to compare different computations of a given family of sets, and they also provide a tool for classifying properties of computable numberings for different families. Following this approach, one can formulate most of the problems on numberings in terms of Rogers semilattices. Goncharov and Sorbi [Algebra Logic, 36:6 (1997), 359–369] started developing the theory of generalized computable numberings. We follow their approach and work in the following setting: Given a complexity class, say Σ^0_n, we consider the upper semilattice $R_{\Sigma^0_n}$ of all Σ^0_n-computable numberings of all Σ^0_n-computable families of subsets of ω. We prove that the theory of the semilattice of all computable numberings is computably isomorphic to first order arithmetic. We show that the theory of the semilattice of all numberings is computably isomorphic to second order arithmetic. Furthermore, it is shown that for each of the theories T mentioned above, the Π_5-fragment of T is hereditarily undecidable. We also discuss related results on various algorithmic reducibilities.

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QUASI-TRUTH AND DEFECTIVE SITUATIONS IN SCIENCE

Session 27D

Congress section(s): B1

Quasi-truth is a mathematical approach to the concept of truth from a pragmatic perspective. It is said that quasi-truth is a notion of truth that is more suitable to current science as actually practiced; intuitively, it attempts to provide a mathematically rigorous approach to a pragmatic aspect of truth in science, where there is not always complete information about a domain of research, and where it is not always clear that we operate with consistent information (see da Costa and French 2003 for the classical defense of those claims). In a nutshell, the formalism is similar to the well-known model theoretic notion of truth, where truth is defined for elementary languages in set theoretic structures. In the case of quasi-truth, the notion is defined in partial structures, that is, set theoretic structures whose relations are partial relations. Partial relations, on their turn, are relations that are not defined for every n-tuple of objects of the domain of investigation. Sentences are quasi-true iff there is an extension of those partial relations to complete relations so that we find ourselves dealing again with typical Tarskian structures (and also with Tarskian truth; see Coniglio and Silvestrini (2014) for an alternative approach that dispenses with extensions, but not with partial relations). In this talk, we shall first present some criticism to the philosophical expectations that were put on quasi-truth and argue that the notion does not deal as expected with defective situations in science: it fails to accommodate both incomplete information as well as inconsistent ones. Indeed, it mixes the two kinds of situations in a single approach, so that one ends up not distinguishing properly between cases where there is lack of information and cases where there is conflicting information. Secondly, we advance a more pragmatic interpretation of the formalism that suits better with it. In our interpretation, however, there are no longer contradictions and no need to hold that the information is incomplete. Quasi-truth becomes much less revolutionary, but much more pragmatic.

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A FORMALISM FOR RESOURCE-SENSITIVE EPISTEMIC LOGIC

Session 6L

Congress section(s): A2

Standard epistemic logic notoriously suffers from the logical omniscience paradox, associated with overidealized deductive powers of agents. A possible solution to the paradox, based on resource-aware reasoning modeled in semilinear contraction-free substructural logics (better known as fuzzy logics), has been informally sketched by Behounek (2013). A recent formalization of fuzzy intensional semantics (Behounek & Majer, 2018) makes it possible to elaborate the proposal in detail. The proposed solution starts with distinguishing the actual, potential, and feasible (or feasibly derivable) knowledge of an agent. Logical omniscience is only troublesome for the feasible knowledge, as the potential knowledge does include all logical truths and the actual knowledge is not closed under logical deduction. Moreover, feasible knowledge is apparently a gradual notion, as longer derivations require more resources (such as time, memory, etc.), and so are less feasible than shorter ones. The gradation of feasible knowledge can conveniently be represented by means of formal fuzzy logic (Cintula, Hajek, & Noguera, 2011, 2015), whose truth values are most often interpreted in terms of gradual truth (Hajek, 1998). The fact that most fuzzy logics belong to contraction-free substructural logics makes them particularly suitable for modeling resource-awareness, and feasibility in general, since the fusion of resources can be represented by non-idempotent conjunction (Behounek, 2009). The suitability of a particular fuzzy logic for resource-aware reasoning is determined by the intended way of combining the resources - e.g., Goedel logic for maxitive resources such as erasable memory, product or Lukasiewicz logic for bounded or unbounded additive resources such as computation time, etc.

In the proposed formalism, the feasible knowledge of an agent is represented by a unary modality K in the framework of a suitable propositional fuzzy logic. The truth degree of the feasible knowledge KA of a given proposition A then represents the feasibility of deriving A by the agent from the actual knowledge. The modal axioms of standard propositional epistemic logic that express the agent's inference abilities are modified to reflect the costs of derivation: e.g., the axiom (K) of logical rationality contains an additional propositional constant c(MP) expressing the cost of applying the rule of modus ponens by the agent. The sub-idempotence of conjunction then decreases the lower bound for the truth degree of KA with each step in the derivation of A by the agent. This resolves the paradox of logical omniscience, since propositions that require long derivations are only guaranteed to have a very small (or even zero) truth degree of feasible knowability.

The apparatus of fuzzy intensional semantics (Behounek & Majer, 2018) facilitates a smooth formalization of the described resource-sensitive epistemic logic, by means of a faithful translation into Henkin-style higher-order fuzzy logic. The translation provides a syntactic method for proving tautologicity in the epistemic logic by deriving the translated formula in first-order fuzzy logic (cf. Hajek, 1998, ch. 8). The resulting formalism admits considering multiple epistemic agents, nesting the epistemic modalities, and freely combining factual and epistemic subformulae. The agents' actual and potential

knowledge can be represented in the framework as well, by using thresholds on the feasibility degrees. Additionally, the use of fuzzy logic automatically accommodates gradual propositions in the formalism.
The talk will present the details of the proposed apparatus, including a selection of its metatheoretical properties; discuss the plausibility of its assumptions and resource-sensitive modifications of epistemic principles; and examine its relation to propositional dynamic logic and other related approaches to epistemic logic.
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FEASIBLE SYNTAX, FEASIBLE PROOFS, AND FEASIBLE INTERPRETATIONS

Session 17A

Congress section(s): A3

Recursion theory – in the guise of the Entscheidungsproblem, or the arithmetic coding of the syntax of first-order theories – has been a part of symbolic logic from its very beginning. The spectacular solution of Post's problem by Friedberg and Muchnik, as well as the many examples of decidable and essentially undecidable theories found by Tarski, focused logicians' attention on the poset of Turing degrees, among which recursive sets appear as the minimal element.
Starting with the work of Cook and others on computational complexity in the 1970s, computer scientists' attention shifted to resource-bounded notions of effective computation, under which primitive recursive – in fact, elementary recursive – algorithms may be deemed "unfeasible". The threshold of "feasible computability" is reduced to polynomial-time and/or polynomial-space computations, or possibly their analogues in singly or doubly exponential times. Under this more stringent standard, for example, Tarski's decision algorithm for the first order theory of the reals is not feasible, and it took considerable effort to discover a feasible alternative.
This talk examines what happens to the classical notion of bi-interpretability when the translation between formulas, and between proofs, are required to be feasibly computable. The case of propositional logic is classical, and the extension to classical first order logic is not hard. Interesting and, I believe, open problems arise when one compares two theories with different underlying logics. The most intriguing case is when the theories do not share a common syntax, such as when one compares first order logic with the lambda calculus, or ZFC with Voevodsky's "Univalent Foundations".
The case of category theory is yet more interesting, since the "syntax" of category theory is not clearly defined. The language of category theory, as understood by the "working category theorist", certainly includes diagrams of "objects" and "arrows". We will also outline some theorems on the computational complexity of verifying the commutativity of diagrams.
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IN DEFENSE OF A CONTRASTIVIST APPROACH TO EVIDENCE STATEMENTS

Session 18C

Congress section(s): B1

This paper addresses a problem pointed out by Jessica Brown (2016) for contrastivism about evidential statements (Schaffer 2005), where evidential support is understood quantitatively (as increase of subjective probability).
Consider Situation 1: it's Friday afternoon and I need to deposit my paycheck, but nothing much hangs on that. Suppose the following Question Under Discussion (QUD) is relevant for me: "Will the bank will be open on Saturday?" and my evidence is that I drove past it two weeks ago and it was open. The sentence "My recent visit to the bank is evidence that it will be open on Saturday" is intuitively true, as well as true according to contrastivism. Consider now Situation 2: I have higher stakes. Unforeseen circumstances causing the bank to close become relevant for me. It seems that in this case the sentence "My recent visit to the bank is evidence that it will be open on Saturday" is false under the following contrastivist interpretation: "My recent visit to the bank is evidence that it will be open [rather than closed due to unforeseen circumstances] on Saturday". Yet, Brown says that this is the wrong prediction, at least under a quantitative construal of the relation of evidential support, for having driven past the bank increases the subjective probability that the bank will be open on Saturday (which is the QUD she associates with Situation 2). This seems to be a problem for contrastivism.
I wish to argue that Brown's argument is not conclusive. Think back of low-stakes Situation 1. The contrastivist can argue that, upon reflection, the QUD should be formulated in the following contrastive way: "Will the bank will be open on Saturday as per its regular schedule, rather than closed on Saturday as per its regular schedule?" It is compatible with everything Brown says that the QUD has this more sophisticated structure; indeed one could argue that careful consideration of the scenario prompts us to this formulation. A similar reformulation can be given of the QUD of Situation 2, where the stakes are higher: "Will the bank will be open on Saturday as per its regular schedule, rather than closed on Saturday due to an unforeseen change of hours?" This allows one to vindicate contrastivism. In Situation 2, the sentence "My recent visit to the bank is evidence that it will be open" is false relative to the QUD of that context, as predicted. For having driven by the bank two weeks earlier does not increase the probability that the bank will be open on Saturday as per its regular schedule rather than closed on Saturday due to an unforeseen change of hours. The contrastivist can therefore resist Brown's accusation.
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BOLZANO'S REAL NUMBERS: SETS OR SUMS?

Session 20B

Congress section(s): B6

Recently, two 19th century constructions of the real numbers have received attention: that of Frege (Snyder and Shapiro 201X) and that of Bolzano (Russ and Trlifajová 2016). While Frege's construction is explicitly placed by Epple (2003)'s conceptual scaffolding into traditional (Frege, Hankel), formal (Hilbert, Thomae) and arithmetical (Cantor, Dedekind) constructions of the reals, it is an open question how to categorise Bolzano's.
Interpreters agree that what Bolzano calls measurable numbers are in fact the reals. If we follow Bolzano literally, numbers, including thus the measurables, are sums, i.e. a certain kind of collections. This follows from the fact that, per GL and PU, numbers are quantities (GL §1) and quantities in turn are defined in terms of sums (PU §6). Hence, the definition of measurable numbers ultimately relies on the concept of sum. In the existing literature (van Rootselaar 1964, Rusnock 2000, Russ and Trlifajová 2016), however, Bolzano's measurable numbers are discussed through set theoretic interpretations. The

reasons for adopting such an interpretation are understandable, as set theory is the language of modern mathematics, and that set theoretic interpretations can thus be mathematically expedient. The downside of such choice is however that it can lead to an anachronistic understanding of the underlying philosophy of science Bolzano endorses, thus hindering efforts of placing Bolzano's measurable within a framework like Epple's.
In this talk I examine some of Bolzano's mathematical proofs and assess them in the context of Bolzano's general philosophy of science while resisting the use of set theoretic resources. Aim of this analysis is to put into starker relief the differences between Bolzano's theory of collections — sums in particular — and modern set theory, while highlighting the importance of Bolzano's conception of science for his philosophy of mathematics. By tracking the concepts Bolzano deploys in his work on the measurable numbers, we can better assess his contribution in relation to those of later mathematicians such as Frege, Dedekind and Cantor.
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CHANGE, TEMPORAL ANTICIPATION, AND RELATIVISTIC SPACETIME

Session 10J

Congress section(s): B4

In science, usually the B-view of time (according to McTaggart's famous classification) is adopted, which speaks of a static, tenseless temporal set-up. Contrasted with it is the A-view, which characterizes time by mere unstructured present, future and past. With the B-view, time is neatly integrated in four-dimensional spacetime and thus obtains a similar character as space. The notions of the present and of an intentional directedness towards a future are not part of that picture, If they are to matter, they are hoped to be derivable from the set-up of the spacetime manifold.
But several concepts which are crucial for our view of the world are foreign to the eternalist B-view and it is hard to see how they can emerge from it. First, temporal directedness cannot be based on physical laws due to the temporal symmetry of microphysics. So, commonly, emergence of temporal directedness is postulated and commonly pushed up the time scale towards sensorimotor integrative processes and the development of an internal narrative, which happen within tenths of seconds, and several seconds, respectively. Those processes, however, can be interpreted as they are only if the mind who integrates and narrates already possesses a concept of temporal anticipation. Similarly, free will, even if epiphenomenal due to the famous Libet experiments, as a notion still requires a forward-looking mental perspective. Further on, immediate perception of succession and of movement, which guides our interpreting of static series if images, requires a pre-existing concept of a present that is more than indexical as well as of temporal directedness. Finally, our indispensable concept of change remains unaccounted for by representing time as a series of time points that is not well-ordered. Even a well-ordered series mimicks progress only for the mind that has an a priori notion of change. Above four concepts mesh nicely with McTaggart's A-view of time. We have them, yet having them provides us with no advantage for survival or procreation.
We postulate:
(P) Concepts which we are strongly inclined to have and which nevertheless do not grant us any biological evolutionary advantage ought to be taken metaphysically serious.
With (P), the B-view of time turns out to be incomplete in important aspects. That suggests adopting of a purified version of McTaggart's A-view, according to which time is a parameter of change, an abstraction from the intentional forwardness of our thoughts. The B-view of time is not dismissed. Instead, (relativistic) spacetime is regarded as ontologically secondary to a set of temporal continua. Those continua ("worldlines") have directions ("future", "past") and are able mutually to intersect at earlier and later points. Intersection points define spacetime points. An axiomatic formal system characterizes intersections and leads to a construction of (relativistic) spacetime on the basis of continuous time.
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GRANGER'S PHILOSOPHY OF STYLE

Session 19D

Congress section(s): C1

I shall be laying forth Gilles-Gaston Granger's Essai d'une philosophie du style (Paris, Armand Colin, 1968; second edition 1988). This book opened a brand new field for scientific epistemology. According to the Aristotelian postulate scientific knowledge grasps only general facts. Granger's aim was to consider the following question through all angles: is formal knowledge possible for individuals? Granger deemed the answer positive, on condition of defining a new relationship between form and content for a scientific piece of work, or for a certain collection of works at a certain time. This relationship is what we call the "style" of said piece or pieces; it refers to a type of rationality, which is explained neither a priori, nor ext post by historical or factual causes. Between the content of a piece of thought (or simply: a book) and its formal structure there is an intermediate level: that of its meaning. Granger's book explores this intermediate level through a comparison with works of art and an appeal to Peirce's semiotic.

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ON THE CONSTRUCTIVE CONTENT OF PROOFS IN ABSTRACT ANALYSIS

Session 26C

Congress section(s): A1

Can a proof in analysis that does not refer to a particular constructive model of the real numbers have computational content? We show that this is the case by considering a formulation of the Archimedean property as an induction principle: For any property P of real numbers, if for all x, (x > 0 -> P(x-1)) -> P(x), then for all x, P(x). This principle is constructively valid and has as computational content the least fixed point combinator, even though real numbers are considered abstract, that is, only specified by the axioms of a real closed field. We give several applications of this principle connected with concurrent computation.

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WHAT SHOULD A NORMATIVE THEORY OF ARGUMENTATION LOOK LIKE?

Session 16L

Congress section(s): A2

What makes argumentation reasonable, rational or justified? I address this question by considering two ways of thinking of the relationship between argumentation and reasonableness/rationality/justification that mirror two very different conceptions of what a theory of argumentation should look like. As argumentation theorists, we can either aim at providing criteria for saying that a target-claim is justified, reasonable or rational, or at characterizing justification, rationality or reasonability from the point of view of the practice of arguing.
For the former group of theorists, the main question would be "should we accept this claim on the basis of those reasons?" In turn, for those interested in "characterizing" what is good argumentation, the main question is: "does this piece of argumentation count as good argumentation, taking into account the conception of good argumentation that underlies the practice of arguing?"
Both conceptions of Argumentation Theory assimilate the goals of a normative theory of argumentation to the goals of a theory of justification, but the former focuses on the conditions for considering that a target-claim is justified, whereas the latter tries to characterize the very concept of justification from the point of view of the practice of arguing. In this paper, I analyze the rewards and shortcomings of both epistemological conceptions of Argumentation Theory and their corresponding criteriological and transcendental accounts of the sort of objectivity that good argumentation is able to provide.
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FREGE AND PEANO ON AXIOMATISATION AND FORMALISATION

Session 4K

Congress section(s): A4

It is commonplace in contemporary historical studies to distinguish two traditions in early mathematical logic: the algebra of logic tradition and the tradition pioneered by Frege. Although he never defended a logicist position, Peano is usually linked to the Fregean tradition. In this talk I shall question this association. Specifically, I shall study Frege's and Peano's conceptions of axiomatisation and formalisation and conclude that they developed different accounts that were, in some respects, irreconcilable.
Peano provided the first axiomatisation of arithmetic in [1889], where he distinguished the logical principles from the set theoretic principles and the arithmetical axioms. In this regard, he departed from the algebra of logic tradition, which - in other respects - had influenced him. Most likely because of the development of his logicist project, Frege failed to acknowledge the importance of this move. At the same time, he reproached Peano the fact that he did not provide a full calculus; Peano did not define any inference rule. This is in stark contrast with Frege's presentation of the concept-script in [1893]. According to Frege, Peano could not guarantee a fully rigorous treatment of arithmetic if he did not provide the means to formalise proofs. I defend that the omission of inference rules in Peano's early works on mathematical logic is not the result of carelessness but due to his decision to conflate the conditional and the relation of logical consequence. This position echoes the practice of algebraic logicians such as Peirce or Schröder.
Frege strongly associated the notion of formalisation to the expression of content. He rejected the perspective of producing what he called an ‚abstract logic'. I defend that Frege's notion of the formalisation of a scientific theory must not be understood as a formalisation in contemporary terms. As can be attested in Carnap's notes from Frege's courses at Jena of 1913–1914 [Reck; Awodey, 2004], for Frege, the formalisation of a theory implied the use of its specific basic principles and primitive symbols, whose meaning had to remain intact. In this sense, Frege intended to use the atomic formulas of a theory as building blocks and complement them with the logical resources of the concept-script.
From the last years of the 1890 decade Peano developed a notion of formalisation that was in direct opposition to Frege's. In [1901] Padoa, a disciple of Peano, described a formalised theory as system whose primitive propositions were understood as conditions and whose primitive symbols did not have specific meaning but were intended to have multiple interpretations. The formalisation of a theory was thus not intended to preserve its content and express it in a rigorous way, as Frege defended. Peano aimed at answering metalogical questions such as the independence of the axioms of a theory. In this context, his perspective can again be associated with the algebra of logic tradition.
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TRANSCRIPTIONS OF GOTTLOB FREGE'S LOGICAL FORMULAS INTO BOOLE'S ALGEBRA AND LANGUAGE OF MODERN LOGIC. SIMILARITIES AND DIFFERENCES

Session 3K

Congress section(s): A4

My aim is to rewrite some of Frege's important formulas from his Begriffsschrift (1879) into other logical languages in order to do a comparison and stress some differences between the languages. I refer to natural language expressions if they were given by Frege.
The languages into which I do the translation are the following:
1. G. Boole's algebra language presented in Laws of Thought (1854).
2. E. Schröder's logical language: "in a form modelled upon Leibnizian-Boole's calculus" as Schröder wrote.
3. The language of modern logic.
As an introduction I make some historical remarks about the relationship Frege – Boole and present a short description of logic understood as a calculus (Boole, Schröder) and logic as a language (Frege).
I posed the following questions:
1. Which of the logical transcriptions is the closest to Frege's verbal expression related to a particular formula?
2. Is there a really proper translation of Frege's logical notation into Boole's algebra, Schröder's logic, or the language of modern logic?
3. Is Frege's logic more understandable when it is expressed in Boole's algebra, in Schröder's logic or in the language of modern logic?

4. Isn't there a risk that the very precise language, which was meant to ensure extreme formalisation of reasoning, can lead to esoteric knowledge for a small group of specialists, and consequently discourage potentially interested people?
Although Frege wrote two papers comparing Boole's algebra and his logic, Frege didn't do a transformation of his formulas into Boole's algebra logic. Schröder was inspired by Boole's algebra and invented his own algebra of logic. Schröder transformed some of Frege's formulas into his language. I am trying to continue Schröder's project to rewrite some of Frege's important formulas into other logical languages.
Frege's notation is two-dimensional as opposed to Boole's algebra, Schröder's logical language and the language of modern logic which are linear notations. In Frege's logic there is a very important distinction between assertion (that is, the judgement-stroke) and predication (that is, the content stroke). It is not possible to express the same in Boole's algebra, in Schröder's logical language or in the language of modern logic. Frege's sentence "it is judged as true" is replaced by the name "tautology" and called 1. There are different Frege's verbal descriptions of this law, as tautology and countertautology. However, they express the same thought in different ways. In notation used by the language of modern logic with an implication it is necessary to use brackets.
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BOLZANO'S THEORY OF GROUND AND CONSEQUENCE AND THE TRADITIONAL THEORY OF CONCEPTS

Session 17B

Congress section(s): B6

As known, Bernard Bolzano (1781–1848) was the first to offer a formally sophisticated account of (objective) explanation (Abfolge or grounding) in his Wissenschaftslehre (1837). Grounding is a relation between (collections of) truths Q and their objective reason or ground P, where P in some objective sense explains Q. Bolzanian grounding can be said to impose a hierarchy on truths: grounds are in some sense more fundamental than, and thus prior to, the truths that they ground, i.e. their consequences.
As of today, it remains an open question under which conditions exactly Bolzano holds that a (collection of) truth(s) is the ground of another. State-of-the-art reconstructions of (sufficient conditions for) Bolzano's grounding are given as deductive arguments satisfying certain conditions of simplicity and economy (cf. e.g. Roski & Rumberg 2016). Unfortunately, such this and similar reconstructions disregard several of Bolzano's claims about grounding, such as the requirement that a ground be at least as general as its consequence.
In this talk we put forward an alternative account of grounding that does justice to Bolzano's claims. We argue that a correct interpretation of Bolzano's views on explanation must take into account Bolzano's envisioning of a hierarchical ordering not only among the truths, but also among the concepts which make up a science. Such an hierarchy of concepts is a substantial part of the tradition of thinking about science, originating from Aristotle's Analytica Posteriora, which heavily influenced Bolzano's ideal of science (de Jong & Betti 2008, de Jong 2001). According to this traditional conception, a science consists of some fundamental concepts, and all other concepts are defined from them according to the well-known model of definitions per genus proximum et differentiam specificam. Concepts, accordingly, are on this conception hierarchically ordered as genus and species. We will argue that the hierarchical ordering that grounding imposes on truths in Bolzano's view emanates from the hierarchical ordering of the concepts which make up those truths.
We will show that only by taking into account the traditional theory of concepts, including the age-old doctrine of the five so-called praedicabilia, can one account for Bolzano's requirements for grounding in a satisfactory manner. We further strengthen our case by showing that our interpretation can account for certain other, general aspects of Bolzano's thinking about science, such as the reason why in Bolzano's view sciences consist of synthetic truths only. One consequence of our account is that Bolzano's attitude to the traditional theory of concepts turns out to be less "anti-Kantian" than usually maintained (cf. e.g. Lapointe 2011).
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TARSKI'S TWO NOTIONS OF CONSEQUENCE

Session 5K

Congress section(s): A4

In the 1930s Tarski introduced two notions of consequence. We present and compare them. We also examine their relations with similar notions presented by others.
The first notion is connected with the consequence operator theory and was presented in (Tarski 1928, 1930a,b). The second one is based on the concept of model and was presented in (Tarski 1936a,b,c, 1937).
We argue that it is misleading to understand and qualify the first as a syntactic or proof-theoretical notion. A more appropriate qualification is "abstract consequence". The word "abstract" has indeed been later on used by Suszko for his theory of abstract logics, a continuation of the consequence operator theory (see Brown and Suszko, 1973).
Regarding the second notion, we point out that besides Bolzano, already notified by Scholz (1937), other people had similar ideas, in particular Abu'l-Barakāt (see Hodges 2018) and Wittgenstein (1921, 5.11). And we compare this notion, in particular using (Corcoran-Sagüillo 2011), with the one later on developed in model theory by Tarski himself (1954-1955).
We discuss the relations between the two notions, emphasizing that the model-theoretical one is a particular case of the consequence operator theory one, and discussing fundamental features of them that can be used to prove a general completeness theorem, following the line of Gentzen's work (1932) about Hertz's Satzysteme (1929), framework connected with the consequence operator.
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ABDUCTIVE INFERENCE AND SELECTION PRINCIPLES

Session 14I

Congress section(s): B2

Abductive inference appears in various contexts of cognitive processes. Usually, the two prominent uses of abduction with respect to the explanatory hypotheses in question are distinguished: a) the context of discovery (or hypothesis-generation/formulation); and, b) the context of justification (or evidential support). Notwithstanding the other uses of abduction (e.g. computational tasks), I pay close attention to an overlooked context of abductive inference: c) the context of explanatory selection.
I propose to distinguish these three kinds of explanatory inferences explicitly by using a notion of a selection principle. The selection principle is optimally construed (or modelled) as a partial function defined on a (non-empty) set of (explanatory) hypotheses with respect to an explicit piece of evidence E and background B comprising doxastic, epistemic and axiological items. If a given selection principle operates on an admissible n-tuple of arguments, it yields at most one explanatory hypothesis (or its content-part) as a function-value. Having the notion of selection principle at our disposal, it is possible to make the difference among those three contexts of the use of abduction completely explicit. In particular, I argue for distinguishing the three kinds of selection principles operating in these contexts. These kinds of principles differ both with respect to the arguments they operate on, and to the function-values they yield.
Moreover, I provide explicit reasons for identifying inference to the best explanation (henceforth “IBE”) only with abductive inference in a justificatory context of reasoning. As a consequence, I show that, at least, some widely-discussed objections against IBE in the literature (such as van Fraassen’s (1989) argument from a bad lot) are not relevant to other forms of abductive inference in the context of discovery or the context of explanatory selection. Hence, such a clarification of different selection principles underlying different contexts of abduction appears to be fruitful for re-considering the question of which traditional objections against IBE are also objections against abductive inference in general.
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COMMON BELIEF LOGICS BASED ON INFORMATION

Session 28D

Congress section(s): A2

Substructural epistemic logics present an example of formal models of beliefs of rational agents, when perspective switches from a traditional, epistemic alternatives based semantical approach, to information based approach. In particular, we can interpret beliefs as based on available information or reasonable expectations, and capture them via diamond-like modalities interpreted over information states or probability distributions. In the former case, the corresponding notion of belief is that of confirmed-by-evidence belief. A logical account of belief along these lines needs to take into account inconsistencies and incompleteness of information, or uncertainty how likely an event is, based on the evidence locally available to agents. This naturally leads us to use and study in general modal extensions of non-classical logics such as substructural, paraconsistent or many-valued (Belnap-Dunn four-valued logic and Lukasiewicz logic especially). Particular examples of such epistemic logics have been investigated as modal extensions of distributive substructural logics [1,3].
As we think that understanding the notion of common belief is crucial to any formalization of group beliefs and their dynamics, the aims of this contribution is to present common belief extensions of some epistemic logics based on information states semantics, and prove their completeness. We will consider Hilbert style axiomatizations of those (both of finitary and infinitary nature), where common belief is formalized as a greatest fixed point expression.
To approach the completeness problem we in particular use two different insights of which we provide theoretical accounts: one coming from abstract algebraic logic, the other from coalgebraic logic: First, to prove the strong canonical completeness of the infinitary versions of the logics we will use a proper version of extension lemmata such as Lindenbaum or Belnap’s pair-extension lemma. A general abstract algebraic perspective at both lemmata for infinitary logics, widening the area of their applicability beyond modal extensions of classical logic, and pointing at their limits, is given in [2]. Second, understanding the frame semantics of logics we consider as given by coalgebras, and generalizing insights available in flat fixed point coalgebraic logics based on classical logic, we prove the completeness of the finitary axiomatization of the logics.
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EXTERNAL VALIDITY AND FIELD EXPERIMENTS IN ECONOMICS

Session 17F

Congress section(s): C7

In social science, external validity is used to mean the extent to which inferences drawn from observational or experimental studies can be generalized to other contexts of interest. Generalizing inferences from a study sample to another population

or context includes an inductive gap: what may hold in the model may not hold in the target. Thus, external validity is a concern for any experimental or observational study that aims at results and inferences that are applicable outside the experimental setup.

In economics, analysis of "external validity" varies within subdisciplines, but focuses, broadly speaking, on detecting the factors that affect the generalizability of inferences as well as developing methods for reliable generalization (cf. List and Levitt 2007). Philosophy of social science usually takes external validity as synonymous with extrapolation and focuses on conceptual and methodological analysis in addition to constructing theoretical frameworks for extrapolation (Guala 2005; Jímenez-Buedo 2011; Steel 2007, 2010). Some argue that the concept of external validity should not be used at all (Reiss 2018).

Because the concept is regularly used by social scientists, abandoning it is not a fruitful choice. Instead, extrapolation should be understood as the inductive process of transporting inferences from the model to the target, and external validity as a set of methodological criteria that are used to evaluate the validity of this process. These criteria vary within different fields' epistemic and practical concerns. In econometrics, external validity is quantitatively measured to evaluate the accuracy of causal predictions. In behavioural and experimental economics, on the other hand, external validity is instead used as a guiding principle in designing experimental studies.

I use field experiments as cases to illustrate the ways in which the concept of external validity is used in experimental and behavioural economics. Studying external validity and extrapolation in specific fields highlights the relationship between the domain-specific concerns and the general issue of extrapolating causal inferences. It shows how methodological practices are used to understand and handle general issues of inductive reasoning. I conclude that external validity is a useful, if incomplete criterion of assessing extrapolation, and that a better understanding of extrapolation complements the analysis of external validity in philosophy of science.

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ON HOW DESCARTES CHANGED THE MEANING OF THE PHYTAGOREAN THEOREM

Session 25J

Congress section(s): C1

1. Euclid's Elements proposition I.47 includes the ancient Greek version of the Pythagorean theorem (PT), namely: "In a right-angled triangle, the square on the side subtending the right-angle is equal to the [sum of] squares on the sides surrounding the right-angle". While the interpolation "sum of" characterizes modern translations, in the Greek mathematics, instead of reference to "sum of" figures, there was rather a reference to figures themselves, like in the phrase: "the square [...] is equal to the squares" (see [2] and [3]).

The proof of the proposition I.47 requires a theory of equal figures designed to compare non-congruent figures. The theory starts with the proposition I.35 and culminates in the construction of squaring a polygon, offered in the proposition II.14. As for its foundations, it builds on axioms named Common Notions, and on the so called geometrical algebra, developed throughout Book II. In modern mathematics, all these principles are covered by axioms for an ordered field. In [4] Hilbert shows that Euclid's theory of equal figures can be developed on a plane F×F, where F is an order field (Archimedean or non-Archimedean) closed under the square root operation. In sum, the Greek version of PT can be represented by the following scheme □,□=□, while the underlying theory is that of equal figures.

2. In modern mathematics, PT is represented by the algebraic formula $a^2+b^2=c^2$, where a, b, c stand for real numbers that are measures (lengths) of sides of a right-angled triangle. In this case, the underlying theory is the arithmetic of real numbers accompanied by a tacit assumption that every segment is characterized by a real number; indeed, [4] provides hints how to prove that claim, rather than a complete proof.

3. We show that [1] includes a third version of PT that is in between the ancient and modern one. While Descartes for the first time in the history applied the formula $a^2+b^2=c^2$, with a, b, c representing sides (line-segments) of a right-angled triangle, the underlying theory is the arithmetic of segments rather than arithmetic of real numbers. Surely, Descartes provides no explicite discussion of that new version of PT, while the very formula is applied implicitly, via references to diagrams. We show that although Descartes' development is founded on Euclid's theory of similar figures (implicitly on the Archimedean axiom), when it comes to the field of segments it can be characterized as a real closed field (Archimedean or non-Archimedean).

4. We discuss the question of unity of PT: on what grounds these three versions as developed in three different mathematical contexts, i.e. theory of equal figures, arithmetics of segments, and arithmetic of real numbers, represent one and the same theorem. We argue that the implicit unity rests on a diagram representing a right-angle triangle.

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DID BOLZANO SOLVE THE EIGHTEENTH CENTURY PROBLEM OF PROBLEMATIC MATHEMATICAL ENTITIES?

Session 18B

Congress section(s): B6

During the eighteenth century, mathematics was widely regarded as the paradigmatic example of apodictic knowledge, especially by the influential philosophers Wolff and Kant. The new mathematical inventions of the seventeenth century, like infinitesimals, were considered as tools to solve problems in the natural sciences rather than as proper mathematical objects that might be the starting point of new mathematical subdisciplines. While mathematics was slowly developing itself into a field independent of its applications, the philosophy of mathematics was still dominated by Wol ,s mathematical method, which is modeled after Euclid's Elements. At the end of the eighteenth century, several minor figures in the history of mathematics and philosophy, such as Michelsen, Langsdorf and Schultz, attempted to reintegrate the mathematical developments of the seventeenth and eighteenth century into the philosophy and epistemology of their time. An important part of their publications is devoted to the manner in which problematic mathematical entities such as infinitesimals and complex numbers should become part of mathematics.

In his early Contributions to a better founded presentation of mathematics of 1810, Bolzano responded to these issues by proposing a much wider conception of mathematics by rejecting the traditional definition of mathematics as the study of quantities. Notes and manuscripts of 1811 and 1812 confirm this radical departure from the tradition. Three decades later, Bolzano returned to the traditional conception of mathematics and offered a solution to the problem of problematic mathematical entities by allowing objectless ideas. While the early Bolzano responded radically to this problem by developing a quite general conception of mathematics, the later Bolzano much more carefully formulates a slightly broader conception of quantity and combines this with a quite advanced epistemology that allows objectless ideas to be meaningful under certain conditions. As a result, the later Bolzano seems to hold that a scientific (sub)discipline can have objectless ideas as its topics of study.

In this paper, I will attempt to answer the following question: why and how did Bolzano change his position and in what manner does this change relate to other developments in the philosophy of mathematics during the first decades of the

nineteenth century? To this end, I will first summarize the issues concerning several problematic mathematical objects as they were discussed at the end of the eighteenth century. Subsequently, I will sketch Bolzano's early and late solutions to these problems. Most of the paper will be devoted to an investigation into Bolzano's notes and manuscripts in order to attain an understanding of why and how Bolzano's changed his solution. Finally, I will compare Bolzano's approach to responses of his contemporaries.

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TWO TYPES OF UNREALISTIC MODELS: PROGRAMATIC AND PROSPECTIVE

Session 12I

Congress section(s): B1

The purpose of this paper is to introduce and assess a distinction among unrealistic models based on the kind of idealizations they resort to. On the one hand, programatic models resort to idealizations that align with the core commitments of a research program. On the other hand, prospective models resort to idealizations that challenge those core commitments. Importantly, unrealistic models are not intrinsically programatic or prospective. Rather, their status is dependent on an interpretation of the idealizations.

Idealizations are features of models that make them different from - typically simpler than - the target phenomena they represent. However, these features become idealizations only after two stages of interpretation performed by the user of the model. First, there is a non-referential interpretation of the model's vehicle. In this stage, the user decides which features instantiated by the vehicle are those that the model is going to exemplify. These features are conceptualised in accordance with the contingent commitments of the user. These features are the bearers of the idealizations-to-be. Second, there is a referential interpretation of the features exemplified by the model. In this stage, the user assigns features exemplified by the model to features of the target phenomenon. In the assignment, exemplified features of the model are evaluated as more or less idealized representations of their denotata. Such evaluation is decided by standards and other epistemic commitments held by the user of the model.

Idealizations, as the product of a user's interpretation, can align with or challenge core commitments of research programs in both stages of interpretation. First, non-referential interpretations can conflict with accepted selections of features that a model exemplifies or with the accepted conceptualizations of such features. Particularly salient are explanatory commitments, which can decide which conceptualizations are legitimate within a research program. Second, referential interpretations can conflict with accepted standards for assignment. Explanatory commitments are also relevant in deciding these standards.

I continue to argue that programatic and prospective models typically aim for distinct epistemic achievements. On the one hand, programatic models aim for how-plausibly explanations, while prospective models aim for how-possibly explanations. However, I contend that how-plausibly and how-possibly explanations should not be regarded as explanations in the traditional sense, but rather as distinct forms of understanding. Thus, programatic and prospective models share a common, although nuanced, aim: the advancement of understanding.

I test this account in a model case study (Olami, Feder, Christensen, 1992). This model is a cellular automaton computer model that simulates aspects of the behaviour of earthquakes. I show how different explanatory commitments, namely mechanistic and mathematical explanatory commitments, align with and challenge core commitments of distinct research programs. I also explore how these commitments lead to distinct understandings of the target phenomenon.

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NEGOTIATING HISTORY: CONTINGENCY, CANONICITY, AND CASE STUDIES

Session 10A

Congress section(s): B1, B6

Recent work on the use of historical case studies as evidence for philosophical claims has advanced several objections to this practice. Our two-fold goal is first to systemize these objections, showing how an appropriate typology can light the path toward a resolution, and second, to show how some of these objections can be recast as advantages for the historically sophisticated philosopher, specifically by describing how attention to contingency in the historical process can ground responsible canonicity practices.

Systematizing objections to the use of historical case studies for philosophical ends shows that they fall largely into two categories: methodological objections and metaphysical objections. The former, we argue, fail to be distinctive—they do not identify special challenges from other forms of philosophical reasoning are immune. Case studies demand responsible handling, but this is unsurprising. History is messy and philosophy is difficult. But the need for care is hardly the mark of a hopeless endeavor. Rather, attention to the ways in which history is messy and in which philosophy is difficult can be resources for developing better historiographical and philosophical practices.

Metaphysical objections do, however, raise special problems for the use of historical case studies. We show that attention to what makes for a canonical case can address these problems. A case study is canonical with respect to a particular philosophical aim when the philosophically salient features of the historical system provide a reasonably complete causal account of the results of the scientific process under investigation. We show how to establish canonicity by evaluating relevant contingencies using two prominent examples from the history of science: Eddington's confirmation of Einstein's theory of general relativity using his data from the 1919 eclipse and Watson and Crick's determination of the structure of DNA. These examples suggest that the analogy between philosophical inquiry and the natural sciences, although imperfect, has important elements that make it worth retaining. This is not to say that we should think of philosophy as modeled on scientific practice, but rather that both succeed by virtue of something more general: their reliance on shared principles of sound reasoning.

Taking seriously the practices necessary to establish the canonicity of case studies makes clear that some examples of the historical process of science are more representative of its general ethos than others. With historiographical sense, we can pick these examples out. Doing so requires attention to the contingencies of history. Rather than undermining the use of historical cases, philosophical attention to contingency aids the development of case studies as resources by making explicit otherwise tacit assumptions about which features of them are most salient and why.

These considerations help us address the question of the value of history of science for the philosophy of science. It is possible, even easy, to use the rich resources that history provides irresponsibly to make a predetermined point. But that is not a genuine case of history of science informing philosophy of science—in part because it proceeds in the absence of historiographical sense. By outlining the practices that render particular cases canonical for certain philosophical aims, we have offered a route by which such sense can be integrated into standard philosophical practices.

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HOW SCIENTISTS ARE BROUGHT BACK INTO SCIENCE – THE ERROR OF EMPIRICISM

Session 25E

Congress section(s): B1, C8, C9

This paper aims at a contribution to critically investigate whether human-made scientific knowledge and the scientist's role in developing it, will remain crucial – or can data-models automatically generated by machine-learning technologies replace scientific knowledge produced by humans?
Influential opinion-makers claim that the human role in science will be taken over by machines. Chris Anderson's (2008) provocative essay, The End of Theory: The Data Deluge Makes the Scientific Method Obsolete, will be taken as an exemplary expression of this opinion.
The claim that machines will replace human scientists can be investigated within several perspectives (e.g., ethical, ethical-epistemological, practical and technical). This chapter focuses on epistemological aspects concerning ideas and beliefs about scientific knowledge. The approach is to point out epistemological views supporting the idea that machines can replace scientists, and propose a plausible alternative that explains the role of scientists and human-made science, especially in view of the multitude of epistemic tasks in practical uses of knowledge. Whereas philosophical studies into machine learning often focus on reliability and trustworthiness, the focus of this chapter is on the usefulness of knowledge for epistemic tasks. This requires to distinguish between epistemic tasks for which machine learning is useful, versus those that require human scientists.
In analyzing Anderson's claim, a kind of double stroke is made. First, it will be made plausible that the fundamental presuppositions of empiricist epistemologies give reason to believe that machines will ultimately make scientists superfluous. Next, it is argued that empiricist epistemologies are deficient because it neglects the multitude of epistemic tasks of and by humans, for which humans need knowledge that is comprehensible for them. The character of machine learning technology is such that it does not provide such knowledge.
It will be concluded that machine learning is useful for specific types of epistemic tasks such as prediction, classification, and pattern-recognition, but for many other types of epistemic tasks —such as asking relevant questions, problem-analysis, interpreting problems as of a specific kind, designing interventions, and 'seeing' analogies that help to interpret a problem differently— the production and use of comprehensible scientific knowledge remains crucial.
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ON THE POSSIBILITY AND MEANING OF TRUTH IN THE HISTORICAL SCIENCES

Session 11A

Congress section(s): B4

The familiar challenges to historiographical knowledge turn on epistemological concerns having to do with the unobservability of historical events, or with the problem of establishing a sufficiently strong inferential connection between evidence and the historiographical claim one wishes to convert from a true belief into knowledge. This paper argues that these challenges miss a deeper problem, viz., the lack of obvious truth-makers for historiographical claims. The metaphysical challenge to historiography is that reality does not appear to co-operate in our cognitive endeavours by providing truth-makers for claims about historical entities and events. Setting out this less familiar, but more fundamental, challenge to the very possibility of historiography is the first aim of this paper. The various ways in which this challenge might be met are then set out, including ontologically inflationary appeals to abstract objects of various kinds, or to "block" theories of time. The paper closes with the articulation of an ontologically parsimonious solution to the metaphysical challenge to historiography. The cost of this approach is a revision to standard theories of truth. The central claim here is that the standard theories of truth have mistaken distinct causes of truth for truth itself. This mistake leads to distorted expectations regarding truth-makers for historiographical claims. The truth-makers of historiographical claims are not so much the historical events themselves (for they do not exist) but atemporal modal facts about the order of things of which those events were a part.

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CONSTRAINING THE UNKNOWN

Session 5I

Congress section(s): B1, B7, C2, C9

Science sometimes proceeds by straightforward empirical tests in which a concrete hypothesis yields a specific quantitative prediction that can be checked against empirical results. This strategy is ill-suited to epistemic contexts in which so little is known about the phenomenon under investigation that specific hypotheses are not forthcoming. Contemporary research in cosmology on the nature of dark energy is an example of just such a context. I will argue that theorizing about dark energy is constrained by putting bounds derived from empirical results on parameterized families of unspecified models of dark energy. This strategy allows researchers to efficiently prune away large swaths of model space, without having to articulate particular proposals regarding the nature of dark energy. Characterizing and appreciating this strategy is valuable because it allows us to understand one of the variety of ways in which scientists make empirical progress in fields where researchers currently know very little about their targets, and thus precisely where there is much new ground to be gained. Moreover, familiarizing ourselves more intimately with this strategy has the potential for significant impact on the accuracy and sophistication of the public's understanding of how science works.
The epistemology and methodology of science to which the science-interested public has access rarely escapes the long shadow of the digestible although implausible popular caricature of Popper's falsificationist philosophy of science. Those physicists who do charitably reference philosophy of science at all, are notorious perpetuators of this mythology about scientific methodology. Even worse, the caricature of Popperian falsifiability is trotted out in public debates as a demarcation criterion precisely when questions about what ought to count as properly scientific methods is a matter of active dispute---that is, precisely when the integrity of science is vulnerable. In cosmology, the question of whether or not "non-empirical confirmation" ought to be recognized as a legitimate aspect of scientific methodology has become a matter of public dispute, or rather a "battle for the heart and soul of physics" according to George Ellis and Joe Silk, who worry that if the battle is lost—if non-empirical confirmation is anointed as scientific—then science will be less well equipped to defend itself from the likes of climate change skeptics. I agree with Ellis and Silk, that something very central and important to the project of scientific inquiry would be lost by endorsing non-empirical confirmation. I also want to stress that it is misguided to defend the special epistemic status of science by relying solely on the Popperian caricature. Therefore, my analysis of contemporary

research in dark energy serves two main points, one parochial/methodological and one synoptic/normative. First, cosmologists are learning about the nature of dark energy in a clever way—without hypotheses—and we should update our conception of scientific methods accordingly. Second, we ought to stop defending the integrity of science to the public by appealing to Popper alone, because doing so fails to emphasize what genuinely does differentiate science from other pursuits and incriminates perfectly good cutting-edge empirical research.

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MODELING BELIEF BASE DYNAMICS USING HYPE SEMANTICS

Session 6L

Congress section(s): A2

Mainstream approaches to dynamic doxastic logics modeled within the DEL paradigm use possible worlds semantic. They approach belief modality as a diamond-like modal operator over epistemic alternatives. However, application of such semantics in doxastic or epistemic contexts results in highly idealized epistemic agents. To address some of these concerns about dynamic doxastic frameworks, I present a new framework with a focus on information states as the basis of beliefs, both at the static level of belief formation and at the level of belief dynamics. I use an extended version of the HYPE model [1], with a preference ordering on the subsets of the situations space, and a binary belief relation between situations, to support a static belief modality and the dynamic belief change operators. On the static aspect, the pieces of information are denoted by propositions supported at situations, and the model explicitly represents possibly inconsistent and incomplete collections of information using sets of situations. The static belief operator is a non-monotonic and paraconsistent modality, which leads to a consistent belief set at any situation for any collection of information. On the dynamic aspect, I present dynamic operators for belief revision and belief contraction, with their duals. The dynamic operators lead to new models, by set theoretic expansion of the total information of the agent with the trigger information and reordering of preferences for successful revision and contraction. The dynamic operators may lead to a number of new models, hence the duals for each operator. By modeling the changes of belief as consequences of the changes of the information of agents, the model meets expectations of belief change such as withdrawal of the beliefs that are merely inferred from an old belief, as result of revision or contraction. The dynamic belief base model shares some motivations and aspects with evidence models [2], epistemic models for sceptical agents[3] and theories of belief base[4].

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BRIDGING THE GAP BETWEEN SCIENCE AND PUBLIC THROUGH ENGINEERING ENVIRONMENTAL CONCEPTS

Session 8B

Congress section(s): C2

Drawing upon Haslanger’s work on the concepts of gender and race [2] as well as (rapidly) developing research program on conceptual engineering [1] we explore the possibility that skepticism regarding climate change could be addressed by targeted engineering of controversial concepts. Expressions like “global warming” and “climate change” which are central in debates about the current and the future state of our environment are politically/ideologically charged [3][4]. This we believe makes effective communication and public discourse at best difficult and at the worst impossible which in turn only exacerbates exaggerated skepticism toward scientific results about climate change.
Building on Haslanger’s idea [2] that we should define our concepts so that they serve best whatever purpose they are meant to serve, combined with the broader argument from conceptual engineering which calls for examination and revision of “defective” concepts we suggest that those environmental concepts which hinder effective communication may be defective, and if so, should be revised [1].
On Haslanger’s analysis [2] we are to consider the pragmatics of the way we employ the relevant concepts (rather than attempting to explicating them). We should consider what practical (or cognitive) task they should enable us to accomplish and whether they are effective tools for accomplishing them. If the concepts under investigation are not adequate tools they are to be revised or replaced.
One way to approach this revision is through conceptual engineering. Cappelen [1] defines conceptual engineering as an activity concerned with how to assess and improve concepts/representational devices. Some of the central questions for this activity concerns how we can assess the adequacy of our representational devices and what strategies are available for amelioration. Using this as a platform we could assess and seek to ameliorate those environmental concepts which are most often disputed in public continuously threatening to increase distrust in scientific results. Such concepts may include climate change itself (specifically in the context of the move from ‘global warming’ as the central concern of environmentalists, to ‘climate change’. More generally, the concepts of ‘environment’ and ‘nature’ are open to question. We hypothesize that skepticism regarding climate change may be attributable at least in part to justified suspicion on the part of the public, to the meaning of the concepts commonly employed in this discourse.

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TEACHING CONCEPTUAL CHANGE: CAN BUILDING MODELS EXPLAIN CONCEPTUAL CHANGE IN SCIENCE?

Session 13D

Congress section(s): B7

This paper considers how novel scientific concepts (concepts which undergo a radical conceptual change) relate to their models. I present and discuss two issues raised by respectively Chin and Samarapungavan (2007) and Nersessian (1989) about perceived (and persistent) difficulties in explaining conceptual change to students. In both cases models are either seen as secondary to concepts/conceptual change or seen as inessential for explanation. Next, I provide an example which to some extent counters these views. On the basis of that example I suggest an alternative view of the role of models in conceptual change and show that the latter could have beneficial implications for teaching conceptual change. The example in question is Robert Geroch's modeling of Minkowski spacetime in Relativity from A to B (1981).

It seems reasonable to think that understanding the conceptual transformation from space and time to spacetime first, makes it easier to build a model of spacetime. This is the underlying assumption that Chin and Samarapungavan make (2007). Their objective is to find ways to facilitate conceptual change because they see the lack of understanding of the conceptual change that produced the concept as the main obstacle for students' ability to build a model of it. I argue that this is not necessarily the case: in certain cases (spacetime for example) building the model can facilitate understanding of the conceptual change. In a similar vein, although understanding how scientific concepts developed can often give clues for how to teach them I argue that in some cases the historical approach is counterproductive. Nersessian argues that the same kind of reasoning used in scientific discovery could be employed in science education (Nersessian, 1989). I essentially agree with this view but with a caveat. I argue that in some cases the historical approach might be constraining and in particular that the spacetime example shows that ignoring the historical path in certain cases is more successful.

Additionally Geroch's way to model spacetime can be of consequence for teaching relativity and quantum mechanics to high school students. Physics is traditionally taught through solving equations and performing experiments which is ill suited for relativity and quantum mechanics. Norwegian curriculum requirements include that students be able to give qualitative explanations as well as discuss philosophical and epistemological aspects of physics. According to ReleQuant (University of Oslo and the NTNU project on developing alternative learning resources for teaching relativity and quantum mechanics to high school students) this opens the door for introducing qualitative methods in teaching high school physics. The conclusion that ReleQuant draws from this is that historical approaches may be profitable when teaching quantum physics on the high school level.

The historical approach might not always be effective – as it is not in teaching spacetime. Teaching through building a model "from scratch" might work better. Building a model from with no or little reference to theory could be viewed as a qualitative method and would essentially be in agreement with the overall ambition of the ReleQuant project.

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INDISCERNIBILITY AND RIGIDITY IN BANACH SPACES

Session 17I

Congress section(s): A1

The concepts of indiscernibility and rigidity in Banach spaces acquire several formal meanings in the different branches of mathematics. We will discuss some of these possibilities in the context of set theory and of Banach spaces and how they interact.

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OPEN-ENDED QUANTIFICATION AND CATEGORICITY

Session 4M

Congress section(s): A2

Consider the following two facts about classical first order logic (FOL): first, due to the Löwenheim-Skolem theorem, a countable first order theory with an infinite model cannot determine it uniquely up to isomorphism (thus, it allows for models of different infinite cardinalities) and, secondly, as proved in [Carnap 1943], the formal rules and axioms for the propositional operators and quantifiers do not uniquely determine their meanings (thus, there are models of FOL in which the propositional operators and quantifiers have the normal meanings and models in which they have a non-normal meaning; for instance, in a non-normal model for FOL, a sentence and its negation are both false, while their disjunction is true, and a quantified sentence "(∀x)Fx" is interpreted as "every individual is F, and b is G", where "b" is an individual constant). Each of these two facts shows that FOL is non-categorical. Nevertheless, since the operators and quantifiers may have a normal meaning in all non-isomorphic models that are due to the Löwenheim-Skolem theorem, while they have different meanings in the non-isomorphic models associated with Carnap's results, the two notions of categoricity should not be confused with one another. But more importantly, what is the relation between these two distinct notions of categoricity? This question becomes even more interesting if we consider a recent argument advanced by [Van McGee 2000, 2015] for the idea that if the formal natural deduction rules of FOL are taken to be open-ended (i.e., they continue to hold even if the language expands), then they uniquely determine the meanings of the logical terms that they introduce and, consequently, the universal quantifier should be taken as ranging over absolutely everything, rather than over a subset of the universal set. This would then seem to imply, against the Löwenheim-Skolem theorem, that all models of a countable first order theory with an infinite model have the same cardinality – that of the universal set.

I argue in this paper that if McGee's open-endedness requirement succeeds to uniquely determine the meanings of the quantifiers, then it simply eliminates the non-normal models discovered by Carnap, without succeeding in uniquely determining the range of the quantifiers in a countable first order theory with an infinite model. The argument goes like this. Let us call LS-categoricity the property that FOL lacks due to the Löwenheim-Skolem theorem, and C-categoricity the property that FOL lacks due to Carnap's results. I argue that even if FOL were C-categorical, then it can still be non-LS-categorical, because the universal quantifier can have the same unique meaning in all the non-isomorphic models due to the Löwenheim-Skolem theorem. In addition, I argue that open-ended FOL is not C-categorical, for McGee's open-endedness requirement does not succeed in uniquely determining the meanings of the propositional operators and quantifiers. The reason for this is that his crucial assumption, that for any class of models there is a sentence that is true just in those models, does not hold for the entire class of models of FOL. Moreover, as Carnap showed, transfinite rules for the quantifiers are needed to eliminate the non-normal models, since not even Hilbert's ε-operator is sufficient for this. The question of the relation between the open-ended rules for the universal quantifier and the transfinite ones will be also addressed in the paper.

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COMMENT ON "KARL R. POPPER: LOGICAL WRITINGS"

Session 15C

Congress section(s): B6

Since Karl Popper's work in logic and its philosophy from the 1940s was, unfortunately, mostly neglected, the publication of a critical edition of Popper's logical writings is a remarkable and fruitful event for the academic community. Thomas Piecha discusses in his talk some central issues of Popper's work that have been later developed by contemporary logicians, most of which are concerned with logical negation. In my intervention, I discuss Popper's logico-philosophical motivation for deducibility relations structurally defined (work developed latter by [Dana Scott 1971, 1974] and [Arnold Koslow 1992]) and for logical negation. Popper's attempt to weaken classical logic in mathematical proofs by weakening the rules for logical negation is important and deserves closer attention. For instance, [Saul Kripke 2015] has recently argued that an affirmativist logic, i.e., a logic without negation and falsity, which is semantically distinct from [Hilbert and Bernays 1934]'s positive logic because it also eliminates falsity, is all that science needs. In this respect, the analysis of the (non-) conservative extensions of a minimal system of logic is necessary and philosophically useful. Popper's interest in logical negation was also generated by [Carnap 1943]'s discovery of non-normal models for classical logic that arise mainly due to negation (an important issue today for classical logical inferentialists).

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THE INQUIRY MODEL OF MEDICINE

Session 19E

Congress section(s): C4

In this paper I contrast two models of medicine, the Curative Model and the Inquiry Model, and favour the latter.
Medical traditions vary as widely as one can imagine in different times and places. Given the apparent variety of actual practices that are either claimed to be or regarded as medical, what, if anything, do they share—other than the fact we are inclined to refer all these wildly different practices as (at least partly) medical. Any adequate philosophy of medicine must respond to this question, and say something about what medicine is.
I consider a simple Curative Model, stating that the goal and business of medicine is to heal the sick. I distinguish the goal of medicine from its core business, that is, the exercise of some competence or skill that is characteristically medical and that is more than mere well-meaning but inexpert assistance, like bringing a sick relative a nice cup of tea and a pair of slippers. I defend the Curative Model's implication that cure is the goal of medicine against various objections.
However, the curative record of medicine, considered across all times and places is dismal. Yet medicine has persisted in many times and places. Why? I argue that this persistence shows that the business of medicine – the practice of a core medical competence – cannot be cure, even if that is the goal. Instead, what doctors provide is understanding and prediction, or at least engagement with the project of understanding health and disease. I argue that this Inquiry Model explains the persistence of ineffective medicine, and defend it against various objections.

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FORMAL AND INFORMAL LOGIC IN THE LVOV-WARSAW SCHOOL

Session 7E

Congress section(s): A4

The Lvov-Warsaw School (LWS) was a group of scholars which included philosophers, logicians, psychologists. The School was founded by Kazimierz Twardowski, a student of Franz Brentano and a great teacher of philosophy. All members of the School are direct or indirect students of Twardowski. Two main centers of the School were Lvov and Warsaw and the most fruitful period in the development of the School was the first four decades of the 20th century.
Representatives of the School analyzed similar problems but did not accepted any common set of theses. The most important binder of the school was methodological. Members of the LWS realized the postulates of precision of speech and justification of convictions. They found the way of achieving clarity and justification in the logical tools. However, the conception of logic in the LWS was broad. Twardowski and his students included into its scope not only the theory of inference and formal issues but also informal theories of cognitive operations, the problems of logical semiotics as well as the methodology of sciences and humanities.
The LWS had a strong formal branch the results of which are commonly known. Łukasiewicz's three-valued logic, metalogic and inquiries in the history of ancient logic, Leśniewski's systems (Prototetics, Mereology, Ontology), Alfred Tarski's formal semantics and his definition of "truth" for formal languages are some standard examples of formal achievements of the LWS. However, partially under the influence of formal research and partially independently from it, the problems contemporarily included into the so-called "informal logic" were analyzed in the LWS. These problems include, among others: the theory of reasoning, including fallible and practical reasoning (Twardowski, Łukasiewicz, Kotarbiński, Czeżowski, Ajdukiewicz,), the theory of argumentation and discussion (Twardowski, Witwicki, Czeżowski), the methodology of sciences, including the problem of induction and discovery/justification distinction (Twardowski, Łukasiewicz, Kotarbiński).
In my paper, I will sketch both formal and informal achievements of the LWS. I will also show the way formal and informal problems penetrated each other in the works of members of the School. Finally, I will demonstrate the contributions of these results to the general history of logic and philosophy.

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SCIENTIFIC WAYS OF WORLDMAKING. CONSIDERATIONS ON PHILOSOPHY OF BIOLOGY FROM GOODMAN'S THEORY OF WORLDS

Session 10K

Congress section(s): B4, C3

The reflection on the worldviews (Weltanschauung) has been very fruitful in several philosophical trends of the 20th century. In analytical philosophy has been treated by various authors and from different points of view, as sociological (L. Fleck and Th. S. Kuhn), logical (R. Carnap) or more metaphysical points of view (W. Sellars), among many others. However, in contrast to the notion of the worldview, the concept of worldmaking is more recent and N. Goodman can be placed at the centre of the discussion.
One of Goodman's main tasks in Ways of Worldmaking (1978) is the defence of a far-reaching epistemological pluralism. His suggestion is an epistemological constructivism in which we do not have only different worlds, but also different ways of describing those worlds. In his work, Goodman focus several chapters on the construction of worlds in painting, music and language (oral and written), but he says not so much on the role sciences can play in the construction of a world.
My contribution in this communication is double. First, I will comment on Goodman's general theory of the worldmaking to see if a distinction can be made between concepts as «world», «description of the world» and «frame of reference» or if, on the contrary, they are impossible to separate. For this, I will consider L. Wittgenstein On Certainty (1969) and Cassirer's notion of symbol as it is collected by Goodman.
Secondly, I will analyse the possibility of building scientifically a world from Goodman's approach. I will focus on genetics in philosophy of biology, considering authors as E. Mayr and E. Fox Keller. I will show how the processes of worldmaking that Goodman refers to as «composition», «weighting» and «supplementation» can be found in the evolution of the gene concept.
As we know, Goodman's position has been widely criticized (P. Boghossian). However, I believe that is still relevant to all philosophy of science that considers certain perspectivism. Therefore, I am going to defend the relevance of maintaining Goodman's theory for the philosophical analysis of the scientific constructions of worlds.
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THE COMMON-SENSE NOTION OF TRUTH AS A CHALLENGE FOR CONCEPTUAL RE-ENGINEERING

Session 19C

Congress section(s): B1

Tarski's semantic theory of truth is often considered one of the prime examples of an explication. Aiming to satisfy what Carnap called the criterion of 'similarity to the explicandum', Tarski claimed that his definition of truth is in line with the ordinary notion of truth, which in turn can broadly be interpreted in the sense of correspondence with reality.
In the first part of the talk, we present results of experimental studies which challenge the idea that – within the empirical domain – the common-sense notion of truth is rooted in correspondence. In these experiments, participants read vignettes in which a person makes a claim that either corresponds with reality but is incoherent with other relevant beliefs, or that fails to correspond with reality but is coherent with other beliefs. Perhaps surprisingly – at least from a philosopher's perspective – a substantial number of participants (in some experiments up to 60%) responded in line with the predictions of the coherence account. These results put substantial pressure on monistic accounts of truth. However, they also seem to undermine their most popular alternative: scope pluralism. While scope pluralists acknowledge that the truth predicate picks out different properties in different domains, no one has yet, as far as we know, worked out a pluralistic account within the same domain.
In the second part of the talk, we explore the consequences of these results for the project of re-engineering truth. In particular, we discuss the prospects of (i) defending a unique explication of truth, of (ii) re-engineering truth as a non-classical concept (e.g. as a family resemblance concept), and of (iii) giving more than one explicatum for true. Whereas the first of these options might seem attractive for theoretical reasons, it performs surprisingly poor with respect to the similarity to the explicandum, given the results of our experimental studies. Adopting (i) would simply amount to dismissing a great deal of applications of the truth-predicate. In this respect, options (ii) and (iii) are more promising. However, the success of the second option would not only depend on whether a non-classical concept of truth could be theoretically fruitful while being in line with enough of the data, but first of all on whether an exact description of the structure of such a non-classical concept could be given in a convincing way. In contrast to the first two options, (iii) would substantiate the claim that 'truth' is ambiguous. While this looks perhaps most apt to account for our data, such a proposal requires us to know more about the mechanisms that play a role in ordinary discourses on truth. Merely specifying several explicata without an account of what the conditions are for using one rather than the other would not make for an adequate conceptual re-engineering.

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CORPORATE FUNDING OF PUBLIC RESEARCH: A FEYERABENDIAN PERSPECTIVE

Session 10G

Congress section(s): B5

Corporate funding of research at public universities raises several issues concerning conflicts of interest, transparency, and bias. Paul Feyerabend's controversial book "Science in a Free Society" (1978) contains two seemingly incompatible statements that are both relevant for the question of how to organize research funding. On the one hand, Feyerabend famously argued that decisions about the uses of tax money for research must be reached in a democratic and participatory way. Expert opinions may be considered, but the last word about the choice of research topics must stay with the tax payers. In another passage of the book, Feyerabend pleas for a strict separation between science and the state in analogy to the separation between the state and the church. I call this the separation claim. The separation claim, by implication, precludes the possibility of using tax money for research.

In this paper, I apply Feyerabend's ideas on research funding to a public controversy that took place four years ago in Switzerland. After a Swiss newspaper had revealed a secret contract between the University of Zurich and the bank UBS, in which the bank offered the university 100 Million Swiss Francs for the establishment of an international research center for economics, a group of Swiss academics launched an international petition for the protection of academic independence. The online petition was signed by over 1000 academics within a couple of days. The signees called on "leaders of the universities and all who bear responsibility for our educational institutions, at home and abroad, to safeguard the precious heritage of free and independent scholarship, and to avoid endangering the academic ethos in controversial collaborations." The management of the university reacted to the petition by declaring that third-party funding was a necessity for a university that wants to engage in intentionally competitive research, and that corporate sponsoring was no threat to academic independence. Eventually, the research center was established, and the university was forced to disclose the contract.
The goal of the paper is to analyze the problem of corporate research funding from a Feyerabendian perspective, and to apply Feyerabend's arguments to the UBS case. I show how the alleged tension between Feyerabend's claim for democratically controlled public funding and the separation claim can be resolved by pointing at the fact that the aim of the separation claim is to achieve a pluralism of traditions, and not the abolishment of state funded research. Public as well private funding schemes are permissible in Feyerabend's account as long as there is no monopoly of experts, who decide about the contents of research. I conclude that "Science in a Free Society" provides no arguments against corporate research funding, and that from a Feyerabendian perspective, private funding does not pose a threat to academic freedom and independence. Quite on the contrary, a diversity of funding sources can be seen as a beneficial factor for a plurality of methodological and theoretical approaches at public research institutions.

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ABSTRACTION BY PARAMETRIZATION AND EMDEDDING. A CONTRIBUTION TO CONCEPT FORMATION IN MODERN AND CONTEMPORARY MATHEMATICS

Session 5C

Congress section(s): C1

The traditional approach to concept formation and definition via abstraction presupposes an Aristotelian ontology and its corresponding hierarchy according to which "definitio fit per genus proximum et differentiam specificam." According to this approach, abstraction is tantamount to removing properties and making the corresponding concept less rich, the more abstract a concept is, the fewer content it has. The traditional approach to abstraction and definition does not, however, provide an adequate model for concept formation and definition in mathematics.
What we need instead of the traditional picture is an account of concept formation and definition that is (1) true to mathematical practice; (2) true to the mathematical experience; (3) is compatible with insights from cognitive science. We take this to mean in particular that any such account should be informed by historical case studies (to satisfy (1)) and explain why and how abstract concepts are oftentimes perceived as more powerful and richer, not poorer in content (in order to meet (2)). Requirement (3) needs to be in place for keeping the analysis scientifically sound.
Recent accounts of abstraction in mathematics approach the topic by rehashing the development of modern mathematics since the 19th century and, consequently, emphasize aspects such as algebra (see, e.g., [2]) set theory (see, e.g., [3]), the rise of category theory (see, e.g., [5], [6]), or link the development in mathematics to broader cultural shifts (see [4]). These studies meet to a certain extent (1) and (2). This paper adds to the existing literature by homing in on a topic that lies in the intersection (1) and (2), namely, the question why abstract concepts are perceived as more powerful and richer, not poorer in content. It does so not by tracing any historical developments but by using a number of selected case studies to identify and then discuss various techniques for abstraction that have so far not received proper attention. An account of requirement (3) was given [1].
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SCALE SEPARATION, SCALE DEPENDENCE, AND MULTISCALE MODELING IN THE PHYSICAL SCIENCES

Session 13E

Congress section(s): C8

In multi-scale modeling of physical systems, dynamical models of higher-scale and lower-scale behavior are developed independently and stitched together with connective or coupling algorithms, sometimes referred to as "handshakes." This can only be accomplished by first separating modeled behaviors into bulk behaviors and surface or interfacial behaviors. This strategy is known as "scale separation," and it requires physical behaviors at multiple length, time, or energy scales to be treated as autonomous from one another.
In this talk, I examine what makes this strategy effective—and what happens when it breaks down. The nanoscale poses challenges to scale separation: there, the physics of the bulk occurs at the same length scale as the physics of the surface. Common scale-separation techniques, e.g. modeling surfaces as boundary conditions, fail. Modeling the scale-dependent physics of nanoscale materials presents a new challenge whose solution requires conceptual engineering and new modeling infrastructure. These considerations suggest a view of physical modeling that is centered not around idealization or representation but around scale.

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MATHEMATICAL VS. EMPIRICAL THOUGHT EXPERIMENTS: BETWEEN INFORMAL MATHEMATICS AND FORMALIZATION

Session 28C

Congress section(s): C1

In spite of the special importance that Ernst Mach attached to mathematical thought experiments (henceforth MTE), the literature paid too little attention to the relationship between this kind of TE and that to be found in the experimental sciences (cf. Witt-Hansen 1976, Müller 1969, Brown 1999, 2007b and 2007c, Van Bendegem 2003, Sherry 2006, Starikova 2007, Cohnitz 2008, Starikowa and Giaquinto 2018; An indirect, though fundamental treatment of TEs in mathematics, based on the distinction between 'formal' and 'informal' (inhaltliche) mathematics, is to be found in Lakatos 1963-4: on Lakatos, see especially Yuxin 1990, Glas 2001 Kühne 2005, pp. 356-366, and Sherry 2006).
To clarify the relationship between empirical and MTEs, in the first part of my talk I shall give a very brief outline both of what I take to be the essential characteristics of thought experiments in the experimental sciences and of what makes up the irreducibility of (formal) mathematics to empirical knowledge.
After this preliminary preparing of the way, I shall approach the main question of my talk. It is usually taken as obvious, from Mach onwards that there are MTEs. But against this opinion I shall maintain that in an important epistemological sense, unlike in the natural sciences and applied mathematics, TEs cannot be regarded as a particular method within formal mathematics. In this sense, it would be misleading, and therefore not advisable, to speak of TEs in (pure) mathematics, since

these cannot, in principle, come into direct conflict with experience, which is a function of the interactions between our bodies and the surrounding reality. Even though visualisation plays an important role in many TEs of pure mathematics, MTEs are to be considered, as far as an epistemologically fundamental aspect is concerned, more similar to formal proofs than to TEs in the natural sciences.
But, in another sense, one can recognize the existence MTEs, without collapsing the distinction between formal analytic knowledge - which cannot directly conflict with experience - and experiential knowledge, which is always connected with the way in which we interact, through our body, with the world of reality around us. Indeed, the notion of a formal system does not express a state, but an ideal type, which is exemplified, but not exhausted, by the relatively well-insulated systems to which the formalizing activity of thought gives rise. Every time mathematics temporarily reopen themselves to the interactions between our actions and the environment around us, then it is liable to undergo change in accordance with some aspects of our empirical knowledge; and the more this reopening runs deeper the more TEs are fruitful and play a role that is not fundamentally different from that existing in the experimental sciences. Indeed, TEs have a role to play not only in cases where new branches or even new mathematical disciplines appear in human culture, but also in the effort to constitute the complete coherence of the network of connections between the various formal theories that makes up mathematics at a certain period of its historical development. It is no accident that Lakatos was at the same time fully aware of the historicity of mathematics and gave one of the most important impulses to the development of a theory of MTEs, although his philosophy is in many ways unsatisfactory, especially because it does not provide any criterion to clearly distinguish, at least in principle, mathematics from experimental sciences.

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PSEUDOSCIENCE WITHIN SCIENCE? THE CASE OF ECONOMICS

Session 17F

Congress section(s): C7

In this contribution, the multicriterial approach to demarcation, predominant over the last decades (Mahner 2007, Hansson 2017), is applied in order to assess whether some common practices in economics should be considered pseudoscientific. To distinguish science from pseudoscience within economics, special attention is payed to the following interconnected features: a priori endorsement of assumptions refuted by experience, no independent testability of auxiliary hypothesis, and lack of assessment of alternative theories.
The paper is focused on an important example of pseudo-scientific practice in economics, namely, the endorsement and hidden a priori justification of assumptions refuted by experience. The highly ambiguous and variable nature of social phenomena makes that under-determination of theory by observation affects social science more dramatically than natural science, so that pseudoscientific assumptions may be more easily kept in the former —despite the fact that disconfirming evidence clearly suggests their falsity. There is no doubt that the highly complex and intertwined nature of social interactions requires their theoretical decomposition by means of idealizing assumptions, whose purpose consists in isolating causal variables (Mäki 2009: 78). However, it becomes obvious that, in some cases, certain false assumptions are kept even though they do not contribute to the isolation of any real causal variable. In this vein, a pseudoscientific use of idealizing assumptions can result from a lack of connection between them and real systems —as when using 'substitute models' as if they were 'surrogate models', in Mäki's terms—, but also from imposing isolations precluded in real systems (Mäki, 2009. P. 85). For example, excluding the role of institutions when representing economic systems may dramatically limit the explanatory capacity of the corresponding representation.
The same combination of inadequate idealization and disregard of refuting information is manifested in the economists' reluctance to depart from certain normatively appealing principles from the expected utility theory (Starmer 2005). Such reluctance is clear in the types of revisions which have been proposed to EUT —which have typically retained normatively attractive principles such and monotonicity and transitivity in spite of contrary evidence.
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INTRODUCING CAUSALITY IN STIT LOGIC

Session 12B

Congress section(s): A2

In stit logic, every agent is endowed at every moment with a set of available choices. Agency is then characterized by two fundamental features. That is, (i) independence of agency: agents can select any available choice and something will happen, no matter what the other agents choose; (ii) dependence of outcomes: the outcomes of agents' choices depend on the choices of the other agents. In this framework, an agent sees to it that F when her choice ensures that F, no matter what the other agents do.
This characterization (or variants thereof) is taken to capture the fact that an agent brings about F. However, the notion of bringing about thus modelled is too demanding to represent situations in which an individual, interacting with others, brings about a certain fact: actually, in most of the cases in which someone brings something about, what the other agents do matters.
In light of this, we aim at refining stit semantic in order to make it suitable to represent the causal connection between actions and their consequences. The key idea is, first, to supplement stit semantics with action types (following Broersen, 2011; Herzig & Lorini, 2010; Horty & Pacuit, 2017; Ming Xu, 2010); then, to introduce a new relation of opposition between action types. We proceed as follows.
Step 1. Let (Mom, <) be a tree-like and discrete ordered set of moments and call "transition" any pair (m,m') such that m' is a successor of m. Given a finite set Ag of agents, we have, for each i in Ag, a set A_i of action types available to i and a labelling function Act_i assigning to each transition an action type available to i, so that $Act_i((m,m'))$ is the action that i performs along transition (m,m'). The joint action performed by a group I of agents along (m,m') is then the conjunction of the actions performed by the agents in I along (m,m'). The joint actions performed by Ag are called global actions, or strategy profiles. In this framework, the next-stit operator [i xstit]F can be given a straightforward interpretation.
Step 2. Intuitively, an individual or joint action B opposes another individual or joint action when B blocks or hinders it (e.g. my action of running to catch a train is opposed by the crowd's standing in the way). In order to represent this relation, we introduce a function O associating to every action B the set O(B) of actions opposing B. We then say that B is unopposed in a global action G just in case B occurs in G and no action constituting G opposes B. The global actions in which B is unopposed represent counterfactual scenarios allowing us to determine the expected causal consequences of B. Specifically, we can say that F is an expected effect of B only if B leads to an F-state whenever it is done unopposed.
Besides presenting an axiomatization of the logic induced by the semantics just sketched, we show that the next-stit operator [i xstit]F is a special case of a novel operator [i pxstit]F, defined in terms of expected effects, and that, by using this operator, we are able to fruitfully analyse interesting case studies. We then assess various refinements of the [i pxstit] operator already available in this basic setting. Finally, we indicate how this setting can be further elaborated by including the goals with which actions are performed.

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BOLZANO'S REQUIREMENT OF A CORRECT ORDERING OF CONCEPTS AND ITS INHERITANCE IN MODERN AXIOMATICS

Session 17B

Congress section(s): B6

The question of the right order of concepts cannot be separated from the problem of rigor in mathematics and is usually formulated with reference to Aristotle's distinction between ordo essendi and ordo cognoscendi: the search for rigor in science should include some kind of activity that could lead us from what is first for us to what is first in itself. Bolzano's remarks about correctness of definitions and proofs are based on the requirement of a right order of concepts and truths. Recent literature has devoted great attention to the objective order of propositions in proofs, explaining it by association with the theory of grounding. Yet, scarce attention has been given to the order of concepts, which is related to the form of definitions and to the distinction between simple and complex concepts. The paper will investigate whether the order of concepts should be considered as having an ontological or epistemological value, given that 'a concept is called simple only if we ourselves can distinguish no more plurality in it'.
Bolzano's view on the order of concepts will be reconstructed on the basis of his mathematical and logical works, in order to understand the relation between his logical and epistemological viewpoints. The ban on kind crossing as well as the use of philosophical notions (e.g. similarity) in geometrical proofs will be analyzed to verify whether a general hierarchical order of scientific concepts regulates the correct ordering of concepts in mathematics.
A comparison with Wolff's conception and the analysis of the definition of similarity of mathematical objects will suggest a tension, inherited from Leibniz, between the tendency to have a unique hierarchical order of all concepts and an order for each specific mathematical discipline. A further comparison with the investigations on explicit and implicit definitions developed by the Peano School will allow establishing whether, notwithstanding different syntactic formulations of definitions, Bolzano's requirement of an order of concepts maintained some role up to Peano's axiomatics.

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INNOVATIVE TOOLS FOR REACHING AGREEMENTS IN ETHICAL AND EPISTEMIC PROBLEMS IN BIOSCIENCES

Session 31D

Congress section(s): C4

This talk will present several innovative methodological tools that are being used in biomedical sciences when epistemic and/or ethical problems arise, and there are different stakeholders with different values, priorities, and aims who need to reach an agreement.
Biomedical sciences may imply scientists and technologists from very different fields, that therefore have a different language, aims, methodology, and techniques. Reaching an agreement when there are so many differences between them can become very complex. Besides, biomedical sciences either when applied or when gathering information about human subjects can generate complex ethical problems which imply reaching agreements among very different agents, such as scientists, doctors, nurses, politicians, citizens or animal rights activists.
After a brief presentation of the state of the art in the subject, we will discuss two main methodological tools:
The Ethical Matrix. Firstly developed to discuss when is ethically admissible to introduce GMO foods in a specific environment, this is a very powerful tool to find agreements in lots of different ethical problems in biomedical sciences and can be helpful also when analyzing epistemically complex situations where agreements among very different disciplines have to be made.
Value maps. Built in a collaborative manner, these maps can help researchers to realize ethical implications of their work they weren't currently aware, and also to discover non-epistemic values that, nonetheless, can be helpful to improve innovate processes in scientific research.

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LAWS OF NATURE AND EXPLANATORY CIRCULARITY

Session 11G

Congress section(s): B4

Some recent literature (Hicks and Elswyk 2015; Bhogal 2017) has argued that the non-Humean conceptions of laws of nature have a same weakness as the Humean conceptions of laws of nature. Precisely, both conceptions face a problem of explanatory circularity: Humean and non-Humean conceptions of laws of nature agree that the law statements are universal generalisations; thus, both conceptions are vulnerable to an explanatory circularity problem between the laws of nature and their instances.
In the literature, the terminology "explanatory circularity problem" has been used to designate two slightly different circularities. A first circularity is a full explanatory circularity, hereafter the problem of circularity C. Synthetically, a law of nature is inferred from an observed phenomenon and, thereafter, it is used to explain that same observed phenomena. Thus, an observed phenomenon explains itself. The other circularity is a problem of self-explanation, hereafter the problem of circularity SE. The problem of circularity SE is a sub-problem of the problem of circularity C. A law of nature explains an observed phenomenon, but the law includes that same phenomenon in its content.
Hicks and Elswyk (2015) propose the following argument for the problem of circularity C:

(P1) The natural laws are generalizations. (HUMEANISM)
(P2) The truth of generalizations is (partially) explained by their positive instances. (GENERALIZATION)
(P3) The natural laws explain their instances. (LAWS)
(P4) If A (partially) explains B and B (partially) explains C, then A (partially) explains C. (TRANSITIVITY)
(C1) The natural laws are (partially) explained by their positive instances. (P1 & P2)
(C2) The instances of laws explain themselves. (P3, P4, & C1) (Hicks and Elswyk 2015, 435)

They claim that this argument also applies to the non-Humean conceptions of laws of nature: “Humeans and anti-Humeans should agree that law statements are universal generalizations (...) If we’re right about this much, anti-Humeans are vulnerable to a tu quoque.” (Hicks and Elswyk 2015, 435)
The argument above can be reframed to underpin the problem of circularity SE.
(P1) The natural laws are generalizations (HUMEANISM)
(P2)* If the natural laws are generalizations, then the natural laws are (partially) constituted by their instances.
(P3) The natural laws explain their instances. (LAWS)
(C2) The instances of the law statements explain themselves.
In this presentation, I will discuss the premises of the above arguments. I will try to show that Armstrong’s necessitarian view of laws of nature (Armstrong 1983) – a non-Humean conception – is invulnerable to these explanatory circularity problems. At the end, I will analyse a semantic circular condition for unsuccessful explanations, recently proposed by Shumener (2018), regarding this discussion.
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BLUR SCIENCE THROUGH BLURRED IMAGES. WHAT THE DIVERSITY OF FUZZY PICTURES CAN DO FOR EPISTEMIC, METHODOLOGICAL AND CLINICAL GOALS

Session 3A

Congress section(s): B1

Different kinds of images include hand-drawings, analogical and digital photographs, and computer visualizations. I show how historically these have been introduced in an ongoing project of simulation of blurred vision that begins with so-called artificial models, artificial visual aberrations and photographic simulations, and experiments. Computer simulations followed suit, each with their own specific conditions.
I show how the different kinds of pictures, (like the roles and goals they serve), do not always arise to replace others, but instead develop different relations to others and introduce new uses. In the new pictorial regime, research and clinical practice rely on a combination of drawings, different kinds of photographs, and computer visualizations. I show how the simulations and the pictures play a number of roles: providing illustration and classification, prediction, potential explanations (a deeper level of classification), exploration, testing, evidence for or against explanatory hypotheses, evidence for or against the effectiveness of research tests and techniques, evidence for or against the reliability of diagnostic tests and the effectiveness of corrective treatments, and tracking the evolution of conditions and treatments.
Fuzziness or blur in images deserves critical attention as a subject and resource in scientific research practices and clinical interventions. I discuss how the project of engaging blur in vision optics is embedded in a constellation of different mathematical and pictorial tools with different standards and purposes—both investigative and clinical—which are often inseparable. An expression of this is the variety of kinds of pictures of blurred vision, many of which do appear blurred, and their different and shifting roles and uses. Their use runs against the commitment to sharpness as an ideal of, for instance, scientific representation, reasoning, and decision making.
My analysis contradicts and supplements a number of other accounts of the significance of images in terms of their content and use. A central issue I focus on is how the central interest in the phenomenon of blur in visual experience prompts pervasive and endemic considerations of subjectivity and objectivity. Different relations and tensions between standards of subjectivity and objectivity play a key role in the evolution of research and clinical intervention. This aspect finds expression in the interpretation, production, and use of pictures.

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ISSUES AT THE INTERSECTION BETWEEN METAPHYSICS AND BIOLOGY

Session 16L

Congress section(s): A2

Recent work in Metaphysics and in Philosophy of Science, and in particular in Philosophy of Biology, shows a revival of interest in issues that might be considered to be either metaphysical issues that can be further elucidated by recourse to biological cases or metaphysical consequences that some advancements in Biology have. In some cases, the application of some metaphysical notions to classical debates in Philosophy of Biology helps to clarify what is at stake and to solve some misunderstandings in the discussion. The interactions that can take place between Metaphysics and Biology are therefore of different kinds. In my contribution, I will present some examples in which such interaction takes place and will explore the way in which such interaction takes place. In general, I will present interactions between Evolutionary Biology, Genetics and Developmental Biology and metaphysical notions such as dispositions, identity and persistence, and teleology.
Although I will present several examples, I will focus in particular on one or two of them, namely the interaction between metaphysics of dispositions and Genetics, on the one hand, and the one between theories of persistence and the species concept issue in Philosophy of Biology.
I will revise the Dispositionalist theory of causation recently proposed by Mumford and Anjum (2011) and evaluate its explanatory potential and difficulties when it is applied to causal analysis in Biology. My main concern is with the application of their theory to Genetics, something that they do as an illustration of their proposal in chapter 10 of their book. I will try to deploy further the advantages and disadvantages of a dispositionalist conception of genes. After introducing some crucial features of their approach, I will revise the advantages of their conception to account for complex biological phenomena, and its potential to overcome the dispute between gene-centrism and developmentalism. However, I will raise a difficulty for the dispositionalist, namely, the difficulty to defend the simultaneity of cause and effect (essential in their proposal) when epigenetic processes are taken into account. I will focus on a particular phenomenon, the mechanism of RNA alternative splicing and will explore some ways out of the difficulty.
Secondly, I will address the question of whether the persistence of biological species raises any difficulty for the thesis of the metaphysical equivalence between three-dimensionalism (3D) and four-dimensionalism (4D). I will try to show that, even if one assumes that ‘species’ is a homonymous term and refers to two entities (evolverons or synchronic species and phylons or diachronic ones), 3D/4D metaphysical equivalence still holds. My argument lies in challenging the strong association between a synchronic view of species and a 3D theory of persistence, and a diachronic view of species and a 4D theory of persistence.
In the last part of my contribution, I will try to characterize the way in which Metaphysics and Philosophy of Biology interact in those issues.

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APPROACHING DETERMINISTIC AND PROBABILISTIC TRUTH: A UNIFIED ACCOUNT

Session 5D

Congress section(s): B2

Popper introduced the notion of the verisimilitude (or truthlikeness) of a scientific theory or hypothesis in order to make sense of the idea that the goal of inquiry is an increasing approximation to “the whole truth” about the relevant domain. Post-Popperian accounts of verisimilitude rely on quite different intuitions, and disagree from each other on some crucial features of the notion of truthlikeness. However, they share an important assumption, i.e., that both theories and the truth

are “deterministic” or “categorical”. In other words, both a theory and the truth are commonly construed as propositions of some language, and truthlikeness is a matter of closeness or similarity between the relevant propositions.
To illustrate, consider the following toy examples. Suppose that the (unknown) truth about tomorrow’s weather in some location is that it will be hot, rainy, and windy. If Adam says that it is hot and rainy, and Eve says that it is hot and dry, it seems clear that Adam’s beliefs will be closer to the truth than Eve’s. All available accounts of truthlikeness can provide this kind of assessments. However, they are not well equipped to deal with cases like the following. Suppose that Adam thinks that the probability of rain tomorrow is 80%, while Eve assesses such probability at 30%; again, it seems that Adam’s estimate will turn out to be more accurate than Eve’s. Or suppose that, the credences of Adam and Eve being the same as above, the actual frequency of rainy days in the relevant location is 90%; again, one would say that Adam’s beliefs are closer to the (objective, probabilistic) truth than Eve’s. In order to make sense of these intuitive judgments, one needs to extend the notion of truthlikeness to probabilistic contexts.
In this paper, we address this issue and provide a unified account of both deterministic and probabilistic truth approximation. More specifically, we proceed as follows. First, we show how to measure the truthlikeness of both deterministic and probabilistic theories in terms of the probability they assign to the “basic features” of the relevant domain. Second, we apply such measure to four different possible cases, investigating truth approximation construed as increasing closeness: 1) of a deterministic theory to the deterministic truth; 2) of a probabilistic theory (a so called epistemic state) to the deterministic truth; 3) of a deterministic theory to the probabilistic truth; and 4) of a probabilistic theory to the probabilistic truth. Interestingly, in our account deterministic truth approximation turns out to be a special case of probabilistic truth approximation. Finally, we discuss the implications of our approach for a number of issues usually discussed across different pieces of literature, including so-called epistemic utility theories, scoring rules for measuring the distance between probability distributions, and measures of the truthlikeness of “tendency” hypotheses (especially relevant in the social sciences).

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A PHILOSOPHY OF HISTORIOGRAPHY OF THE EARTH. METAPHOR AND ANALOGIES OF “NATURAL BODY”

Session 20L

Congress section(s): B4, B6, C2, C3

Long before the “Gaia hypothesis”, the analogy between “Earth” and “body” has an history that may starting with history of thought itself. Indeed, combining political, social, theological and scientific issues, “body” is the image par excellence of the functional whole lasting over time. Can we say then that it is a “metaphor”? What does this mean from an epistemological point of view? If metaphor is only a “trope” in classical rhetoric, what can be said about its use and function within modern theories ? Is it, to use Paul Ricoeur’s distinction in the “métaphore vive”, a catachesis or a real semantic innovation? Does it serve as a model, a “tool”, to direct scientific research towards a new interpretation of reality or does it have a specific heuristic? What is the relationship between metaphor and analogy in modern thought?
These questions will be explored from the examples of three early modern philosophers, namely Hobbes, whose Leviathan makes an analogy between the animal body and the social body, serving as a justification for placing the sovereign at his head, Herder, who taking up Leibniz’s notion of “organism” and transforms it into an epistemological tool for thinking nations, seen as living, growing, dying, and have to be interpreted by historians themselves, and finally Leibniz, who, between Hobbes and Herder, not only criticizes Hobbes (and Pufendorf in his wake), but extends the use of the animal body’s analogy (seen as “organism”) to many areas of his thought, from natural history to human history, because the world as the organism contains its own principle of action and development. Criticizing the Cartesian idea of reducing the body to the mathematical extent and seeking at the same time how to apply the principle of bodies’ individuation, Leibniz finds the specificities of what he calls the “machines of nature”, that is to say of organic bodies that are not only mechanical but have in them a principle of order infinitely superior to human artefacts. The problem of the body’s individuation exceed the biological domain and has to be extended to the whole nature, because, according to Leibniz, Earth is also a kind of natural body, which has specific organs producing and destroying metals: that is a metaphor of rhetorical he used aiming at rehabilitating as quickly as possible the mining in decadence. But this strategic form is repeated when Leibniz presents to King Ernest August the fruit of several years of genealogical research on royal funds and, by so, speaks a language that the prince’s education, nourished by humanism and pufendorfian thought, made familiar. The animal body, a structured whole whose organs are defined by their function, becomes in that correspondance the metaphor of history.

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THE GENERALIZED ORTHOMODULARITY PROPERTY: CONFIGURATIONS, PASTINGS AND COMPLETIONS

Session 20M

Congress section(s): B2

Quantum logic is a set of rules for reasoning about propositions that takes the principles of quantum theory into account. This research area originated in a famous article by Birkhoff and Neumann [3], who were attempting to reconcile the apparent inconsistency of classical logic with the facts concerning the measurement of complementary variables in quantum mechanics, such as position and momentum. A key role is played by the concept of orthomodular lattice, an algebraic abstraction of the lattice of closed subspaces of any Hilbert space [1, 14, 2, 5, 9, 15, 13]. In 1968, it was realized that a more appropriate formalization of the logic of quantum mechanics could be obtained relaxing the lattice conditions to the weaker notion of orthomodular poset [4, 15].
Actually, in the case of orthomodular posets, even if the orthocomplementation is still a total operation, lattice operations are only partially defined, in general. This difficulty cannot be resolved by considering the completion of the underlying poset. In fact, Harding [2] [14] showed that the Dedekind-MacNeille completion of an orthomodular lattice need not be orthomodular, in general. Therefore, there is no hope to find orthomodularity preserved by completions even for posets.
In our approach, we will weaken the notion of orthomodularity for posets to the concept of generalized orthomodularity property by considering LU-operators [6], and then analyzing the order theoretical properties it determines. This notion captures important features of the set of subspaces of a (pre-)Hilbert space, the concrete model of sharp quantum logic [7].
After dispatching all basics, we define the concept of generalized orthomodular poset, we provide a number of equivalent characterizations and natural applications. In the same section, we study commutator theory in order to highlight the connections with Tkadlec’s Boolean posets. Finally, we apply those results to provide a completely order-theoretical characterization in terms of orthogonal elements, generalizing Finch’s celebrated achievements [8]. Subsequently, we will describe some Dedekind’s type Theorems, that describe generalized orthomodular posets by means of particular subposets, generalizing [6]. We then propose a novel characterization of atomic amalgams of Boolean algebras [1]. In particular, a development of our arguments will yield Greechie’s Theorems as corollaries [10, 11].
Finally, we will prove that our notion, for orthoalgebras, corresponds to orthomodularity. This allows to conclude that an orthoalgebra has a Dedekind-MacNeille completion if and only if its induced poset is orthomodular, and it can be completed to an orthomodular lattice. To the best of our knowledge, these results are new and subsume, under a unifying framework, many well know facts sparsely scattered in the literature [16, 17].
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KARL POPPER, PREHISTORIC TECHNOLOGY AND COGNITIVE EVOLUTION

Session 16H

Congress section(s): C7

More than a century and a half after Darwin it is almost a commonplace that human species is the outcome of an evolutionary process going back to the origin of life. Just as the human brain-body has been shaped by evolutionary pressures operating in our ancestral past, the biological structures and mechanisms relating to human cognitive aspects might also have been selected for and it probably took millions of years for the distinctive cognitive faculties to evolve.
One way to find possible evolutionary explanations of these cognitive abilities is to explore the domain of prehistoric stone tool technology. Scholarly interest in the evolutionary impacts of lower Paleolithic stone tool making (and use) on the initial emergence of hominin cognitive behaviour is growing steadily. The most controversial questions include, for example, how in the evolutionary process did cognitive abilities (or consciousness) emerge in a world hitherto purely physical in its attributes? Or, what role did these early stone tools play in the evolution of human (or hominin) cognitive system? Stone tools are typically described in the archaeological literature as mere products of hominin cognition. Evidently the causal arrow assumed in this standard perception is one way— from cognition to tools or artefacts.
Since late 1990s several interesting approaches to cognition have come up challenging this simplistic one-way-causal-arrow view. Cognitive processes are increasingly interpreted as not just something happening entirely inside our head but as extended and distributed processes (e.g., Clark & Chalmers 1998; Hutchins 2008). Interesting is to note, Karl Popper’s theory of the emergence of consciousness (or cognition) posed a serious challenge to the one-way-causal-arrow view decades before the appearance of this beyond-the-body conception of human cognition. Reinterpreting Darwin’s views on the biological function of mental phenomena Popper’s psycho-physical interactionist (though not a dualist interactionist) theory (Popper and Eccles 1977; Popper 1978) not only questioned the mere epiphenomenal status of tools or artefacts but placed great emphasis on the role of such extra-somatic environmental resources in transforming and augmenting human cognitive capacities. What’s more, Popper’s conjectures about the emergence of consciousness seem strongly convergent with current experimental-archaeological research on early stone tool making and cognitive evolution (e.g., Jeffares 2010; Malafouris 2013).
The present paper seeks to synthesize the critical insights of Popper with those of the experimental-archaeologists to see if some fresh light could be thrown on the problem of hominin cognitive evolution.
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ABANDONING MODELS: WHEN NON-EMPIRICAL THEORY ASSESSMENT ENDS

Session 16C

Congress section(s): B1, B3, C2

The standard model of physics has several conceptual problems (for example, it has no explanation for the three almost identical particle generations) and explanatory gaps (for example, it has no dark matter candidate). These issues have motivated particle physicists to develop and test new models which go beyond the standard model (BSM). Currently, none of the unique predictions of any BSM model have met with any experimental success. The Large Hadron Collider, the most powerful particle accelerator in the world, has reached unprecedented energies, but it has found no compelling evidence for the new physics which researchers are convinced we will eventually find. Despite these experimental failures, physicists continue to pursue various BSM projects. The various groups of BSM models receive different degrees of confidence from physicists, which can be roughly tracked by observing the number of preprints and publications detailing them and the way they are discussed in the summary talks of large physics conferences. From this, we can see that the core ideas of these BSM models persist, even as various concrete predictions stemming from those core ideas fail. This persistence can be explained using classic schemes of theory assessment. For example, once suitably modified to accommodate models alongside theories, Lakatosian research programmes and Laudanian research traditions offer compelling explanations for this phenomena: in Lakatos the hard cores of BSM projects are shielded from contrary experimental results while in Laudan BSM projects can be understood as solving problems, despite their experimental failings.
However, by evoking such explanations, a new problem arises. With the next phase of particle physics uncertain, since there is no consensus on the plans for the next generation of particle accelerators, it is unclear how the various BSM models are properly discriminated without empirical findings to determine success and failure. Non-empirical justifications can be given for the continued pursuit of these kinds of models and theories (those which make predictions we lack the capacity to test), but we must also analyse the non-empirical justifications for abandoning a line of research. I will argue that particle physicists lose confidence and eventually abandon models because of two related factors. First, although a framework like Lakatos’s or Laudan’s can insulate BSM models from the experimental failures for a time, as the prospects of finding evidence for new physics at the LHC diminish, there is an equivalent depreciation of confidence in these models as a consequence of this lack of fruitfulness. Second, changes in the degree of support for the problems and solutions motivating BSM models cause similar changes in support for the models (for instance, with less confidence in naturalness as a guiding principle, models built to be more natural fall out of favour). These two factors lead to increasing disfavour in these models, and eventually their abandonment. Once we have established this non-empirical reasoning behind giving up models and theories, we can add it to our non-empirical assessment criteria at play in cases where theory extends beyond experiment.

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WHY SHOULD PHILOSOPHERS CARE ABOUT THE PERIODIC TABLE?

Session 26G

Congress section(s): B6, C2

The periodic table of chemical elements is one of the most recognizable icons in the entire history of science. Its ubiquitous presence in all kinds of scientific environments and even popular culture worldwide is a clear testimony to its usefulness and informativeness. But what makes the periodic table so special? Mendeleev was by no means the only chemist to attempt to make a convenient and informative ordering of chemical elements, and I will present a brief overview of the history of such attempts in the 19th century. I will also present some debates concerning the epistemic merits of Mendeleev's system, and show how the history of the periodic table can be used to make effective illustrations of epistemic values in action, focusing especially on explanation and prediction.

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AN INTRODUCTION TO ARABIC HYPOTHETICAL LOGIC

Session 5M

Congress section(s): A4

In this talk, I will present the hypothetical logics of the authors pertaining to the Arabic tradition, namely al-Fārābī, Avicenna, Averroes and some later logicians, especially in the Western area of the Arabic Medieval World (mainly North Africa). The main problems that I raise are the following: what is the hypothetical logic about? How is it viewed by the Arabic logicians, whether in early Medieval times (8th to 12th centuries) or in later ones (14th to 15th centuries)? What are the main theories and results provided by these authors?
To answer these questions, I will study the early Arabic authors such as al-Fārābī and Avicenna but also some later authors in order to see how this particular logical system has developed in the writings of the various Arabic authors.
I will show that in Averroes' frame the hypothetical logic, which studies the propositions containing 'if...then' (the conditional) and 'either...or' (the disjunction), is seen as really secondary, since he provides only the most basic hypothetical syllogisms, although he does use it in his proofs by reductio ad absurdum, while in al-Fārābī's frame, it is given a little more importance and developed in various ways. But it is in Avicenna's frame that it is studied at the greatest length and given the most importance, since Avicenna introduces quantification in this field and presents several systems of hypothetical logic, having distinct features. In later times, the hypothetical logic is given much importance in particular in the Western Arabic areas (North Africa) with the writings of Ibn 'Arafa and Senoussi, among others, who seem influenced by Avicenna and especially his follower al-Khūnajī.
I will thus present (briefly) the basic features and rules of these systems, and will compare between them in order to show the main differences in the analysis of these connectives and the evolution of this field in the Arabic tradition. I will also analyse these rules in the light of the modern classical formalism of propositional and predicate logics.

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AN ENGINEER: BRIDGING THE GAP BETWEEN MECHANISMS AND VALUES

Session 30B

Congress section(s): B1, B5

Emerging discourse in the philosophy of engineering within the modern field of the philosophy of science and technology is obviously focusing on a task of defining the "engineer concept". Author argues that the philosophy of engineering unlike the philosophy of technology highlights much attention to the engineering community that declares and documents its goals and values. In this context, the M. Weber's understanding of the term Beruf in his "Wissenschaft als Beruf" gives a contribution to the concept of modern engineer. On the one hand, the engineer possesses specialized skills, on the other hand, these skills have a broad ethical dimension since they are associated with the values of freedom and responsibility, with the relationship between the state and the individual. (For example, American philosopher of engineering M. Davis answering the question who are the engineers notes that the working functions they perform cannot help with the answer. The first, to equate designing, building, or the like with engineering makes distinguishing engineers from other technologists impossible. Second, to equate engineering with designing, building, or the like gives a misleading picture of what engineers in fact do).
Different national cultures use their methods to work with this engineer concept; one of the most convincing in my opinion is the American one, in which the philosophy of engineering is based on the study of engineering associations and their statements and codes. Among such approaches, I'd like to note E. Layton' famous work "The Revolt of Engineers", 1971. M. Davis, which I mentioned above, argues that the concept of an engineer cannot be understood through the functions that engineers routinely perform, the philosophical sense is important, so he, for example, seeks it in the comparative structure of engineering education, focusing on the role of humanitarian subjects there. Davis is also engaged in philosophical interpretations of engineering codes, seeking which tasks are higher priority for engineers (as well borrowing the approaches of the philosophy of law).
In my report, I use the approaches of M. Davis - the analyzing the declarations of the Association for Engineering Education of Russia (AEER) (founded in 1992), which deals with the procedure of accreditation for the educational programs for future engineers. The emphasis on the analyzing the structure of engineering education seems to be more relevant than the work with engineering codes due to the historical specifics of Russia. The obtained results can be compared with those presented by Davis, that helps to answer on the key questions of the philosophy of engineering: a) what is the most adequate research methodology for the work with problem of engineer concept, b) does this "concept" have a national cultural "face", despite the globalization of scientific and technological progress.
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COMPARING THE GEOMETRIC STYLE AND ALGEBRAIC STYLE OF ESTABLISHING EQUATIONS IN CHINA, 11TH-13TH CENTURIES

Session 19D

Congress section(s): C1

This contribution is part of the symposium "Styles in Mathematics"(SIM)
Co-organizers of the session: Erich Reck & Georg Schiemer
Chinese sources composed between the 11th and the 13th century attest to various styles of establishing algebraic equations to solve problems. Some documents like Liu Yi's 劉益 11th century Discussing the source of the ancient (methods), which survived through quotations in Yang Hui, Quick methods for multiplication and division for the surfaces of the fields and analogous problems, 1275, attest to a geometric statement of quadratic equations and hence a diagrammatic work to establish an equation. These documents are in continuity, both conceptual and practical, with the type of treatment of equations to which the canonical work The Nine Chapters on Mathematical Procedures (九章算術) and its commentaries by Liu Hui 劉徽 (263) and Li Chunfeng 李淳風 (656) testify. Other documents, like Li Ye's 李冶 Measuring the Circle on the Sea-Mirror 測圓海鏡, attest to a symbolic statement of algebraic equations, and they present symbolic modes of establishing equations. Interestingly, these other documents are also in continuity with the treatment of equations given in The Nine Chapters. In fact, the former types of documents represent an older practice, on the basis of which, I argue, the new style

took shape. This corpus of text thus documents an important phenomenon with respect to mathematical styles, that is, how a style is supplanted by another one. My talk will focus on the historical process that led from one style to the other. What motivated the change of style? How did it occur, and how did the change affect working with equations? What new opportunities and losses did this change of style represent? These are the questions I plan to address.

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PLURALISM ABOUT CRITERIA OF REALITY

Session 8J

Congress section(s): B4

The scientific realism debate has developed into a large-scale war, and in this paper, we first distinguish three lines of battle in order to situate our own contribution to the debate. (1) Within the realist camp, deployment realists, structural realists, and entity realists debate with one another regarding which parts of theories (working posits, structures, or posited entities) successfully represent unobservables. This is the ontological aspect of the debate. (2) Wholesalists/globalists and retailists/localists argue about whether the realism debate ought to be settled by arguments regarding our best theories in general (wholesale arguments) or by arguments regarding particular theories, claims, and/or entities (retail arguments). This is the methodological aspect of the debate. (3) Realists and anti-realists disagree about whether or not there is a criterion of reality according to which we can demonstrate that some parts of theories (usually the 'successful' parts, according to some notion of success) actually represent an unobservable reality. Realists argue that there is such a criterion while anti-realists argue that there is not. This is the epistemological aspect of the debate.
Methodologically, we adopt a retailist approach to the debate. Ontologically, we see the disputes among those in the realist camp as tracing back to a shared commitment to monism about criteria of reality paired with disagreement regarding which proposed criterion ought to serve as the single criterion of reality. And epistemologically, we adopt a pluralist view regarding criteria of reality. Our defense of this view consists of developing a theoretical framework that combines retailism with pluralism about criteria of reality, and illustrating that framework by showing how it applies to cases from the history of science.
Regarding the framework, we argue that a commitment to retailism leads naturally to a pluralist view of criteria of reality. Methodologically, retailists restrict their arguments to particular theories, claims, and/or entities. Their refusal to generalize the conclusions of such arguments to similar cases makes sense only if, epistemologically, retailists are open to the possibility that a single criterion of reality is not applicable to all cases. After all, if one and the same criterion is applicable to all cases, retailists would have a direct route to generalizing their conclusions to similar cases, which would make it difficult for them to maintain that such generalization is unwarranted.
Regarding the application of the framework, we consider three historical cases that are often discussed in the context of the realism debate: J. J. Thomson's work on corpuscles and cathode rays, Jean Perrin's work on atoms and Brownian motion, and Robert Millikan's measurement of the charge of the electron. We consider various criteria of reality (for example, manipulation, experimental individuation, and measurement). We determine which criteria are applicable in each of the three cases. And we draw some conclusions regarding whether the work in question provides a sufficient reason for adopting a realist attitude towards the entities involved in these cases.

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NATURAL ANALOGY: A HESSEAN APPROACH TO ANALOGICAL REASONING IN THEORIZING

Session 16J

Congress section(s): B1

This paper aims to explore the use of analogy in scientific theorizing via Mary Hesse's original understanding of analogical reasoning. The approach is thus Hessean. I revise Hesse's interpretation and symbolic schema of analogy to develop a new framework that can be used to analyze the structure and cognitive process of analogy.
I take Hessean approach for two main reasons: (1) By a preliminary comparison with the probabilistic, the cognitive, and the computational approaches, I think that Hessean approach is more suitable for investigating the use of analogical reasoning in theorizing than are other approaches. I will defend this claim in terms of comparing my approach with the cognitive approach such as structural mapping theory (SMT). (2) Hesse's approach is more natural than others are. The adjective "natural" is understood in the following sense: Relative to SMT, Hessean approach preserves "pretheoretic similarity" in our natural languages as a necessary element of analogy. Moreover, Hesse's symbolic schema of "dyadic items" best reflects the comparative and contrastive characteristic of the analogical reasoning naturally emerged in our minds. Therefore, I would like to call the framework developed via Hessean approach "natural analogy" – a concept similar to "natural deduction."
My framework of natural analogy revises Hesse's original in the following two ways: (1) Hesse follows logical empiricists' distinction between the formal type and the material type of analogy. In this paper, I will argue that analogy, in the field of scientific theorizing, is both formal and material. To mark this revision of Hesse's framework, I will use a new contrast between "structural/theoretical" and "conceptual/pretheoretical" as two aspects or elements of analogical reasoning to replace the old dichotomy of "formal" and "material" types. The meanings of the new pair of concepts will be elaborated. As a consequence, my framework does not only consider the conceptual/pretheoretical similarities, but also tracks the structural correspondence between two analogues. (2) I modify and expand Hesse's original symbolic schema of dyadic items to build up three new schemas and use them to analyze the role of analogical reasoning plays in scientific theorizing in historical cases. Two symbolic schemas for representing the structure of analogy and the third schema for simulating the cognitive operations of analogical reasoning have been proposed. Those schemas we introduce step by step lead us to suggest that the use of analogy in theorizing can be analyzed into four cognitive operations: abstraction, projection, incorporation, and fitting. I will use the scheme to analyze the process in which Coulomb's law was proposed by analogizing to Newton's law of gravitation, a famous case which is usually deemed as an exemplar of formal analogy, to illustrate the schemas of natural analogy."

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USING REPERTOIRE TO UNDERSTAND PSYCHOTHERAPY

Session 9I

Congress section(s): C4, C5, C7, C8

Psychotherapy is a multidisciplinary field involving psychiatrists, clinical psychologists, and mental health counselors. It is debatable that whether psychotherapy is best considered as part of medical sciences. This is partly because some clinical psychologists and mental health counselors are not trained as medical doctors/scientists. But this paper aims to show that, if we use the notion of repertoire to characterize and understand the psychotherapy field, it is not worthwhile to ask whether or not psychotherapy is part of medical sciences. Ankeny and Leonelli (2016) propose to use the notion of repertoire to characterize the performative, social, financial, and organizational conditions that enable research communities to organize themselves to conduct their professional activities. They originally apply the notion of repertoire to analyze large-scale and multidisciplinary projects in biological sciences. I aim to apply their notion to the psychotherapy field in order to reveal the performative, social, and cultural conditions that enable the psychiatrists, clinical psychologists, and mental health counselors in Taiwan to work together as a professional community. I will use a case study to demonstrate how the notion

of repertoire can be applied to reveal the relevant performative, social, and culture conditions. The case study is about how the practice of family therapy originated in North America are transferred to and adopted by the psychotherapists in Taiwan. Based on the case study, I aim to establish that it is more worthwhile to focus on analyzing the relevant performative, social, and culture conditions that enable Taiwanese psychotherapists to acquire the relevant professional skills and improve them by performing both clinical research and counseling practice. This kind of analysis tracks how professional knowledge in a given community is transferred, replicated, and improved. It captures the content of knowledge and the processes by which a given community produce, maintain, and improve their knowledge. This is what is epistemologically valuable to a given professional community. Thus, instead of debating whether or not psychotherapy is part of medical sciences or not, I propose that it is better to use the notion of repertoire to analyze the relevant conditions that are epistemologically valuable.

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SOME FORMAL AND INFORMAL MISUNDERSTANDINGS OF GÖDEL’S INCOMPLETENESS THEOREMS

Session 19K

Congress section(s): A1, A4, C1

Gödel’s incompleteness theorem is one of the most remarkable and profound discoveries in foundations of mathematics. Gödel’s incompleteness theorems have wide and profound influence on the development of mathematics, logic, philosophy, computer science and other fields. Gödel’s incompleteness theorems raise a number of philosophical questions concerning the nature of logic and mathematics, as well as mind and machine. However, there are ample misinterpretations of Gödel’s incompleteness theorems from the literature and folklore.

In this paper, we will focus on some misinterpretations of Gödel’s incompleteness theorems in mathematics and logic which are not covered in [1]. The motivation of this paper is to review and evaluate some formal and informal misinterpretations of Gödel’s incompleteness theorems and their consequences from the literature and folklore as well as to clarify some confusions based on the current research on Gödel’s incompleteness theorems in the literature.

In this paper, we focus on how recent research on incompleteness clarifies some popular misinterpretations of Gödel’s incompleteness theorems.

Firstly, we discuss some misinterpretations of Gödel’s first incompleteness theorem (G1). Especially, we will focus on the following interpretations or aspects of G1: the claim that there is a truth that cannot be proved; the metaphorical application of G1 outside mathematics and logic; the claim that any consistent formal system is incomplete; the claim that any consistent extension of PA is incomplete; the dependence of incompleteness on the language of the theory; the difference between the theory of arithmetic and the theory of reals;

the claim that Gödel’s proof is paradoxical due to the use of the Liar Paradox;

the difference between the notion of provability in PA and the notion of truth in the standard model; sentences of arithmetic independent of PA with real mathematical content; and the theory with the minimal degree of interpretation for which G1 holds.

Secondly, we discuss some misinterpretations of Gödel’s second incompleteness theorem (G2). Especially, we will focus on the following interpretations or aspects of G2: a delicate mistake for the proof of G2; the vagueness of the consistency statement; the intensionality of G2; the claim that G2 holds for any consistent extension of PA; the claim that there are arithmetic truths which can not be proved in any formal theory in the language of arithmetic; the claim that the consistency of PA can only be proved in a stronger theory properly extending PA; and the claim that G2 refutes Hilbert’s program.

Finally, we discuss the popular interpretation that Gödel’s incompleteness theorems show that the mechanism thesis fails based on the current advances in this field.

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COMMENT ON “DE FINETTI MEETS POPPER”

Session 14C

Congress section(s): B6

The claim that subjective Bayesianism is a form of falsificationism in which priors are rejected in light of evidence clashes with a fundamental symmetry of Bayesianism, $p(A) = 1 - p(A)$, tying together confirmation and falsification of a hypothesis. Wherever there is disconfirmation, there is falsification. This duality arises from the Boolean structure of a probability space. While Popper holds that there is a fundamental asymmetry between the two, subjective Bayesianism must view confirmation and disconfirmation as two sides of the same coin. Moreover, the standard account is that priors are neither falsified nor verified, but are revised in light of accepted evidence, again, dually.

That said, there are forms of Bayesianism that are closer to Popper’s methodology. In particular, one form, which we might term London Bayesianism, has more in common with the Popperian approach than is generally recognized. (I choose the name since this is where it originated, especially in the work of Colin Howson and Peter Urbach: I take it as opposed to the Princeton Bayesianism of David Lewis and his students). This form of Bayesianism is motivated by the acceptance of a negative solution to the problem of induction and a deep scepticism towards mechanical approaches to scientific methodology. In particular, Howson’s rejection of diachronic conditionalization in favour of synchronic conditionalization shifts Bayesianism toward a generalized account of consistency at a given time, away from a view of Bayes Theorem as providing the ideal account of learning from experience (whatever that might be). This also leads to the rejection of countable additivity both by Howson and others.

Finally, as the author points out, standard statistical methodology incorporates an adjustable parameter (levels of significance) for which no independent justification is given. Thus it exemplifies an ad hoc solution to scientific discovery, and so cannot be seen as taking falsifications seriously.

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DECOLONISING SCIENTIFIC KNOWLEDGE: MORALITY, POLITICS AND A NEW LOGIC

Session 20E

Congress section(s): B5

I argue that among the issues in the production and dissemination of scientific knowledge which occupy philosophers of science nowadays is a neglected concern in the form of institutional coloniality that gate keep against epistemologies of the south. This has led to the bordering of scientific knowledge—a segregation that validates own side and invalidates the other side. But should philosophers of science be at the border where standing on one side they draw a line between cultures that have the right to regulate and the power to determine what counts as knowledge and those that don’t? Coloniality of knowledge bases its logic on the pretension that the Western episteme is acontextual and universal and therefore superior to the rest that are not. Here, I employ a decolonial strategy to confront the ethics that seeks to regulate, and the politics that seeks to determine, what counts as knowledge in order to address the residualisation of knowledge formations from the global South. I transcend the approach in extant decolonial literature which confines itself to the remediation of epistemic injustices and offer a new logic that might be able to disborder the philosophy of science.

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IS THERE A HARD PROBLEM FOR THE INTEGRATED INFORMATION THEORY OF CONSCIOUSNESS?

Session 24F

Congress section(s): C5

Consciousness seems particularly hard to fit into our scientific worldview when we consider its subjective aspect. Neurobiological theories that account for consciousness starting from its physical substrate seem unable to explain the problem posed by experience. Why certain neural processes are accompanied by certain experiential features, while others are not? This is the Hard Problem (HP) of consciousness and any theory that attempts to explain this phenomenal feature of reality needs to address it. In this contribution, we discuss how HP affects the Integrated Information Theory (IIT), which today is regarded as one of the most prominent neurobiological theory of consciousness.

By adopting a top-down approach from phenomenology to the mechanism of consciousness, IIT starts with five axioms that characterize the essential properties of every experience (i.e. intrinsic existence, composition, information, integration, and exclusion). Then, it infers for each axiom a corresponding postulate that specifies the properties that physical systems must satisfy in order to generate consciousness. Finally, IIT holds that experience is a maximally irreducible conceptual structure (MICS), which means that there is an identity between the phenomenological properties of experience and causal properties of a physical system (Oizumi et al. 2014, Tononi 2015).

We propose our own analysis of Chalmers' Hard Problem (Chalmers 2010, 2018), the Layered View of the Hard Problem, according to which there is a phenomenal layer and a further conceptual layer that together constitute HP. The first makes subjective experience an explanandum, generating the Monolayered Hard Problem (MHP). The second adds epistemic claims about how an explanation of experience should proceed, given the conceivability of zombies' scenarios, thus creating the Double-Layered Hard Problem (DHP). We take DHP to be the standard Hard Problem as it is discussed in the literature (HP=DHP).

If our analysis is correct, then the relation between HP and IIT depends on the theory's stance on conceivability scenarios and it presents four possible different outcomes. Firstly, regarding MHP, IIT takes the road of nonreductive fundamental explanation and thus can be said to indirectly attempt to solve it. Secondly, the theory directly denies that there is a DHP for it to answer to due to its methodological choice of a top-down approach. Thirdly, IIT indirectly denies that there is in general a DHP, either by allowing only for functionally, but not physically identical zombies (no conceivability), or by holding the necessary identity between an experience and its MICS (no possibility). If our argument is sound, then IIT and HP in their current state cannot be both true: one of them needs to be revised or rejected.

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TOWARD A COEVOLUTIONARY MODEL OF SCIENTIFIC CHANGE

Session 16C

Congress section(s): B3

In this work, I attempt to develop a coevolutionary model of scientific change, which affords a more balanced view on both the continuous and discontinuous aspects of scientific change. Supposing that scientific inquiry is typically goal-directed, I'm led to propose that scientific goals, methods and theories constitute the main components of scientific inquiry, and to investigate the relationships among these components and their changing patterns. In doing so, first of all, I identify explanatory power and empirical adequacy as primary goals of science. But, facing what I call the problem of historical contingency according to which those primary scientific goals could not be justified because they are historically contingent, I explore the possibility of evaluating scientific goals and suggest that several well-motivated measures of evaluating scientific goals allow us to alleviate the problem of historical contingency. Then I try to bring out the major features of how those main components of science are related to each other. One major feature is that they mutually constrain each other, and thus each main component operates as a selective force on the other components. Another major feature is that the main components of science are induced to change reciprocally, but with certain intervals. Considering these features together, I suggest that scientific change is evolutionary (rather than revolutionary), as well as coevolutionary. Further, I claim that there are other important features, which deserve our serious attention. They are the modes and tempos of changes in the main components of scientific inquiry. Firstly, the modes of changes in the main components of scientific inquiry are not homogeneous. That is to say, unlike what has happened in scientific methods and theories throughout the history of scientific inquiry, what I take as primary goals of science seem to have experienced a sort of strong convergence. Secondly, the tempos of changes in the main components of scientific inquiry also are not quite homogeneous. Particularly, the tempo of change in primary goals of science seems to have been much slower than those in method or theory. So I come to conclude that, despite mutually constraining relationships among these main components, what really anchors scientific activities are goals rather than methods or theories. Finally I argue that this coevolutionary model of scientific change does not yield to what I call the problems of circularity and scientific progress. The problem of circularity is that the evaluation process occurring in the coevolutionary model of scientific change is structurally circular. I argue, however, that the changes resulting from evaluating one of the main components of scientific inquiry under the constraints of other components are not circular viciously, but usually self-corrective. Further, the problem of scientific progress results from the observation that my coevolutionary model seems quite similar to Laudan's reticulated model of scientific change. While admitting that there exist significant similarities between the two models of scientific change, I claim that the mode of scientific progress in my coevolutionary model is not transient as what happens in Laudan's reticulated model but transitive.

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PRIORITY MERGE AND INTERSECTION MODALITIES

Session 28D

Congress section(s): A2

Distributed knowledge, defined by taking the intersection of the individuals' indistinguishability relations or information cells, is the standard notion of pooled information in epistemic logic. It is well-suited to represent the pooling of true information. In contrast, intersection of "softer" types of information, for instance plausibility orderings, yields counter-intuitive results. Indeed, the corresponding notion of "distributed belief", defined as the intersection of the individual underlying plausibility orderings on possible worlds, becomes inconsistent as soon as individual opinions diverge too radically. To avoid such inconsistencies, we focus on an alternative way to pool beliefs, the so-called priority (or lexicographic) merge.

Priority merge aggregates plausibility pre-orders as follows. Assume that the group members are somehow ordered in terms of expertise or influence in the group. Then priority merge proceeds by lexicographically considering the strict preferences of each agent in order of priority. Every pair of states strictly ordered by the agent on top of the order is also strictly ordered for the group. For the pairs about which the topmost agent is indifferent, we move to the second agent and order them strictly if she does so, and so on until all pairs are strictly ordered, or we have gone through all agents. Priority merge has been generalized by [2] to arbitrary priority operators which pool any number of relations using priority graphs for agents, i.e., relations that need not be linear nor transitive. Most importantly, [2] shows that any such

priority operator can be represented as a combination of two simple operations. The logical framework proposed here relies directly on this result.
We start with a systematic comparison between the logical behavior of priority merge and the more standard notion of pooling through intersection, for different notions of belief, on multi-agents plausibility models. We then provide a sound and complete axiomatization of the logic of priority merge, as well as a proof theory in labeled sequents that admits cut.
To the best of our knowledge, the only previous logical study of lexicographic merge has been done in extended modal languages [3]. One of the contributions of the present paper is therefore to show that priority merge can also be captured by a modal logic without any hybrid machinery. Finally, we study Moorean phenomena and define a dynamic resolution operator for priority merge, in the same way as [1] does for distributed knowledge, and we provide a complete set of reduction axioms.
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COMMITMENTS OF FOUNDATIONAL THEORIES: INTRODUCTION

Session 26J

Congress section(s): A1

Our objective is to provide a conceptual analysis of the notion of commitments of a foundational theory, i.e., the commitments of a theory that provides a logical platform for the development of significant portions of mathematics. These commitments are, loosely speaking, all the restrictions on the ways the world might be, which we should accept given that we accept all the basic principles of the theory.
The notion of commitments lies at the core of many contentious debates in contemporary formal philosophy (for example, it has been involved in the debate over deflationism or over the role of classical logic in mathematical reasoning). Here we restrict ourselves to discussing two specific types of commitments:
• Epistemic commitments. Given that we accept the axioms and the inference rules of the theory Th, we should also accept some other statements in the language of Th (or possibly, statements in the language of Th extended with the truth predicate). These additional statements are the epistemic commitments of Th.
• Semantic commitments. Given that we accept the axioms and the deductive machinery of the theory Th, some conclusions about possible interpretations of Th should also be accepted. These conclusions are the semantic commitments of Th.
The most direct examples of epistemic commitments are simply the theorems of Th. However, there is also an interesting category of implicit commitments discussed in the literature. It has been claimed (notably, by Feferman 1991) that given your acceptance of a mathematical theory Th, you should also accept some essentially new sentences, unprovable in Th. A typical example of such a sentence is the consistency statement for Th. Another example is the statement that all theorems of Th are true.
Semantic commitments differ from the epistemic ones in that we do not require that they can be described in the language of Th (even after adding the truth predicate to this language). A typical description of such commitments involves explaining how our specific choice of axioms restricts the class of possible models of Th.
A classical paper on the commitments of axiomatic theories is (Feferman 1991), where a method is described permitting us to generate “larger and larger systems whose acceptability is implicit in acceptance of the starting theory”. In this context, several authors emphasised the role of truth, claiming that a proper characterisation of a set of commitments of Th should take the form of a theory of truth built over Th (cf. (Ketland 2005)). We will propose a different strategy, initiated in (Cieśliński 2017), which explains epistemic commitments in purely epistemic terms. In particular, the non-epistemic notion of truth will not play any essential part in the proposed explanation.
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FORMALIZING THE SORITES PARADOX IN MATHEMATICAL FUZZY LOGIC

Session 10M

Congress section(s): A2

The sorites paradox has been intensively discussed in the literature and several competing theories of vagueness have emerged. Given a vague predicate F and a sequence of objects 1, 2, ..., n, such that: F(1) is true, F(n) is false, and for each i, the objects i and i+1 are extremely similar in all respects relevant to the application of F; the sorites paradox is an argument which, based on two apparently true premises F(1) and “for each i: F(i) implies F(i+1)”, after n applications of modus ponens reaches the clearly false conclusion F(n).
The standard account of this phenomenon using fuzzy logic goes back to Goguen: the second premise is almost true — so for ordinary purposes we accept it — but it is not fully true and so the argument is unsound. Hajek and Novak presented an alternative formalization of the sorites paradox aimed at emancipating it from ad hoc aspects of Goguen’s solution while, allegedly, preserving Goguen’s main idea.
To account for the fact that there is a small difference between the truth values of F(i) and F(i+1), they used a new unary connective to reformulate the second premise as “for each i: F(i) implies *F(i+1)” with the intended reading “if F(i) is true, then F(i+1) is almost true”. An easy proof in the natural axiomatic system governing the behaviour of * shows that we can soundly derive the conclusion *^n F(n), where *^n is the consecutive application of * n-times — and this conclusion, unlike the original conclusion F(n), can be accepted as being fully true.
Any solution to the sorites paradox should be able to answer the question why we initially go along with the sorites reasoning, rather than immediately rejecting some instance of the second premise, nor claiming that there is a fallacy — and yet ultimately, when we see where the argument goes, we do not accept the conclusion, but instead begin to lose the feeling that the premises are true and the reasoning is correct.
In our talk we use a general heuristic known as the Ramsey test to show that Hajek and Novak’s formalization yields a natural answer to the above question. We also observe that the formulation of the second premise as “for each i: *(Fe(i) implies Fe(i+1))” better captures Goguen’s original idea, that Hajek and Novak’s axiomatization of * can be used to derive from this premise a different, but also perfectly acceptable conclusion, *^(2^n) F(n), and that this conclusion can also be reached via a weaker notion of “almost true”. Finally we show that a good answer to the above question can still be given once these changes are made.
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DARWIN'S CAUSAL ARGUMENT AGAINST INTELLIGENT DESIGN

Session 31I

Congress section(s): B6, C3

In the Origin of Species, Darwin presents his "one long argument" against the thesis of special creation (SC) and for his alternative hypothesis of evolution by natural selection (ENS). His objections to SC come in two, perhaps contradictory, forms. First, Darwin sometimes argues that some observations, such as imperfect traits, seem to provide evidence against an omnipotent and benevolent creator. More specifically, using the Law of Likelihood, the claim is that for some observations, Pr(O|SC) < Pr(O|ENS), so O is evidence that favors ENS over SC.

However, in other places, Darwin argues that SC makes no predictions at all. Because the goals and intentions of an all-perfect God are unknowable, we can't assign precise likelihoods to various outcomes on the hypothesis of design. Worse, if we make favorable assumptions about the creator, we can generate a high probability for any outcome whatsoever. Hence, Darwin complains that SC makes no genuine predictions or explanations: "On the ordinary view of the independent creation of each being, we can only say that so it is;—that it has so pleased the Creator to construct each animal and plant" (Darwin 1859, Ch. 13).

This latter possibility has come to be known as the Preference Problem (Sober 2008). I explicate a way out of the Preference Problem that Darwin himself found compelling, using the modern tools of causal modeling frameworks. In frameworks obeying the Causal Markov Condition, probabilistic dependencies between two variables are indicative of a causal relationship between them (or between them and some common cause). The Preference Problem states that the design hypothesis can accommodate any probabilistic dependency between observed traits and the designer and hence preserve a causal connection between the two, come what may.

However, Darwin himself held that the most persuasive evidence against the design hypothesis was a particular probabilistic independence, namely, that the variations that occur in a population are probabilistically independent of what would be good for those organisms to possess (in modern terminology, mutation is random with respect to fitness) (Lennox 2010, Beatty 2006). Given certain assumptions, such as faithfulness, probabilistic independencies indicate the absence of a causal connection between two variables. Darwin's insight is that if there were a designer that is a common cause of both variation and natural selection, then we would predict a probabilistic dependence. The design proponent can only capture the data by assuming that a designer would guarantee that faithfulness was violated.

While this gives Darwin a kind of evidence against the SC hypothesis that does not fall victim to the Preference Problem, there are several complications inherent in the causal modeling framework that provide an "out" to the design proponent. I argue that the probabilistic independence Darwin identifies doesn't merely put theological pressure on our conception of a designer, but in fact, standard causal reasoning justifies the inference that there is no such designer.

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THEORETICAL EQUIVALENCE AND SPECIAL RELATIVITY

Session 29L

Congress section(s): B2, C2

Quine [1975] proposes an attractive criterion - later refined by Barrett and Halvorson [2016] - for when two first order systems count as formulations of the same [scientific] theory: a specific sort of translation function must exist between the two languages so as to map one set of axioms into a logical equivalent of the other. Barrett and Halvorson [2016] ask also for a reverse function that returns for each formula a logical equivalent of the original. Elementary philosophical considerations - about the way in which the reference of theoretical terms gets fixed - suggest that equivalent theories are simply notational variants of each other. No rational preference can be had and no distinction about ontology can be made between pairs of intertranslatable theories. Unfortunately few of the interesting cases of theories believed to be equivalent - such as Lagrangian and Hamiltonian mechanics, matrix and wave quantum mechanics or the manifold and the algebraic formulations of general relativity - have been examined under this strict notion of equivalence. A lack of axiomatisations is mainly to blame. In the present work we will begin by liberalizing the class of translation functions admitted by Quine [1975] to include mappings that send a predicate of a fixed arity to predicates of a fixed larger arity. We illustrate the need for this extension by some mathematical examples that we will employ later: the interpretation of the theories of matrices and of polynomials of a fixed degree in the theory of their [field of] coefficients and also that of rational numbers in that of the integers. We will then consider two systems of axioms for light rays moving in an empty Minkowski spacetime. One is a revision of the system of [Andréka, Németi et al. 2011], in which what appear to us as several mistakes in the formulation are corrected. This first system does not appear, at least at first, to assume the existence of spacetime points and merely assigns coordinate values to physical objects. It treats mainly of frames of reference and of the transformations between frames. The second set of axioms is our own and attempts to formalize the geometric account of spacetime given in [Maudlin 2012] and [Malament, unpublished]. This second system assumes the existence of spacetime points, but its vocabulary is purely geometric and it makes no reference to coordinates or to numbers. We will present the axioms of both system and then proceed to prove their equivalence by constructing an appropriate translation manual.

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THE ONTOLOGY OF MASS AND ENERGY IN SPECIAL RELATIVISTIC PARTICLE DYNAMICS

Session 10J

Congress section(s): B4, C2

This paper develops a new account of the relationship between mass and energy in special relativistic particle dynamics. Marc Lange has recently argued that the standard picture of the mass--energy relationship is problematic, and proposed that the alleged conversion between energy and mass is unphysical and merely "perspectival". I reject both the traditional account and Lange's account in this paper. I consider various explanatory puzzles associated with the interpretive role of Lorentz invariance, and I develop a new ontology of special relativistic particle dynamics derived from the geometry of Minkowski

spacetime, according to which genuine cases of mass--energy conversation are fundamentally coordinate-dependent ways of characterizing the interactions between impure 4-forces and massive bodies.

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ROBUSTNESS, INVARIANCE, AND MULTIPLE DETERMINATION

Session 3I

Congress section(s): B1, B6

Multiple determination is the epistemic strategy of using multiple independent empirical procedures to establish "the same" result. A classic example of multiple determination is Jean Perrin's description of thirteen different procedures to determine Avogadro's number (the number of molecules in a gram-mole of a substance), at the beginning of the twentieth century (Perrin 1909, 1913). In the contemporary literature in philosophy of science, multiple determination is considered to be a variant of robustness reasoning: "experimental robustness", "measurement robustness", or simply "robustness", are the terms that are commonly used to refer to this strategy (Wimsatt 1981, 2007; Woodward 2006; Calcott 2011; Soler et al., eds. 2012). In this paper, I argue that the strategy of using multiple independent procedures to establish "the same" result is not a variant of robustness. There are many variants of robustness strategies, but multiple determination is not one of them. I claim that treating multiple determination strategy as a robustness variant mischaracterizes its structure and it is not helpful for understanding its epistemic role and import in scientific research. I argue that there are many features that distinguish multiple determination from the many robustness variants. I present these features and argue that they are related to the same central difference: whereas all the robustness variants can be construed as involving some sort of invariance (of the robust result) to different types of perturbations, multiple determination cannot be so construed. The distinguishing feature of the multiple determination strategy is its ability to support a specific type of a no-coincidence argument. Namely that it would be an improbable coincidence for multiple determination procedures, independently of one another, to establish "the same" result, and yet for the result to be incorrect or an artefact of the determination procedures. Under specific conditions, the no-coincidence argument from multiple determination - in addition to being used to argue for the validity of the result - can be also used to argue for the validity of the determination procedures. No such no-coincidence argument can be construed from simple invariance to perturbations. Robustness is a set of epistemic strategies better suited for discovering causal relations and dependencies.
Finally, I claim that, besides the philosophical reasons, there are also historical reasons to keep multiple determination and robustness distinct. Multiple determination can be considered to be the historical descendant of William Whewell's nineteenth century notion of consilience of inductions (a form of hypothetico-deductive reasoning). On the other hand, robustness strategies can be considered to be the descendants of John S. Mill's nineteenth century methods of direct induction (a form of inductive reasoning).
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FEYERABEND AND THE RECEPTION AND DEVELOPMENT OF LOGICAL EMPIRICISM

Session 6G

Congress section(s): B6

As is well known, in his early intellectual trajectory, Feyerabend mounted a sustained assault against logical empiricism. More precisely, in the second half of the 1950s Feyerabend found fault with Carnap's criterion of empirical meaningfulness whereas in the first half of the 1960s he repeatedly targeted Nagel's account of reduction and Hempel's account of explanation. The aim of this paper is to examine the far less-known responses of Hempel and Nagel to Feyerabend's criticism. This will allow to get a better view both of Feyerabend's reception of logical empiricism and of the impact that Feyerabend's stimuli had on its later development.
In a series of papers published between 1962 and 1966, Feyerabend deployed a two-pronged attack on the 'orthodox' accounts of reduction and explanation, focusing on the descriptive adequacy and on the normative desirability of two basic principles or conditions that Feyerabend identified as underlying Nagel's and Hempel's proposals: (a) the deducibility, or at least the consistency, between the reducing and the reduced theories and between the explaining law or theory and the description of the phenomenon to be explained; and (b) the even more fundamental meaning invariance between the main descriptive terms of the theoretical frameworks involved in reduction or explanation. Feyerabend claimed that Hempel's and Nagel's accounts failed as their assumptions were both inaccurate with respect to the history of science, as they do not reflect actual scientific practice, and objectionable with a view to the progress of scientific knowledge, as they would promote a conservative approach favouring traditionally well-established and entrenched theories as against novel ones.
Feyerabend's vocal criticism shook North American philosophy of science, then largely dominated by logical empiricism, and prompted Hempel's and Nagel's replies, which appeared in print in the second half of the 1960s and in the early 1970s. Hempel and Nagel readily conceded some of Feyerabend's points, such as the unsoundness and undesirability of the methodological rules exposed by Feyerabend; only to retort that these did not warrant Feyerabend's most radical conclusions. Hempel clarified that Feyerabend's methodological analysis was 'completely mistaken' and that Feyerabend offered 'no support' for his allegations; whereas Nagel was downright dismissive about what he considered Feyerabend's 'absurd remarks' and 'deliberate distortions'. Indeed, it seems that, although Feyerabend was steeped in the relevant literature, he substantially misunderstood the logical empiricist research programme, which, as emphasized by recent scholarship, revolved around a conception of philosophy of science as conceptual engineering and the ideal of explication. On the other hand, Hempel also recognised that Feyerabend's insistence on the theory-ladenness affecting the meaning of any descriptive term, despite having been long acknowledged by logical empiricism, could have more far-reaching semantic and methodological consequences than previously envisaged and that these deserved to be thoroughly discussed. In this respect, Hempel's later and more sophisticated attempts to provide a rational reconstruction of the structure of scientific theories can be seen at least partly as the result of Feyerabend's challenging insights.

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EXTENSIONALIST EXPLANATION AND SOLUTION OF RUSSELL'S PARADOX

Session 20A

Congress section(s): C1

In this paper, I propose a way out from the aporetical conclusion of the debate about the, so-called, explanation of Russell's paradox. In this debate, there are traditionally two main and incompatible positions: the "Cantorian" explanation and the Predicativist one. I briefly rehearse the reasons why both these ones can be neglected and propose a third, "Extensionalist", one.
The Extensionalist explanation identifies the key of Russell's Paradox in a proposition about the extensions: $\forall F \exists x(x = ext(F))$, which allows to derive, from the existence of Russell's concept, the existence of Russell's extension. This proposition

is a theorem of classical logic whose derivation presupposes the classical treatment of identity (Law of identity) and quantification (Law of Specification and Law of Generalisation). So, we can explain Russell's paradox by the (inappropriate) classical correlation between concepts and extensions: the flaw of this correlation does not consists (as in the Cantorian explanation) in the injective feature of the correlation but (as in the Predicativist explanation) in its domain, namely in the implicit assumption that it is defined on the whole second order domain; however this result does not mean that, for restoring consistency, the whole second order domain has to be restricted (as in the Predicativist solution) but only the domain of the extensionality function.

The solution related to the Extensionalist explanation consists in a reformulation of Frege's theory which allows the derivation of Peano Arithmetic as a logical theory of extensions. The new language L comprises two sorts of first order quantifiers (generalised Π, Σ, and restricted ∀,∃) respectively governed by classical and by negative free logic. From a syntactic point of view, the proposed system consists of the axioms of propositional classical logic, some specific axioms of predicative negative free logic, an impredicative comprehension's axioms schema and, as the only non logical axiom, a version of Basic Law V (with a generalised universal first order quantification) restricted to the existents abstracts: ∀F ∀G(ext(F) = est(G) ↔ ∃x(x = ext(F)) ∧ Πx (F x ↔ Gx)). This version of Basic Law V characterises the behaviour of the correlation denoted by "ext" as functional and injective only for a subset of second order domain, which excludes Russell's concepts. Then, this system does not allow to derive Russell's Paradox.

From a semantic point of view, the interpretation of the theory is provided by a model M = <D, D0, I> , in which D is the domain of restricted quantification (such that D ⊆ D0), D0 is the domain of generalised quantification and I is a total interpretation function on D0. The symbol "ext" is interpreted as a partial injective function from a subset of the power set of D0 in D.

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FOUR REALIST THESES OF MARIO BUNGE

Session 9F

Congress section(s): B1, B7, C2

4.1. The Ontological Thesis

Bunge upholds the existence of a world independent of the mind, external to our thinking and representations (Ontological Thesis). His supporting reasoning on this matter draws from both general considerations as well as some of the special sciences.

4.2. The Epistemological Thesis

Bunge complements the previous proposal with an epistemological thesis made of three major claimS:

4.2a. It is possible to know the external world and describe it at least to a certain extent. Through experience, reason, imagination, and criticism, we can access some truths about the outside world and ourselves.

4.2b. While the knowledge we thus acquire often goes beyond the reach of the human senses, it is multiply problematic. In particular, the resulting knowledge is indirect, abstract, incomplete, and fallible.

4.2c. Notwithstanding its imperfections, our knowledge can be improved. Bunge accepts that theories are typically wrong as total, unqualified proposals. In his opinion, however, history shows with equal force that successful scientific theories are not entirely false, and also that they can be improved.

4.3. The Semantic Thesis

This component of Bunge's realism is framed by the previously stated ontological and epistemological theses. It comprises four interrelated ideas:

4.3a. Some propositions refer to facts (as opposed to only ideas).

4.3b. We can discern the proper ("legitimate") referents of a scientific theory by identifying its fundamental predicates and examining their conceptual connections in order to determine the role those predicates play in the laws of theory.

4.3c. Some factual propositions are approximately true.

4.3d. Any advance towards the truth is susceptible of improvement.

4.4. Methodological Thesis

The fourth facet of Bunge's realism I am highlighting focuses on methodology and comprises at least three proposals: (a) Methodological scientism, (b) Bunge's version of the requirement that theories must allow for empirical testing, and (c) a mechanistic agenda for scientific explanation.

4.4a. Scientism asserts that the general methods developed by science to acquire knowledge provide the most effective available exploration strategy at our disposal. The methods of science—whose main use is given in the development and evaluation of theories—use reason, experience, and imagination.

4.4b. A theoretical proposal should lead to distinctive predictions, and it should be possible to subject at least some of those predictions to demanding empirical corroboration.

4.4c. According to Bunge, we cannot be satisfied with merely phenomenological hypotheses of the "black box" type (i.e., structures that do not go beyond articulating correlations between observable phenomena). Good methodology, Bunge insists, presses for further exploration, prompting us to search the world for regularities at deeper levels that provide illuminating explanations of the discovered regularities—ideally "mechanical" ones.

The realism project that Bunge articulates seems, therefore, to have some major issues still pending. Meanwhile, however, I think there is no doubt that Mario Bunge will continue to make valuable contributions in this and other areas of the realist project, responding with honesty and clarity to the enigmas posed by the most intellectually challenging fundamental theories of our time.

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FUNCTIONAL ONTOLOGIES AND REALISM: THE CASE OF NUCLEAR PHYSICS

Session 19G

Congress section(s): B3, B4, C2

The existence of enduring inconsistent models for the same scientific object can ruin attempts to interpret them realistically. One representative case is the proliferation of contradictory models of atomic nuclear structure that make radically different assumptions about the same thing: the nature of the nucleus. The conceptual tensions between the various models have prompted forceful anti-realist charges in recent literature. A close examination of the case, however, suggests that its antirealist import is exaggerated. I argue that the more successful nuclear models exemplify opportunities for crucial resources of selective realism, particularly functional ontologies.

One major complication in modeling nuclear structure is that atomic nuclei are both too large for direct calculation and too small for the use of statistical approaches. Treating nuclei as bound states of nucleons, physicists work out semi-experimentally an effective two-nucleon potential, which in principle allows for the calculation of the eigenstates and energies of multi-nucleon systems. In practice, however, the number of nucleons in nuclei with A> 3 makes the calculation intractable. So, guided by basic quantum mechanics and experimental findings, physicists from the 1930s on have constructed various models for nuclear structure, each with some significant (if limited) success. Most nuclear physicists think that considerable understanding has been achieved in this way. Still, nuclear physics is commonly regarded as an ugly duck in basic science because its approaches lack fundamentality, "united" only at a very abstract level to the "fundamental" theory of nucleon-nucleon interaction.

So, to what extent (if any) can those nuclear models receive realist interpretation? Taken as whole constructs, all current nuclear models are patently false. But then, as whole constructs, all empirical theories are false according to general good sense. Larry Laudan (1981) turned this insight into an ambitious anti-realist argument that realists forcefully challenge. Bringing the matter home to the nuclear case, some of the responses given to Laudan help to articulate realist reactions to the situation of nuclear models, or so I suggest in this contribution.

The realist strategy I suggest goes roughly as follows. Typically, in modern scientific fields, successor theories retain parts of earlier theories, deeming them prospectively truthful. This applies, in particular, to "functional" versions that bypass

incommensurability charges by limiting their descriptive focus to abstract theoretical levels of lesser fundamentality and scope and greater coarse-graining than they had in the original proposal. Accordingly, on the selective approach I propose, realist commitment goes primarily to "functional" theories or parts thereof. The entities and regularities of realist import are identified by what they do rather than by what they "ultimately are" or are made of. A functional theory [T] comprises existential claims and laws (drawn from a fuller theory T but now more restricted, abstract, and coarse-grained). Realist selection of [T] out of a theory T amounts to asserting that the kinds of entities and regularities spelled out in [T] are real (i.e., they are at play in the intended domain). Functional models can originate in radically different ideas associated with different, incompatible ontological foundations—i.e., functional models are schematic and typically present multiple realizability. As such, they can be accepted as an accurate description of underlying stuff much more easily than their fuller base models, without contradiction. The abstraction associated with each [T]-version of a theory responds to specific interests and focuses on just some aspect of the world, but its application is objective. The very same object can belong in very different functional ontologies over different domains of interest. The "ordinary" description of an apple differs markedly from its molecular description ("Eddington's apple"), yet the two descriptions are "correct," each over different concerns and regimes of size and energy. Both tell precise stories about the modal structure of an apple in their intended contexts.

A recent paper Margaret Morrison (2011) articulates well-structured complaints about the leading models of nuclear structure. She gives perceptive expression to the interpretive unease provoked by much of nuclear physics. Accordingly, I focus on her analyses and consider the main charges she raises, roughly in order of strength, and then suggest replies selective realists can offer, particularly to extant charges that the models' lack unification, are incomplete, show explanatory failure, empirical underdetermination, and contain false central posits. If the analyses I propose are correct, then the noted antirealist allure of nuclear models is largely an artifact of an epistemological overvaluation of fundamentality in the philosophy of physics.

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THE METHODOLOGICAL TRADITION OF EXPLICATION

Session 18C

Congress section(s): B1

A distinction can be made between three kinds of systematic investigations into explication: (i) Some investigators are interested in the puzzles sourrounding explication, e.g. the analysis paradox ([13], [12]). (ii) Other investigators are interested in >getting explication straight< by means of a structural account ([1], [8]) or a systematic distinction between different kinds of explication ([3]). They sometimes need to presuppose, that certain puzzles of explication are solved in one way or another. (iii) Finally, some investigators are interested in making methodological contributions in the sense that they either concentrate on setting up an explicative method which is able to instruct potential explicators in performing an explication or they enhance a preexisting method of explication by governing the performance of subsidiary activities (e.g. explicandum clarification). These scholars are providing a (partial or full) procedural account of explication. In most cases these investigators rely on a more or less explicit structural account of explication and thus, by extension, on certain answers to some puzzles of explication ([2], [5], [7], [9, sect. II.3.c], [10], [11]). -- The proposed ternary distinction is not a strict trichotomy since some investigations fall into more than one category.

In my talk I will first sketch this distinction by means of some examples. This will include some remarks on how the three kinds relate to one another and a brief outline of the tradition associated with the third kind of investigations. Special attention will be given to the purposes of explication in philosophy and in other contexts (including, but not limited to, the sciences and humanities). In particular, I will illustrate to what extent different purposes of explication were recognized in procedural accounts of explication. Evidently, investigations of the third kind have a normative dimension -- explicators can see themselves as being directed by procedural accounts of explication. This internal normativity is affected by the external normativity of language in general, which is one main theme in the recent debate on different kinds of conceptual engineering ([6, ch. 13]). Thus, the question arises as to how various purposes of explication relate to the normativity of these accounts and to the normativity of language in general.

The considerations will allow for a conscious approach to the task of establishing a method of explication which takes into account explicative purposes and the normative pressure exerted on explicators. While the elaborations could be converted for the application to other kinds of conceptual engineering, in the presentation this possibility will only be hinted at. Instead I will assess one specific procedural account of explication, which can be framed in formal or informal terms. It will be put forward in conjunction with a classical example of an explication ([4]) in order to avoid an assessment which is too remote from reality because of idealized perspectives on explicative purposes and the normativity of explication. The aim is to arrive at a procedure which is applicable and which can be thought of as having been applied.

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METAPHYSICAL PLURALISM: BETWEEN SKEPTICISM AND TRIVIALIZATION?

Session 9J

Congress section(s): B4

In an early 2018 paper, Hasok Chang argued that pluralism is fully compatible with scientific realism, contrary to a common, wide-spread impression. In this way, Chang gets on board along with Nancy Cartwright (1999), John Dupré (1993) and Matthias Lynch (1998) in defending a more robust type of scientific pluralism –a metaphysical pluralism. It claims that a pluralist stance is the adequate attitude not only to account for the plurality of scientific theories and their multidimensional empirical success, but also to regard reality: metaphysical pluralists hold that reality is not unique but rather plural, rich in properties and things (Cartwright 1999: 1), and science may have literal knowledge of such plurality. They claim that one can grant "the reality of phlogiston and positrons in the same sense as the reality of "medium-sized dry goods" (Chang 2018: 181). To make sense of such claims, metaphysical pluralists decline that science may have epistemic access to Reality in itself (the Kantian noumenon). They rely on a Kantian correlational structure (Lynch 1998: ch.1, Chang 2018: 181-182) in which cognitive agents play an active role in knowledge: ontological claims are always relative to a conceptual framework

that constitutes the knowledgeable reality. Metaphysical pluralists claim that their stance is better than scientific monism in accounting for scientific practice and what the world is like.
In this paper, we shall argue that metaphysical pluralism falls quite short of its intending aims. We shall claim that metaphysical pluralism is either a sort of scientific skepticism or is at risk of trivialization. Firstly, we shall show that scientific monism and metaphysical pluralism are not even competing to the extent to which they speak of reality differently: the metaphysical pluralist is pluralist with respect to a deflated, constituted reality, though it is forced to accept, as the scientific monist does, that a unique, unknowable Reality must exist (for she supports a somewhat Kantian structure). However, the scientific monist claims that Reality is unique (as the metaphysical pluralist does), but she claims that such a Reality is knowable. So, they are not metaphysically competing about if reality is unique or plural ('plural' and 'unique' are predicates of different types of realities), but whether Reality is knowable or not. To support that Reality in itself is unknowable is a skeptic thesis, therefore it is an epistemic thesis, not a metaphysical one. Secondly, we will show that in order for metaphysical pluralism to be non-trivial, it must be able to sharply draw a distinction between "having a difference concept of x" and "having a different belief about x". As the metaphysical pluralist cannot draw this distinction, her position is at risk of trivialization.
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ANOMALOUS AVERAGES, BOSE-EINSTEIN CONDENSATION AND SPONTANEOUS SYMMETRY BREAKING OF CONTINUOUS SYMMETRIES, REVISITED

Session 4J

Congress section(s): C2

The study of the so-called spontaneous symmetry breaking (SSB) of continuous symmetry, is a fundamental notion in quantum statistical physics [1-7], specifically in the phase transitions theory (PTT).
At finite volume, the breaking of a continuous symmetry (U(1) symmetry group) is associated with many infinitely degenerated ground states connected between them by unitary transformations. In this sense, these states are physically equivalent, having the same energy, and being the ground state of the system understood as a superposition of them. However, in the thermodynamic limit, these connections vanish, and an infinite collection of inequivalent ground states, orthogonal to each other, arise.
On the other hand symmetries can be broken by small disturbances. Mathematically speaking, the disturbance once chosen and provided that the parameters on which it depends are fixed, selects a unique ground state for the system (vacuum).
In this sense, in the framework of the study of the free Bose gas and in the case of a superfluid model N. N. Bogoliubov [6] eliminates the above mentioned degeneracy by introducing a small term on the original energy operator, preserving the self-adjointness but suppressing the symmetry corresponding to the total number conservation law. In this context the limit thermal averages defined by using these perturbations of the original energy operators have been denominated Bogoliubov quasiaverages (QA) or anomalous averages (AA).
It must be taken into account that both Bose condensation, understood as macroscopic occupation of the ground state, as SSB of the continuous symmetry occurs only in the thermodynamic limit which in real physical systems is never reached. In this sense, the introduction of an external field does not explain by itself the broken of symmetry. Moreover an underlying question is whether there is only one restricted class of perturbations, constituted by operators associated to the same sort of particles in the condensate, compatible with the chosen order parameter and with the existence of pure states and ODLRO (off diagonal long range order) [7].

The scenario becomes even more complicated considering that all current experiments are carried out on finite atom systems (trapped Bose gases for which the total number conservation law is preserved).
In this work, the aforementioned problems, their consequences on the fundamental principles of the quantum PTT and the viability of experimentally testing the QA approach shall be discussed.
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ON THE INFINITE GODS PARADOX VIA REPRESENTATION IN CLASSICAL MECHANICS

Session 11J

Congress section(s): C2

The Infinite Gods paradox is introduced by Benardete (1964) in the context of his metaphysical problems of the infinite. Priest (1999) starts the discussion with the publication of a logical analysis and then follows the argument by Yablo (2000) in which he defends that the paradox contains a logical impossibility. This last conclusion achieves a broad acceptance in the scientific community but reasonings introduced by Hawthorne (2000), Uzquiano (2012) and Pérez Laraudogoitia (2016) imply the questioning of that idea.
Contextualised in this discussion, my communication is based on the introduction of a proposal for a representation of the Infinite Gods paradox in the strict context of Classical Mechanics. The objective of following such a methodology consists in deepening in the understanding of the paradox and clarifying the type of problem that underlies it using the analytical power of Classical Mechanics.The methodology consisting in analysing a metaphysical paradox in the context of a specific theory is in line with what Grümbaum (1967) defended concerning the analysis of the supertasks and has later been followed by other philosophers of science who introduce proposals of representation for different paradoxes of the infinite. Nevertheless, no strictly mechanical representation of the Infinite Gods paradox has been published yet.
The results of my mechanical analysis are in agreement with the violation of the "Change Principle" introduced by Hawthorne (2000). But in clear contrast to his contention, this is not a big metaphysical surprise but a simple and direct consequence of causal postulates implicit in Classical Mechanics. Furthermore, the analysis via my mechanical representation shows in a very simple way that the necessary condition that Uzquiano (2012) proposes for the existence of a "beffore-effect" is refutable. Finally, it also leads to conclude that the problem that underlies the paradox is not logical but causal, and thus, is in clear opposition to the reasoning defended by Yablo (2000). Consequently, next objective consists in explaining the diagnosis of what I consider is erroneous in this last argument.
In addition to the achievement of the main objective consisting in deepening in the understanding of the paradox and clarifying the type of problem that underlies it, the analysis of the problem of evolution via my mechanical representation possibilitates clarification on the type of interaction in it. This in itself is a conceptually interesting result in the theoretical context of Classical Mechanics.
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BRIDGING THE GAP BETWEEN PROOF TEXTS AND FORMAL PROOFS USING FRAMES AND PRSS

Session 13A

Congress section(s): C1

We will debate how different layers of interpretation of a mathematical text are useful at different stages of analysis and in different contexts. To achieve this goal we will rely on tools from formal linguistics and artificial intelligence which, among other things, allow to make explicit in the formal representation information that is implicit in the textual form. In this way, we wish to contribute to an understanding of the relationship between the formalist and the textualist position in the investigation of mathematical proofs.
Proofs are generally communicated in texts (as strings of symbols) and are modelled logically as deductions, e.g. a sequence of first order formulas fulfilling specified syntactical rules. We propose to bridge the gap between these two representations by combining two methods: First, Proof Representation Structures (PRSs), which are an extension of Discourse Representation Structures (see Geurts, Beaver, & Maier, 2016). Secondly, frames as developed in Artificial Intelligence and linguistics.
PRSs (Cramer, 2013) were designed in the Naproche project to formally represent the structure and meaning of mathematical proof texts, capturing typical structural building blocks like definitions, lemmas, theorems and proofs, but also the hierarchical relations between propositions in a proof. PRSs distinguish proof steps, whose logical validity needs to be checked, from sentences with other functions, e.g. definitions, assumptions and notational comments. On the (syntacto-) semantic level, PRSs extend the dynamic quantification of DRSs to more complex symbolic expressions; they also represent how definitions introduce new symbols and expressions.
Minsky (1974) introduces frames as a general "data-structure for representing a stereotyped situation". 'Situation' should not be understood too narrowly, as frames can be used to model concepts in the widest sense. The FrameNet project prominently applies frames to represent the semantics of verbs. For example, "John sold his car. The price was € 200." is interpreted as meaning that the second sentence anaphorically refers to the "price" slot of "sell", which is not explicitly mentioned in the first sentence.
In the context of mathematical texts, we use frames to model what is expected of proofs in general and specific types of proofs. In this talk, we will focus on frames for inductive proofs and their interaction with other frames. An example of the interaction of different proof frames is the dependence of the form of an induction on the underlying inductive type, so that different features of the type (the base element and the recursive construction[s]) constitute natural candidates for the elements of the induction (base case and induction steps).
The talk will show how to relate the two levels (PRSs and frames), and will sketch how getting from the text to a fully formal representation (and back) is facilitated by using both levels.

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ECONOMIC SCIENCES AND THEIR DISCIPLINARY LINKS

Session 17F

Congress section(s): C7

For Mäki (2002: 8) the notion of economics constitutes a dangerous mélange of notions: 'there is no one homogeneous economics'. Backhouse and Medema claim that 'economists are far from unanimous about the definition of their subject' (2009: 223). However, John Stuart Mill, Carl Menger and Neville Keynes, 'methodological precursors' of economic science, have developed useful definitions and classifications of economic sciences.
Mill distinguishes:
- The 'Teleological' art of definition of the ends of economic actions, a normative discipline (1882: 657);
- 'Political Economy', an 'abstract' science considering only economic motives: 'the desire of obtaining the greatest quantity of wealth with the least labor and self-denial' ([1844] 2006: 323).
- The 'art of economic practice', considering all motives influencing actual economic phenomena.
Menger's classification is this (1960 and [1883] 1985):
1. The 'historical sciences of economics': economic statistics and economic history that investigate concrete economic phenomena.
2. 'The morphology of economic phenomena, whose function consists in classifying economic facts' (1960: 14).
3. 'Economic theory, which has the task of 'investigating and establishing the laws of economic phenomena, i.e., the regularities in their coexistence and succession, as well as their intrinsic causation' (1960: 14). It has the role of demonstrating (Darstellung) and understanding (Verständnis) (1960: 7) economic phenomena. It has two orientations: the 'realistic-empirical' and the 'exact'. The former uses the Baconian induction that cannot reach at universal laws, but general tendencies ([1883] 1985: 57). The later seeks 'to ascertain the simplest elements of everything real' arriving at forms qualitatively strictly typical ([1883] 1985: 60).
4. Practical or applied economics, with its specific method (1960: 16, 21-22).
Concerning Keynes, he distinguishes 'positive science', 'normative or regulative science' and 'an art' ([1890] 1955: 34-35), respectively dealing with 'economic uniformities, economic ideals and economic precepts' ([1890] 1955: 31, 35).
This paper will construct a classification of economic sciences based on the previous, it will characterize the different disciplines arising from it, and will analyze their disciplinary relations, whether they are multidisciplinary, crossdisciplinary, interdisciplinary or transdisciplinary, according to Cat's (2016) systematization of these concepts.

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A CONSTRUCTIVIST APPLICATION OF THE CONDORCET JURY THEOREM

Session 18K

Congress section(s): B2, C1, C7

The Condorcet Jury Theorem (CJT) tells us (roughly) that a group deciding on a yes or no issue by majority voting will be more reliable than any of its members, and will be virtually infallible when the number of members tends to infinity, provided a couple of conditions on individual reliability and independence are in place. Normally, the CJT presupposes the existence of some objective fact of the matter (or perhaps moral fact) F, whose truth (or desirability) does not depend on the method used to aggregate different opinions on whether F holds/ should hold. Thus, the CJT has been vindicated by advocates of epistemic democracy (with some caveats), while this move has typically been unavailable to authors with sympathies for proceduralist or constructivist accounts.
In this paper I suggest an application of the CJT in which the truth/correctness of F is a direct result of the action of voting. To this effect I consider a n-person generalization of the stag hunt game, in which a stag is hunt only if there is a majority of hunters choosing stag. I show how to reinterpret the independence and competence conditions of the CJT to fit the example, and how to assess the import of the infallibility result in the present context of discussion. As a result of this we are able to identify both a selfish and a cooperative instance of the theorem, which help us draw some morals on what we may call 'epistemic optimism'. More generally, the proposal shows that we can establish links between epistemic and purely procedural conceptions of voting; this, in turn, can lead to novel ways to understand the relation between epistemic and procedural democracy.

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BERNARD BOLZANO'S 1804 EXAMINATION: MATHEMATICS AND MATHEMATICAL TEACHING IN EARLY 19TH CENTURY BOHEMIA

Session 19B

Congress section(s): B6

We shall present here a manuscript containing Bernard Bolzano's written examination to become professor of elementary mathematics at Prague University. This examination took place in October 1804, and consisted of a written and an oral part. Only two candidates took part to it: Bernard Bolzano and Ladislav Jandera. The latter won. The committee asked three questions to the candidates : to find the formula of the surface and volume of a sphère, to find the formula which measures the speed of water filling a tank, to explain the proof of the law of the lever. In our talk, we shall analyze Bolzano's answers, especially to the first question, in light of his later reflections on the foundations of mathematics. This document represents an important source to understand both the evolution of Bernard Bolzano's mathematical thought and, more generally, an important source on the practice of teaching in early 19th Century Bohemia.

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HOW PRAGMATISM CAN PREVENT FROM THE ABUSES OF POST-TRUTH CHAMPIONS

Session 16G

Congress section(s): B5

How to establish if a sentence, a statement, or a theory are true has become a problem of public relevance. To defend that scientific knowledge is not a privileged way for understanding the reality and, therefore, that there are not good reasons for using science as the basis for committing some decisions, has grown a widespread argument. Even prevalent relativistic conceptions about science, like Fuller's, defend the acceptance of the post-truth: "a post-truth world is the inevitable outcome of greater epistemic democracy. (...) once the instruments of knowledge production are made generally available—and they have been shown to work—they will end up working for anyone with access to them. This in turn will remove the relatively esoteric and hierarchical basis on which knowledge has traditionally acted as a force for stability and often domination." (Fuller, 2016: 2-3). However, epistemic democracy does not necessary lead to the acceptance of that position. As the editor of Social Studies of Science has pointed out: "Embracing epistemic democratization does not mean a wholesale cheapening of technoscientific knowledge in the process. (...) the construction of knowledge (...) requires infrastructure, effort, ingenuity and validation structures." (Sismondo, 2016: 3). Post-truth, defined as "what I want to be true is true in a post-truth culture (Wilber, 2017, p. 25), defends a voluntaristic notion of truth, and there is nothing democratic in individualistic and whimsical decisions about the truthfulness of a statement. For radical relativism scientific consensus is reached by the same kind of mechanisms as in other social institutions, i.e. by networks that manufacture the "facts" using political negotiation, or by other ways of domination.
However, the notion of truth that relativists (for instance Zackariasson, 2018: 3) are attacking is actually a straw man: the "God's eye point of view" that very few among philosophers or scientists, defend any more. We suggest that an alternative to post-truth arguments, that at the same time suggests mechanisms for developing a real epistemic democracy, is the notion of truth from pragmatism. This could seem controversial, if we have in mind the debunked and popularized version of pragmatism —the usefulness of an assertion is the only thing that counts in favour of it being true—. Nevertheless, whether among classic pragmatists as Dewey or neo-pragmatists (e.g. Kitcher, 2001), the construction of scientific knowledge, with all its limitations, is the best way for reaching if not the Truth, at least partial or rectifiable, but reliable and well-built knowledge.
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EXPERIMENTS IN HISTORY AND ARCHAEOLOGY: BUILDING A BRIDGE TO THE NATURAL SCIENCES?

Session 12A

Congress section(s): B1, B3, C7

The epistemic challenges to the historical sciences include the direct inaccessibility of their subject matters and limited empirical data whose scope and variety cannot be easily augmented. The output of historiographic research is rarely in the form of universal or general theory. Nonetheless, these properties do not distinguish the historical sciences from other disciplines. The historical sciences have been successful in generating knowledge of the past.
One of the methods common to the natural sciences that historians and archaeologists pursue in order to bridge different academic cultures is the experimental method, most clearly manifest in experimental archaeology. This paper examines the use of experiments in historical and archaeological research and situate them in relation to contemporary philosophies of historiography. Experiments in historiography can take many forms – they can be designed based on textual, pictorial, or other non-textual evidence including fragments of artefacts; they can take place inside the laboratories or in the field. Designers of experiment can aim to describe an exact occurrence in the past (e. g. specific event), types of production techniques, to interpret technical texts, or to inquire into the daily life of our ancestors. However, can the results of such experiments cohere with other scientific historical methods? Can experiment in archaeology truly verify or falsify historiographic hypotheses? Is the experimental method suitable for historical research and to what extent? How do we represent the results of experimental archaeology? These questions accompanied by individual examples of experimental archaeology are discussed in relation to the constructivist approach to historiography and in relation to historical anti-realism. It is argued that despite the fruitfulness of some experiments, their results generally suffer from the same underdetermination as other historiographic methods and theories.

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WHY EPISTEMIC PLURALISM DOES NOT ENTAIL RELATIVISM

Session 8J

Congress section(s): B1, B4, B6

There is a widespread view according to which the denial that the conditions of knowledge are truth-evaluable inevitably leads to a form of epistemic pluralism that is both quietist and internally incoherent. It is quietist because it undermines the possibility of genuine epistemic disagreement. It is internally incoherent because it simultaneously denies the existence of universal knowledge claims and makes the universal claim that there is no such knowledge. The goal of this paper is to show that denying that the conditions of knowledge are truth-evaluable does not necessarily entail a commitment to a form of epistemic relativism that is both quietist and internally incoherent.
The paper begins, in section I, by considering Boghossian's characterization of epistemic pluralism in Fear of Knowledge. (Boghossian 2006) According to Boghossian, the descriptive claim that there are different belief systems, combined with the denial that the conditions of knowledge are truth-evaluable, leads to a questionable form of epistemic pluralism, one which is relativist in outlook. I consider Boghossian's account of epistemic pluralism because it seems to capture the widespread view that pluralism and relativism must go hand in hand if one denies that the conditions of knowledge have truth-values. Section II outlines a form of epistemic pluralism that, I argue, is unfairly described as relativistic. This form of non-relativistic pluralism arises not in response to the descriptive claim that there is a plurality of belief systems, but to the normative claim that explanation should be fit for purpose. Once pluralism is conceived in this light it no longer has the quietist overtones that tend to be characteristic of epistemic relativism. The distinction between the relativistic pluralism that is the target of Boghossian's critique and non-relativistic pluralism is illustrated through a reconstruction of Collingwood's account of absolute presuppositions in An Essay on Metaphysics, a highly neglected but important contribution to hinge epistemology. This reconstruction is offered as an illustration of how hinge epistemology could be construed to avoid two standard objections that are often raised against epistemic relativism, namely that it makes it impossible to criticise other cultures and that it is self-undermining. Section III considers these two standard objections and argues that they do not apply to the kind of epistemic pluralism that arises from the consideration that explanation must be fit for purpose and sensitive to the goals of inquiry.
The paper concludes by arguing that the decoupling of epistemic pluralism from epistemic relativism argued for in this paper demonstrates that invoking the threat of postmodern relativism to bolster the correspondence theory of truth amounts to a form of philosophical scaremongering.

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UTOPIAS IN THE CONTEXT OF SOCIAL TECHNOLOGICAL INQUIRY

Session 13J

Congress section(s): B6, C7

This paper elaborates on Otto Neurath's proposal that utopias can be used in social scientific and technological methodology of research. In Foundations of the Social Sciences (1944), Neurath claims that such imaginative works may provide means for scientists to overcome the limitations of existing social arrangements in order to devise alternatives to experienced problematic situations.
I compare this point of view with the work scientists do with models and nomological machines in Nancy Cartwright's conception, as presented in The Dappled World (1999). That is, utopias are abstractions that depict the complexity of social arrangements and that provide idealized situations to which our understanding of some aspects of society applies. As models, they enable scientists to visualize the functioning of social scientific laws and generalizations, as well as new possibilities, since models allow the operation of features and consequences of the abstract arrangements. In this operation scientists acquire knowledge not only of imagined arrangements, but also of concrete social institutions, since models mediate between more abstract and more concrete parts of scientific experience. But how does this mediation take place? That is, why is that knowledge valid?
A common answer to this question in the recent controversy on models in philosophy of science assumes some form of (more or less mitigated) scientific realism: that scientific models represent some features of reality. Such an answer can be found in Cartwright's proposals, since she claims that scientific models and nomological machines instantiate real capacities of the modeled systems. This stance seems not to be compatible with an account of the complexity of social situations, which have many concurring causes that are not always describable in mathematical terms. In other words, social arrangements do not present the stability that Cartwright's models and nomological machines seem to require.
An approach of utopias as models is meant to bring together scientific and literary social thought. A realist claim, such that science apprehends some aspects of reality while literature does not, offers too sharp a line between these modes of social reasoning. Nevertheless an appropriate account of social scientific models must offer a way to distinguish between models in scientific investigations and utopias when they are regarded as fictional works.
My suggestion is that this problem is properly addressed by considering the pragmatic contexts of inquiry in which utopias as models of social science and technology appear. In this paper I am going to develop this suggestion by drawing inspiration from the works of John Dewey as well as from some recent theories of inquiry. In this perspective, scientific abstract constructions are to be considered as answers to experienced problematic situations and as projected courses of action to deal with social problems. The difference in regard to utopias as works of art is not in the composition of the abstraction, but in the context of inquiry that they elicit. By focusing on the context of inquiry, this approach dismisses the need for realist claims, in the spirit of Neurath's well-known anti-metaphysical stance.

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EQUILIBRIUM THEORY AND SCIENTIFIC EXPLANATION

Session 19J

Congress section(s): B1, C7

General equilibrium theory and its concordant welfare theorems are often regarded as centerpieces of neoclassical microeconomics. Attempts to prove the theory began in the 19th century by Walras (1874) though it was proven canonically by Arrow and Debreu (1954) in the mid-20th century. The theory asserts that under a set of mathematical assumptions about competitive markets, an economy can achieve a Pareto-optimal state of equilibrium, where there is no excess demand for goods as well as labor, all without the assistance of a central planner or government authority. Such a condition appears to be highly desirable: since there is no excess demand for labor, every agent is satisfactorily employed; moreover, since there is no excess demand for other goods, the general economy does not contain agents who are hungry, malnourished, or otherwise in want. Hence, the mathematical theory has sometimes been invoked in certain laissez-faire policy recommendations where a government should not intervene into markets. The scientific status of general equilibrium theory has been subject to much debate. I consider general equilibrium theory in terms of philosophical theories of scientific explanation and conclude that a new model of explanation would be required, which is called Analogical-Projective Explanation. I argue this makes sense of claims by Sugden (2000) and Gibbard and Varian (1978) that microeconomic theorizing involves making “caricatures” or “credible worlds.” (Cf. Maki (2002) for more examples of fiction in economics.) I make this argument in three stages.
In the first stage, I consider other classical models of scientific explanation given by Hempel and Oppenheim (1948) as candidates for the explanations purported by the theory. I argue that Deductive-Nomological Explanation is not satisfactorily compatible with the sort of explanations purportedly given by equilibrium theory, since it requires that scientific explanations consist in deductions which identify the causes of events using universal natural laws (such as Newton’s Law of Gravitation or Boyle’s Law for Gases). Equilibrium theory does not put forward any causal explanations from universal and necessary laws of nature. Likewise, I argue that the explanations purported by equilibrium theory are not Inductive-Statistical explanations: the claims of the theory are not inductive generalizations from an accumulation of evidence.
In the second stage, I propose a generalization of Hempel’s Inductive-Statistical explanation which I call Analogical-Projective Explanation, in which a feature or condition in a target model is explained by appealing to a feature or condition in a analogical model. Such claims are deductions made in analogical logic based upon a set of projective similarities between the two models. We make many such explanations in science and everyday life: for example, we may make some claims about an architectural structure (i.e. the target model) by investigating a scale model of it (i.e. the analogical model).
In the third stage, I show how we can make use of A-P explanation in microeconomics like the Solow-Swan growth model or Akerlof’s Market for Lemons. However, I also argue that the kind of analogy required for equilibrium theory is highly problematic with respect to analogical inference, since the target (i.e. the real economy) and analogical model (i.e. the mathematical equilibrium economy) cannot be related in projective similarity to each other axiomatically or through a structural homomorphism.

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PROSPECT OF NBICS DEVELOPMENT AND APPLICATION

Session 14G

Congress section(s): C8

The report considers the basic principles of the philosophical approach to NBICS-convergence. Being a method of getting a fundamental knowledge, NBICS-technologies turn into an independent force influencing nature, society and man. One of the basic ideas of nanotechnology concept is an opportunity to consider a man as a constructor of the real world, e.g., by means of constructing human perception due to nanochips, programming the virtual reality in human brain. It might lead to some new forms of consciousness and emergence of the modified objective reality. Developing and introducing nanotechnologies brings up new scientific issues being closely connected with the realization of possible projects such as, for instance, complete description of thinking processes and perception of the reality by human brain, slowdown of aging processes, opportunity of human organism rejuvenation, development of brain/brain or brain/computer interfaces, creation of robots and other devices possessing at least partial individuality, etc. Penetrating technologies into human perception inevitably results in the hybrid reality, which eliminates any borders between man’s virtual personality and his physical embodiment. Space ideas of physical limits of communication and identification also change due to the fact that human presence in the communication medium is cognized as virtual and real simultaneously. It turns out to be an absolutely new phenomenon of human existence having in many ways the constructivism principles in its foundation.
The active role of cognition is the most important aspect of the paradigm analyzed in the report as the methodology of this new type of technologies. Such an opportunity opens unlimited perspectives for individual and collective creative work.
The author examines the dialogue between man and nature by means of the technologies. He demonstrates that they are directed to the decision of scientific issues, mostly having a constructive nature under the influence of virtualization of human consciousness and social relations. The report illustrates on the example of the ‘instrumental rationality’ paradigm that as NBICS-technologies include the Internet, they can’t be used in vacuum. They are interconnected and imply a number of political, economical and social aspects which accompany them. As a result, they’re becoming a characteristic of the public style of thinking. The emphasis is made on socio-cultural prospects of the new kind of technologies and their constructivism nature. Any cognition process turns into a social act as some norms and standards, which are not related to a significant originator, but being recognized by all the society involved in the process, appear among the representatives of different knowledge spheres during the communication process. From the scientific point of view, the consequences of NBICS application are both the unprecedented progress in medicine, molecular biology, genetics, proteomics and the newest achievements in electronics, robotics and software. They will provide a chance to create artificial intelligence, to prolong the life expectancy unprecedentedly, to create new public forms, social and psychical processes. At the same time man doesn’t stop to be rational under the influence of technologies. His cognition process is accompanied by creative and constructive human activity leading to the effects that can reveal themselves, for instance, in the modification of human sensitivity level by means of significant transformation of physical capabilities. In turn, it should lead to nonreversible consequences, because man himself, his body and consciousness turn into an integral part of complex eco-, socio-cultural and socio-technical systems. That’s why the philosophical reflection of ecological, social and cultural results of NBICS-technologies introduction and application is becoming more and more topical. The report concludes that NBICS overcomes all previous technological achievements according to its potential and socio-cultural effects.

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RECEPTION OF ABSOLUTE PROPOSITONS IN THE AVICENNIAN TRADITION: IBN SAHLĀN AL-SĀWĪ ON THE DISCUSSIONS OF THE CONTRADICTION AND CONVERSION OF ABSOLUTE PROPOSITIONS

Session 29K

Congress section(s): A4

There is no doubt that Avicenna (Ibn Sina, d. 1037) is one of the most important logicians of the Middle ages, and one of the most interesting parts of his logic is his theory of absolute propositions, which roughly correspond to Aristotle's categorical sentences. While Avicenna takes a sharp departure from the First Master, Aristotle, in his views on the definition, contradiction, and conversion of his absolutes, some of his disciples and commentators feel need to express their objections to, or reservations about, these views of his. Ibn Sahlān al-Sāwī (d. 1145), one of the mediate disciples of Avicenna, is among those who are not content with his accounts of absolute propositions.
This paper will deal with Sāwī's theory of absolute propositions, and particularly their contradiction, and conversion. It will also discuss his objections, or qualifications, against Avicennian theory, upon which the former builds his own logical ideas. I think, this paper is important because it aims to shed light on Sāwī as a logician, who seems to have been so far overlooked in the literature despite the fact that he is known to have influence over such prominent, and influential figures as Shahāb ad-Dīn al-Suhrawardī (d. 1191), the founder of Ishrāqī philosophy, and the philosopher-theologian, Fakhr al-Dīn al-Rāzī (d. 1209). This paper is based on a treatise by Sāwī, exclusively devoted to the discussion of contradiction of absolute propositions, and to clarifying some of his ideas on the issue put forward in his magnum opus, al-Basāir al-Nasīriyya, fame of which reached so much further than its author's that Sāwī has been often referred as 'the author of al-Basāir' in the philosophical sources of the Islamic world.

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CONSIDERING THE FACTIVITY OF NON-EXPLANATORY UNDERSTANDING

Session 11K

Congress section(s): B1

One of the characteristics of the debate around factivity of understanding is its focus on explanatory sort of understanding. The non-explanatory kind was barely considered. The proposed contribution tries to take some steps in this direction and to suggest this way some possible points of an investigation.
The inquiry will look at the routes of realization of factivity in situations that were marked in the literature to instantiate non-explanatory understanding. Without holding on a specific account the investigation will take as reference suggestions offered by authors such as Lipton, Gijsbers or Kvanvig, though Lipton's view involving explanatory benefits as the bearers or understanding will take a central stage.
The main quest will look at the differences between the issues raised by factivity in explanatory cases and non-explanatory ones. I will look at the variation & specificity of this routes in the different ways of instantiating this sort of understanding. One focus will be on the modality historical arguments and the ones from idealizations raised against supporting the non-factivity claim get contextualized in the non-explanatory cases of understanding. As some of the non-explanatory means do not involve propositional content the factivity issue has to be reassessed. I will therefore reject the pure reductvist view that non-explanatory forms are just preliminary incomplete forms of explanatory understanding i.e. proto-understanding (Khalifa 2017) and so to be considered just under the received view on factivity.
In the last part I will turn to a second point by reference to the previous discussion. The effectiveness condition was advanced by de Regt as an alternative to the veridicality condition. I will support a mixed view which states the need of including reference to both conditions. The cases of non-explanatory understanding, might better illuminate the way the two components are needed in combination. Moreover, in some non-explanatory cases one of the above conditions might take precedence over the other, as for example along the separation of the ones with propositional content (possible explanation, thought experiments) and the other of a non-propositional nature (e.g. manipulations, visualizations).

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DO HEURISTICS EXHAUST THE METHODS OF DISCOVERY?

Session 9K

Congress section(s): B1, B2, C6

Recently, one of us presented a paper on the history of "algorithmic discovery" at an academic conference. As we intend the term, algorithmic discovery is the production of novel and plausible empirical generalizations by means of an effective procedure, a method that is explicitly represented and executed in finitely many steps. In other words, we understand it to be discovery by computable algorithm. An anonymous reviewer for the conference saw things differently, helpfully explaining that "[a]nother, more common name for algorithmic discovery would be heuristics." This comment prompted us to investigate further to see what differences (if any) there are between heuristics and algorithmic discovery.
The aim of this paper is to compare and contrast heuristics with algorithmic discovery and to explore the consequences of these distinctions within their applications in science and other areas. To achieve the first goal the term 'heuristic' is treated as a family resemblance concept. So for a method or rule to be classified as a heuristic it will have to satisfy a sufficient number of the properties involved in the family resemblance. There are ten features that we specify that are involved with being a heuristic. The first of these corresponds to Polya's project developed in How to Solve it and other works. The next five correspond to the heuristic search program in artificial intelligence. The last three pick out more general characterizations of heuristics as methods that lack a guarantee, are rules of thumb, or transform one set of problems into another. We argue that there are methods of algorithmic discovery that have none of the ten features associated with heuristics. Thus, there are methods of algorithmic discovery which are distinct from heuristics.
Once we've established that heuristic methods do not exhaust the methods of algorithmic discovery, we compare heuristic methods with non-heuristic discovery methods in their application. This is achieved by discussing two different areas of application. First, we discuss how heuristic and non-heuristic methods perform in different gaming environments such as checkers, chess, go, and video games. On the side of heuristics Deep Blue and Chinook are discussed. AlphaGo Zero and DQN provide examples of non-heuristic programs. We find that while heuristics perform well in some environments – like chess and checkers – non-heuristic methods perform better in others. And, interestingly, hybrid methods perform well in yet other environments. Secondly, heuristic and non-heuristic methods are compared in their performance in empirical discovery. We discuss how effective each type of method is in discovering chemical structure, finding diagnoses in medicine, learning causal structure, and finding natural kinds. Again, we find that heuristic and non-heuristic methods perform well in different cases. BACON and DENDRAL provide examples of heuristic methods while support vector machines and the PC algorithm provide examples of non-heuristic methods.
We conclude by discussing the sources of the effectiveness of heuristic and non-heuristic methods. Heuristic and non-heuristic methods are discussed in relation to how they are affected by the frame problem and the problem of induction. We argue that the recent explosion of non-heuristic methods is due to how heuristic methods tend to be afflicted by these problems while non-heuristic methods are not.

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EXPLICATING "EXPLICATION" VIA CONCEPTUAL SPACES

Session 17J

Congress section(s): B2, C1

Recent years have witnessed a revival of interest in the method of explication (Carnap, 1950) as a procedure for conceptual change in philosophy and in science (Kuipers, 2007; Justus, 2012), especially in connection with the metaphilosophical nest of positions that goes under the name 'conceptual engineering' (Cappelen, 2018).
In the philosophical literature, there has been a lively debate about the different desiderata that a good explicatum has to satisfy (Shepherd and Justus, 2015; Brun, 2016; Dutilh Novaes and Reck, 2017).
It is still difficult to assess the usefulness of explication as a philosophical method, though. If it is indeed true that many different criteria of adequacy have been proposed and discussed, thereby offering a plethora of recipes for any wannabe explicator, it is nevertheless difficult to judge these proposals due to the vagueness and ambiguity in which they are (almost) always framed.
The main aim of this work is to explicate 'explication', providing a precise bridge-theory into which the explicandum and the explicatum can be represented, thereby allowing an exact framing of the different readings of explication desiderata and therefore a more precise judgment of the adequacy of a given explication. In order to frame my proposal, I am going to rely then on the theory of conceptual spaces (Gärdenfors, 2000; Gärdenfors and Zenker, 2013; Douven et al., 2013).
Specifically, I will show how various readings of explication desiderata (e.g. similarity, fruitfulness, exactness, simplicity, and others) can be precisely framed as geometrical or topological constraints over the conceptual spaces related to the explicandum and the explicatum.
Moreover, with the help of two case studies of successful explications from the history of science (the scientific concept of temperature and the propensity interpretation of fitness), I show how the richness of the geometrical representation of concepts in conceptual spaces theory allows us to achieve fine-grained readings of explication desiderata. For instance, I will show how we can read similarity as a quasi-isometry, exactness as a measure of boundary regions, fruitfulness as convexity, and many others.
I will also argue that these tools allow us to overcome some alleged limitations (Brun, 2016; Reck, 2012) of explication as a procedure of conceptual engineering such as the so-called "paradox of adequate formalization" (Dutilh Novaes and Reck, 2017).
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RECIPROCAL GROUP OUGHTS

Session 11B

Congress section(s): A2

In [2], Horty shows that the framework of STIT logic can be used to reason about what agents and groups ought to do in a multi-agent setting. To decide what groups ought to do he relies on a utility function that assigns a unique value to each possible outcome of their group actions. He then makes use of a dominance relation to define the optimal choices of a group. When generalizing the utilitarian models of Horty to cases where each agent has his own utility function, Horty's approach requires each group to have a utility function as well.
There are several ways to do this. In [4], each group is assigned an independent utility function. This has the disadvantage that there is no connection between the preferences of a group and its members. Another option is to define the utility of a given outcome for a group of agents as the mean of the utilities of that outcome for the group's members, as is done in [3]. However, this requires that utilities of individual agents be comparable. A third option is pursued in [5], where Turrini proposes to generalize Horty's notion of dominance such that an action of a group X dominates another action X' just in case, relative to the utility function of each group member, X dominates X'. The optimal actions of a group can then be defined using this modified dominance relation. This approach, however, leads to undesirable outcomes in certain types of strategic interaction (e.g. a prisoner's dilemma).
Here, we present a new approach towards evaluating group actions in STIT logic by taking considerations of reciprocity into account. By reciprocity we mean that agents can help each other reach their desired outcomes through choosing actions that are in each other's interest. We draw upon the work of Grossi and Turrini [1] to identify certain group actions as having different types of reciprocal properties. For example, a group action can be such that, for each agent a in the group, there is some other agent a' such that the action of a' is optimal given the utility function of a. We compare these properties and show that by first selecting a certain type of reciprocal action and only then applying dominance reasoning we are left with group actions that have a number of desirable properties. Next, we show in which types of situations agents can expect to benefit by doing their part in these reciprocal group actions.
We then define what groups ought to do in terms of the optimal reciprocal group actions. We call the resulting deontic claims reciprocal oughts, in contradistinction to the utilitarian oughts of [2] and strategic oughts of [3]. We end by comparing each of these deontic operators using some examples of strategic interaction.
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INCONSISTENCY AND BELIEF REVISION IN CASES OF APPROXIMATIVE REDUCTION AND IDEALIZATION

Session 24D

Congress section(s): B1

[This is a contribution to the symposium "From contradiction to defectiveness to pluralism"]
We must face with the fact that in science, as well as in ordinary life (think of certain forms of self-deception, if there really exists such a phenomenon), there are or there seem to be inconsistencies, inconsistent beliefs or inconsistent commitments. The reasons for this situation can be multiple. We can find internal inconsistencies within a theory, although these inconsistencies may be just temporal and may disappear with time as far as we are able to dissolve them in the face of new information or as a consequence of a decision. In fact, as Peter Vickers and other authors have pointed out, whether a theory contains inconsistencies or not may well depend on the way we construct or reconstruct the theory. We can find inconsistencies between a theory and the body of observational data, a very usual phenomenon that leads to the problem of how to react to the existence of anomalies. And finally, there may be inconsistencies that leap out when we try to relate a theory that is partially rejected, though is considered to be approximately correct at least in some of its parts, to a theory that is thought to be a more successful successor. The Kepler-Newton case, the Newton-Einstein relation, or the relation between classical and quantum equations, are good examples of this. It is the several ways of treating these different kinds of inconsistencies from a formal point of view, and specifically the latter ones, what I am interested in in the present contribution. It must not be that scientists, in these cases, in fact have inconsistent beliefs. It suffices that they have inconsistent commitments or assumptions, that for different reasons, they want nevertheless to maintain. In this contribution, I want to consider several proposals of dealing with these inconsistencies. In particular, I will consider AGM belief revision theory, as it has been used by Hans Rott and other authors as an adequate formal tool for cases of so-called approximative reduction containing idealization (for example, Rott applies AGM to the Kepler-Newton case). I will compare this version of belief revision theory, where the logical consequence relation is classical, with the paraconsistent belief revision theory proposed by Graham Priest. I will also compare these tools with others that make use of different alternative model-theoretic approaches. The consequences of these comparisons when applied to the analysis of historical scientific cases will provide us with interesting conclusions about the relation between consistency, rationality, and scientific progress, as well as about the different formal approaches to the reconstruction of scientific theories and the relations between them.

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MODELING IN NEUROSCIENCE: CAN COMPLETE AND ACCURATE UNDERSTANDING OF NERVE IMPULSE PROPAGATION BE ACHIEVED?

Session 28G

Congress section(s): C3

Explanatory understanding of phenomena in neuroscience is typically achieved via the construction of models. A prime example is the celebrated Hodgkin-Huxley model (HH-model) of the action potential (Hodgkin and Huxley 1952, Craver 2007). The HH-model is central to the electricity-centered conception of the nerve impulse that dominates contemporary neuroscience. In recent years, however, the HH-model has been challenged because it is unable to account for non-electrical aspects of the nerve impulse, some of which have been known for decades. Consequently, alternative theories and models of nerve impulse propagation have appeared upon the scene, using a thermodynamic or mechanical framework instead of an electrical one. One of these models is the Heimburg-Jackson model (HJ-model) (Heimburg and Jackson 2005), according to which the nerve impulse is an electromechanical density pulse in the neural membrane. Its proponents assume that this model is potentially able to replace the HH-model.
Alternatively, one might think that these models of nerve impulse propagation should not be regarded as rivals but may be integrated in a general unifying model. An example of such a proposal is the model of Engelbrecht et al. (2018), which has been developed to unify all relevant manifestations of the nerve impulse and their interactions. The attempt of Engelbrecht et al. aligns with the widespread neuroscientific conviction that the ultimate goal of neuroscience is to develop models that represent neuroscientific phenomena accurately and completely. In this paper, however, we argue that the Engelbrecht model does not provide an accurate and complete representation of the nerve impulse. One reason for this conclusion is that the HH-model and the HJ-model, which the Engelbrecht model attempts to integrate, contain inconsistent assumptions.
We submit that the above-sketched approaches to modeling nerve impulse propagation are motivated by a misguided assumption, namely that accurate and complete representation is a unique, objective criterion for evaluating neuroscientific models. By contrast, we propose, in line with Giere (2004), to take into account the purpose for which a model is used when evaluating the value of models; models are tools that represent the nerve impulse accurately and completely enough to achieve a specified goal. Considering models as tools for specific purposes, and acknowledging that different purposes often require incompatible assumptions, suggests that it will be impossible to develop a consistent general unifying model of nerve impulse propagation (cf. Hochstein 2016, Craver and Kaplan 2018). Instead of aiming at explaining such a complex phenomenon in a single model, neuroscientists would do better to employ a 'mosaic' (cf. Craver 2007) framework of models. From this collection of models the explanation of nerve impulse propagation can be inferred based on the piecemeal and sometimes contradictory representation of it in distinct models.
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THE EPISTEMIC BASING RELATION IN MATHEMATICS

Session 30C

Congress section(s): C1

In my talk, I will discuss different ways in which mathematicians base a mathematical belief on a proof and highlight some conditions on the basing relation in mathematics. The (proper) basing relation is a relation between a reason – in this case a proof – and a doxastically justified belief. I will argue that in mathematics if a subject bases a belief on a proof then she recognizes the proof as a good reason for that belief. Ceasing to recognize the argument as a proof (that is, as a sound argument), she would often be disposed to weaken her confidence in the belief or even to abandon it. Moreover, the basing relation for theorems in mathematical practice (as opposed to other domains) is put in place by a conscious rational activity: grasping how a proof supports a claim. This constraint will lead me to explore in the case of mathematics Leite's (2004) general proposal of how justification is tied to the practice of justifying.
As has been pointed out (see for example Azzouni (2013)), there are different ways of grasping how a proof supports its conclusion and therefore the basing relation can assume different forms. It is possible to identify at least two broad types of grasping, leading to different types of basing: 1) a local, step-by-step grasping and 2) a holistic grasping. These are not mutually exclusive, and often basing in practice will be a combination of the two. In some cases, the strength of informal proofs lies in providing us with a holistic grasping, whereas formal proofs often underwrite the possibility of checking the validity of all the inferential steps, since these are generally decomposed into basic steps and thus allow us to gain a local grasp of how the conclusion is supported. At one end of the spectrum, there is a formal derivation of a complex result in an

interpreted formal system: we can grasp how the derivation supports its conclusion, but at the same time fail to be aware of the over-all structure of the argument. At the other end, we gain a holistic grasp of how an informal proof supports its conclusion just by appreciating its large-scale structure, without going into all the details, which we accept on testimony or authority.

We often grasp how a proof supports its conclusion through a perceptible instantiation of one of its presentations. I will argue that in certain cases a proof presentation (or even a proof) facilitates a type of grasping, while making the other type of grasping difficult. For example, a purely formal proof will tend to be presented in ways which support only the local grasping. This in turn implies that basing the belief in the theorem on the proof involves such grasping.

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RISK FACTORS, EXPLANATION AND SCIENTIFIC UNDERSTANDING

Session 7I

Congress section(s): C4

The notion risk factor is omnipresent in contemporary medical research, medical practice (e.g. prevention campaigns) and layman understanding of health and disease. This is a recent phenomenon, in the sense that it started in the 1950s. During the second half of the 20th century and the first decade of the 21st century, talk in terms of risk and risk factors has become ever more pervasive. Nevertheless, the work of medical scientists and sociologists of medicine shows that there is no consensus about how the term is best used. In general, four different meanings of the notion "risk factor" can be discerned in the literature:

- Risk factor0 = any factor associated with the development of a given disease.
- Risk factor1 = risk factor0 considered to be a cause of the disease.
- Risk factor2 = risk factor0 of which it is not known whether it is a cause of the disease or not.
- Risk factor3 = risk factor0 thought not to be a cause of the disease.

Distinguishing these meanings is important because the value of risk factor knowledge may differ depending on the kind of risk factor involved. In my talk I will use this fourfold distinction as a basis for my analysis of whether and how risk factors can explain a disease and whether and how they provide scientific understanding. Given that causal factors are generally taken to have explanatory power, it seems uncontroversial to claim that type 1 risk factors explain and thereby provide scientific understanding. The interesting question is whether and how this extends to type 2 and 3 risk factors. Do they explain? If so, in what sense? And what do they explain? Additionally, do they provide scientific understanding (with or without explanation)? And again: if so, in what sense?

As a starting point of my analysis, I will take the possibility that non-causal risk factors somehow explain seriously. However, this implies that I also need to develop an account of what makes non-causal risk factors explanatory powerful, without being causal. I will try to do that by shifting my attention away from (causally) explaining the onset of a disease to explaining differences in chances. Understanding why person a has a higher chance of getting breast cancer than person b may require that we have knowledge about probabilistic dependency relations in the world, without these relations being causal. I will explore whether taking this route helps us further in getting a grasp on whether and how non-causal risk factors can explain and/or provide scientific understanding.

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VARIETIES OF CONCEPTUAL CHANGE: THE EVOLUTION OF COLOR CONCEPTS

Session 20C

Congress section(s): C5

In recent years, several philosophers have explored the prospect of conceptual engineering, i.e., the deliberate modification of our concepts for specific cognitive, scientific, societal, or ethical purposes. The project is not uncontroversial and various objections can be raised. First, it may be argued that concepts cannot be changed, either because the very concept CONCEPT is defective, or because concepts are immutable. Second, it can be argued that conceptual engineering requires evaluative standards, which may not exist. Third, it can be argued that "engineering" of concepts is not possible, because intentional planning and controlled implementation of the changes are not feasible.

I will argue that the first and second objection are less compelling, but that the third objection does impose restrictions on the possibility of conceptual engineering. I will argue that continuous but slow conceptual change is ubiquitous, that conceptual change often results in an optimization of our conceptual structure, that conceptual change often results from intentional changes, but that in very few cases there can be genuine planning and controlled implementation. Conceptual change can best be viewed as an evolutionary process, in which there can be random change (drift), unplanned optimization (adaption), and intentionally designed but uncontrolled amelioration (reform) and intentionally designed fully controlled optimization (engineering). This picture of conceptual change will reduce the scope of conceptual engineering considerably; conceptual engineering appears as a limit case of conceptual change in which there is optimization based on deliberate intentions and under full control.

I will focus on color concepts to illustrate the notions of drift, adaption, reform, and engineering. I will describe the evolution in time of color concepts and highlight the varying degrees of amelioration, intentional planning, and control. Subsequently I analyze the evolution of color concepts from the perspective of Cappelen's Austerity Framework for conceptual engineering. I conclude with a brief discussion of the relevance of the color case for theorizing about the change of concepts in general and for the prospects of conceptual engineering.

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COMMENT ON "POPPER AND THE QUANTUM CONTROVERSY"

Session 11C

Congress section(s): B6

In this comment, I will discuss in detail the genesis of Popper's EPR-like experiment, which is at the centre of Prof. Freire's paper. I will show that Popper devised his experiment already in 1980, namely two years before the publication in his

renown “Quantum Theory and the Schisms in Physics” (1982). Moreover, I will focus on the early resonance that such a Gedankenexperiment had in the revived debate on quantum foundations.
At the same time, I will describe how Popper’s role in the community of physicists concerned with foundations of quantum physics evolved over time. In fact, when he came back to problems of quantum mechanics in the 1950s, Popper strengthened his acquaintances with some illustrious physicists with philosophical interests (the likes of D. Bohm, H. Bondi, W. Yourgrau), but was not engaged in the quantum debate within the community of physicists (he did not publish in physics journals or participate in specialised physics conferences). From mid-1960s, however, with the publication of the quite influential essay “Quantum Mechanics without the Observer” (1967), Popper’s ideas on quantum physics garnered interest and recognition among physicists. At that time, Popper systematised his critique of the Copenhagen interpretation of quantum mechanics, proposing an alternative interpretation based on the concept of ontologically real probabilities (propensities) that met the favour of several distinguished physicists (among them, D. Bohm, B. van der Waerden and L. de Broglie). This endeavour led Popper to enter a long-lasting debate within the community of physicists.

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CHALLENGES OF NEW TECHNOLOGIES: THE CASE OF DIGITAL VIGILANTISM

Session 5E

Congress section(s): C7, C8

This paper is part of the bigger project where different philosophical and sociological approaches are evaluated and merged in order to understand and to show how new technologies could change political life. It is very important that to work in the one discipline is not enough to try to understand the new phenomena. The main aim of this paper is, by using the case of Digital Vigilantism, to analyse what kind of change the technologies bring in and how understanding of the self and self-act is changing during the process when more and more new technologies are integrated in people’s everyday life. The swarm is a metaphor, which Zygmunt Bauman uses to show how understanding of communities is changing in liquid modernity. The swarms are based on untied, uncontrolled, short-term relationships between consumers/users to achieve some goals (Bauman, 2007). Swarms could be very massive in numbers and have a lot of power for a very short period. One of the examples could be Digital Vigilantism, which is an act of punishing certain citizens (they are believed to deserve being punished) by other Internet users. One of the type of Digital Vigilantism is putting someone’s personal information on display for everybody to spread shaming acts. Sometimes this kind of act gains enough power to change political agenda. It is important to see, as states Daniel Trottier, technological tools makes visibility (profiles, social media and so on) as a weapon to control the internet (Trottier, 2017). Moreover, the moral norms, which are used to act as vigilantes, are very simplified, puritanical, and straitlaced. There are no room for understanding that human acts could be complex and not straightforward. In addition, the DV case lets us rethink the problem of responsibility, o as Denis Thompson (1980), the problem of many hands, formulated it. The new technologies makes this problem much more difficult and problematic, in swarms we do not share responsibility, if something goes wrong, everybody leaves the swarm. The moral issues of responsibility is unnecessary burden, which should be minimized, so that is why the morality is changed by the ethics/rules. The third problem is that people are interested in some actions for a very short period, the speed is enormous, but the real political act/change requires an active and stable effort. The main intrigue is whether the political act itself will change influenced by the swarm effect? “Twitter diplomacy” is becoming a new norm, but whether it will works to sustain stable life or the permanent chaos is the new foundation for the new political order?

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METAPHYSICAL ISSUES IN MODERN PHILOSOPHY OF TIME: V.I. VERNADSKY’S IDEA OF “CAUSE” OF TIME (“SOURCE” OF TIME)

Session 28K

Congress section(s): B4

In the studies on the nature of time, its forms and instances, it is common to distinguish between scientific (instrumental) and philosophical (metaphysical) approaches. The former concerns mathematical and scientific knowledge of how to measure and use time, whereas the latter concerns philosophical reflexion upon what time is and what its nature might be. Aristotle considered that these approaches are not opposite, but mutually complementary. In the 20th century, a synthesis of these approaches was undertaken by V.I. Vernadsky (1863-1945). Although Vernadsky is commonly associated with the concept of biological time as an instance of time, his major contribution is more profound. Since his early works, he poses the metaphysical question of the nature of time and its origin.
We can identify the following theses in his works [Denisova 2018]:
1. A complex object is a dynamic system, characterized by processes occurring in time that has its own internal, autonomous time. The specific features of the internal time of the system are determined by these processes, generated by them and do not exist outside of them.
2. Since a living object does not only exist (is present, existent), but also lives (is born, grows and perishes), it generates time by its whole existence.
3. Consequently, the source and cause of time is both an individual living organism and the biosphere as a whole.
4. Since inert matter is not capable of generating a living substance, the biosphere has no beginning in time; it is eternal, like Cosmos.
5. If the biosphere, as source and cause of time, is eternal, then the question of the “beginning of time” is meaningless and incorrect.
6. Then the only correct question is that of the cause of existence of time and the possibility of absolute time as universal point of reference of all time instances. Biological, but not astronomical time is such a kind of universal time, because all life processes (growth, becoming, aging of organism, reproduction, succession of generations) have a stable duration. Its absoluteness and universality consists not in the fact that it concerns directly any material object and is associated with any motion, but in its suitability for measuring any processes, since it always advances at the same tempo, regardless of external conditions and follows objective laws.
The intuitive idea of unity and integrity of the world, pervading Vernadsky’s scientific and philosophical work, was thus consolidated by his scientific theory of biosphere that produces time [Vernadsky 1988].

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THE SEMANTIC ACCOUNT OF SLURS, APPROPRIATION, AND METALINGUISTIC NEGOTIATIONS

Session 20C

Congress section(s): B1

What do slurs mean? The semantic account has it that slurs express complex, negative properties such as ‘ought to be discriminated because of having negative properties, all because of being [neutral counterpart]’. In this paper I discuss whether the semantic account can explain the phenomenon of appropriation of slurs, and I argue that the best strategy for the semantic account is to appeal to a change of literal meaning. In my view, we could appeal to the phenomenon of meta-

linguistic negotiation, in order to make sense of conversations where the two parties employ the same slur with different meanings.

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FUZZY LOGIC AND QUASI-LEGALITY

Session 9L

Congress section(s): A2

Given a set of norms N within a legal system L it is normally said that N is a sharp set. This, of course, has a significant consequence: It means that for all normative situation it is possible to decide whether such situation can be interpreted in terms of N or not. According to this point of view legal systems are complete; this means that there is a clear distinction between lawlessness and what is complete legal.
Nevertheless, the reality is quite different: Norms are sometimes vague or ambiguous. Of course, vagueness and ambiguity are different semantic phenomena, the last is perhaps less complex than the first. If an expression is ambiguous, it is enough to specify the sense in which the expression is being used and the problem is sorted out, but in the case of vague expressions the solution is not so easy. In such cases: how do judges make decisions? Not exclusively based on the content of the norms but maybe appealing to other rational resources such as analogy or ponderation.
Law is vague in at least two senses. In the first sense, it means that legal terms are vague or fuzzy. Think for example in criminal responsibility. Criminal law distinguishes between perpetrator of the crime and the accomplice and distinguishes between primary and secondary accomplice. So, criminal responsibility could be understood as a matter of degree: the perpetrator and the (primary or secondary) accomplice are responsible in different degrees and, by this reason, they receive different penalties.
The second sense is more interesting. In this sense we are not talking about terms but about the norm as a fuzzy concept. It means that there are either/or norms, that is: norms that are clearly part of the legal system but there are also norms that are not part of the legal system (v.g., moral rules or principles, customary rules). It means, then, that legality is not a precise concept but a fuzzy one.
This is what Oren Perez has called quasi-legality. What we will do is to analyze from the fuzzy logic point of view the relevance of quasi-legality for contemporary legal theory and the its consequences for legal positivism.

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BEYOND BELIEF: LOGIC IN MULTIPLE ATTITUDES (JOINT WORK WITH A. STARAS AND R. SUGDEN)

Session 3E

Congress section(s): B2

Logical models of the mind focus on our beliefs, and how we reasons in beliefs. But we also have desires, intentions, preferences, and other attitudes, and arguably we reason in them, particularly when making decisions. Taking a step towards logic in multiple attitudes, we generalize three classic logical desiderata on beliefs - consistency, completeness, and implication-closedness - towards multiple attitudes.
Our three “logical” desiderata on our attitudes - hence on our psychology - stand in interesting contrast with standard “rationality requirements” on attitudes, such as the requirement of having transitive preferences, non-contradictory beliefs, non-acratic intentions, intentions consistent with preferences, and so on. In a theorem, we show a systematic connection between our logical desiderata and rationality requirements: each logical desideratum on attitudes (i.e., consistency, completeness, or implication-closednes) is equivalent to the satisfaction of a certain class of rationality requirements. Loosely speaking, this result connects

logic with rational choice theory. This has important implications for whether reasoning in multiple attitudes can help us become consistent, complete, or implication-closed in our attitudes.

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TINKERING WITH NOMENCLATURE. TEXTUAL ENGINEERING, CO-AUTHORSHIP, AND COLLABORATIVE PUBLISHING IN EIGHTEENTH-CENTURY BOTANY

Session 4A

Congress section(s): B1

This paper explores how the messiness of eighteenth-century botanical practice that resulted from a constant lack of information generated a culture of collaborative publishing. Given the amount of information required for an accurate plant description let alone a taxonomic attribution, eighteenth-century botanists and their readers were fully aware of the preliminary nature of their publications. They openly acknowledged the necessity of updating them in the near future, and developed collaborative strategies for doing so efficiently. One of these was to make new material available to the botanical community as quickly as possible in a first printed version, while leaving the process of completing and correcting it to be undertaken by stages at a later date. Authors updated their own writings in cycles of iterative publishing, most famously Carl Linnaeus, but this could also be done by others – in the context of this paper by the consecutive editors of the unpublished papers of the German botanist Paul Hermann (1646-1695) who became his co-authors in the process.
Hermann had spent several years in Ceylon as a medical officer of the Dutch V.O.C. before he returned to the Netherlands in 1680 with an abundant collection of plants and notes. When he died almost all of this material, eagerly awaited by the botanical community, was still unpublished. As the information economy of botany, by then a discipline aiming for the global registration and classification of plants, tried to prevent the loss of precious data, two botanists – William Sherard (1650-1728) and Johannes Burman (1706-1779) - consecutively took on the task of ordering, updating, and publishing Hermann’s manuscripts. The main goal of these cycles of iterative publishing was, on the one hand, to add relevant plants and, on the other, to identify, augment, and correct synonyms – different names that various authors had given to the same plant over time. As synonyms often could not be identified unambiguously, they had to be adjusted repeatedly, and additional synonyms, which would, in turn, require revision in the course of time, had to be inserted. The process of posthumously publishing botanical manuscripts provides insights into the successive cycles of accumulating and re-organizing information that had to be gone through. As a result, synonyms were networked names that were co-authored by the botanical community. Co-authorship and a culture of collaborative publishing compensated for the messiness of botanical practice.

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A META-LOGICAL FRAMEWORK FOR PHILOSOPHY OF SCIENCE

Session 16

Congress section(s): B2

In the meta-theoretic study of science we can observe today a tendency towards logical abstraction based on the use of abstract model theory [1], where logical abstraction is understood as independence from any specific logical system.
David Pearce’s idea of an abstract semantic system in 1985 [2] was characterised by this tendency, and so was the idea of translation between semantic systems, which is directly linked to reduction between theories [3]. A further step towards logical abstraction was the categorical approach to scientific theories suggested by Halvorson and Tsementzis [4]. Following the same direction we argue for the use of institution theory - a categorical variant of abstract model theory developed in computer science [5] - as a logico-mathematical modeling tool in formal philosophy of science. Institutions offer the highest

level of abstraction currently available: a powerful meta-theory formalising a logical system relative to a whole category of signatures, or vocabularies, while subscribing to an abstract Tarskian understanding of truth (truth is invariant under change of notation). In this way it achieves maximum language-independence. A theory is always defined over some institution in this setting, and we also define the category of all theories over any institution I. Appropriate functors allow for the translation of a theory over I to a corresponding theory over J. Thus we get maximum logic-independence, while the theory remains at all times yoked to some particular logic and vocabulary.

To clarify our point we present an institutional approach to the resurgent debate between supporters of the syntactic and the semantic view of scientific theory structure, which currently focuses on theoretical equivalence. If the two views are formalized using institutions, it can be proven that the syntactic and the (liberal) semantic categories of theories are equivalent [6][7]. This formal proof supports the philosophical claim that the liberal semantic view of theories is no real alternative to the syntactic view; a claim which is commonly made - or assumed to be true. But it can also - as a meta-logical equivalence - support another view, namely that there is no real tension between the two approaches, provided there is an indispensable semantic component in the syntactic account.

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IN DEFENCE OF BRANCH COUNTING IN EVERETTIAN QUANTUM MECHANICS

Session 31K

Congress section(s): C2

The main challenge for the Everett interpretation of quantum mechanics (EQM) is the 'probability problem': If every possible outcome is actualized in some branch, how can EQM make sense of the probability of a single outcome as given by the Born rule, which, after all, makes for the empirical adequacy of the original theory?

Advocates of EQM have sought to make conceptual room for epistemic probabilities. There are two prominent approaches: the decision-theoretic approach (Deutsch (1999), Greaves (2004), Wallace (2012)) and the self-location uncertainty approach (Vaidman (1998; 2011), Sebens/Carroll (2016)). In the decision-theoretic approach, one conceives of rational agents facing hypothetical bets pertaining to the outcomes of quantum measurements. Whilst some such agent knows that all the possible outcomes are instantiated, she may nonetheless have betting preferences from which her credences can be operationalized. In the self-location approach one conceives of a rational agent who has undergone branching but is yet to see the outcome of the quantum measurement and is therefore uncertain about which branch she is in, and probabilities here are her credences about her self-location.

Both approaches aim to show that a rational agent is required to set her credences as per the Born rule. In the first, the result is variously proved from a set of decision-theoretic axioms. In the second, Sebens/Carrol prove the result from a single principle, their "Epistemic Separability Principle" (ESP).

Prima facie, the right way to set one's credences is by "Branch Counting" (BC): the credence a rational agent ought to have in a particular quantum measurement outcome is equal to the ratio of the number of branches in which that (kind of) outcome is actualized to the total number of branches. After all, each branch is equally real.

BC is demonstrably at odds with the Born rule and thus advocates of EQM have sought to argue against BC in various ways. The aim of this paper is to show that these arguments are not persuasive, and that, therefore, the probability problem in EQM has not been solved, neither in the decision- theoretic approach nor the self-location uncertainty approach. Specifically, I consider three different ways in which BC has been attacked: 1) that BC is not rational because an agent using it can be Dutch-booked; 2) that BC is not rational because there is no such thing as the number of branches in EQM; and 3) that BC is not rational because it conflicts with a more fundamental principle of rationality, namely ESP.

Apropos 1: Wallace (2012) argues that BC is irrational because an agent accepting it can be subjected to a diachronic Dutch book. However, I show that in the Everettian multiverse some such diachronic Dutch book cannot even be constructed. That it seems to be constructible owes to certain underspecifications of the when bets are placed and paid-out. Once these are fully specified, there is no Dutch book.

Apropos 2: Wallace (2003, 2007, 2012) has argued that BC is irrational because there is no such thing as the number of branches. Following Dizadji-Bahmani (2015), a distinction is drawn between two possible claims: that the number of branches is indeterminate (a metaphysical claim) and that the number of branches is indeterminable (an epistemological claim). It is argued that the former claim is not defensible and that the second claim would only show that BC is irrational given a further strong assumption which is not justifiable. Here it is shown that this analysis extends to the Sebens/Carrol (2016) approach.

Apropos 3: The Sebens/Carrol (2016) self-location uncertainty approach turns on ESP, which, in short, requires that the "credence one should assign to being any one of several observers having identical experiences is independent of the state of the environment." Adopting this principle, they argue that BC is (in some cases) irrational. This is shown by their thought experiment 'Once-Or-Twice', wherein two agents face a bifurcation at time t2, and then a trifurcation at time t3, of their initial branch. BC is inconsistent with ESP in this case, and they advocate adopting the latter. I argue contra this that A) BC is a far more intuitive principle than ESP in the given context and that B) whilst tracing out the environment - a crucial move in the Sebens/Carrol argument - leaves the mathematical representation of Alice's state from t2 to t3 unchanged, there is an important physical difference between these two states and to which Alice is in principle privy, and that this warrants changing her credences in violation of the Born rule.

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MORAL ACHIEVEMENT OF A SCIENTIST

Session 29B

Congress section(s): B1, B5

Weber's sociology, particularly its fact/value dichotomy and its concept of instrumental rationality, has been criticised by authors such as A. MacIntyre or M. Horkheimer for its narrow understanding of rationality, which is reduced to the choice of the most efficient means for an end. The validity of this end cannot, in turn, be established in a rational way.

If the choice of values and ultimate ends is the result of an irrational and subjective decision, then ethics, as a discipline that presupposes the possibility of a rational study of values, concerning right and wrong, should be impossible. In particular, the ethics of science, as a discipline, which presupposes the importance of moral evaluations in scientific activity, should also be impossible. Weber's claim that science should only deal with facts and abstain from value judgments would seem then to support nihilist and emotivist understanding of his works.

In my presentation, arguments challenging this interpretation are proposed. Weber's speech «Science as a Vocation» suggests the idea of «moral achievement» of a scientist. A good teacher's primary task is to teach students to recognise facts, even if those facts are inconvenient to their political position. Success in this task is qualified by Weber as an «moral achievement». My thesis is that the ability to recognise inconvenient facts makes it possible to reconcile Weber's concept of scientist with the ethics of science. The scientist position is similar to the political position in the way that both of them require certain presuppositions which depend on «ultimate attitude toward life».

For the scientific position, these presuppositions include the significance of the results of scientific researches. If the recognition of inconvenient facts implies the sensitivity of one's position to such facts (i.e. that one's position can be modified by these recognised facts), then there is a connection between facts and values. If the recognition of inconvenient facts does not imply that, then it is not clear what the difference is between recognising those facts and neglecting them.

Inconvenient facts for scientists are those facts which challenge their presuppositions, such as the importance of the results of their investigations. For example, it is likely to be an inconvenient fact for a scientist that the results of their research can be used in weapons development. Although recognition of this fact does not imply that the presupposition of the importance of advances in a particular area in nuclear physics should be abolished, it does imply, however, that, when this fact is recognised, this presupposition should clearly be reevaluated with the regard to this fact. It should be at least possible that this position will be changed as the result of recognition of those facts. Weber suggests that clarification of the ultimate meaning of one's actions is the task of philosophical disciplines. This is precisely what ethics of science is concerned with. However, although this connection between values and facts centred on the recognition of inconvenient facts to one's position can provide the basis for an ethics of science (and thereby prove wrong nihilist and emotivist understandings of Weber's thought), such an ethics would ultimately be limited. It would be impossible, for example, to justify the necessity of subjecting scientific methods and practices (such as experiments on animals) to moral evaluation.

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DISCOVERING UNFAITHFUL CAUSAL STRUCTURES FROM OBSERVATIONS AND INTERVENTIONS

Session 6I

Congress section(s): B1, B2

The framework of causal Bayesian networks or directed acyclic graph (DAG) (Spirtes et al. 2000, Pearl 2000) provides a powerful tool for scientists to infer causal structures from observational data. In this framework, several assumptions are required to bridge the gap between joint probability distributions and unknown causal structures. One assumption, which is called "the Faithfulness condition", says that if two variables are probabilistically independent, there should be no causal link between them. This assumption has brought about a few discussions in philosophy of science (Zhang 2008, Steel 2008, Andersen 2013, Zhang & Spirtes 2016, etc).

The main issue with Faithfulness is that it might be violated, and moreover, causal structures violating Faithfulness are notoriously difficult to discover. In this paper, I try to deal with this problem in the real-world context in which experimental interventions and additional assumptions are considered (since learning unfaithful causal structures from purely observational data is so hard). Specifically, I deal with two problems in my paper:

(1) To what extent we can identify unfaithful causal structures from observational data: Although Zhang (2008) has proved that undetectable violations of Faithfulness only happen locally to two kinds of triangles, it is difficult to identify these unfaithful triangles in a large causal structure. I show that we can significantly reduce the difficulty of identifying unfaithful causal structures if we add an additional assumption, which claims that one kind of unfaithful triangles is rare in the real world; in contrast, the other kind of unfaithful triangles (that are more likely to be found in the real world due to their selective advantages) will always appear as V-structures (also called "colliders") in the inferred DAGs.

(2) How many interventions we need to fully identify an unfaithful causal structure: The primary difficulty with experimental design in the presence of unfaithfulness is that we do not know which variables to intervene on. Since we do not know where those (undetectable) violations are located, scientists cannot use experiments to efficiently catch these violations out (Andersen 2013). But I point out it is actually practically efficient to search for violations of faithfulness using experiments, because we are able to restrict the potential occurrence of unfaithfulness only to a relatively small number of V-structures in the draft DAG. This way, need a few experiments are needed to check and revise these V-structures and discover the true causal DAG.

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HOW SHOULD WE TREAT HUMAN ENHANCEMENT TECHNOLOGY: ACCEPTANCE OR REJECTION?

Session 8I

Congress section(s): C4, C5, C7, C8

In recent years, the idea of human enhancement exits no longer just in science fictions and fictional films, but is actually supported by the related technology. In the philosophical research about the human enhancement technology, the related views can be summarized into Conservatism, Radicalism and Eclecticism. Fundamentally speaking, the reason for the disagreement among these three different viewpoints is that they hold different positions regarding to our own existence as "human being" and the issue of personal identity. Can an enhanced person be called a "human "? Can I" still be the original "I" after enhancement? In consideration of such questions, different views will inevitably lead to different answers on the basis of their own positions. Based on a detailed analysis of existing viewpoints about these kinds of technology, combining with the narrative theory and the embodiment theory, this paper attempts to elucidate why the personal identity is crucial for human beings, how the human enhancement technology will have a negative impact on the personal identity and further endanger the intrinsic nature of human. Therefore, as many scholars suggested, in front of the development of human enhancement technology, we should hold a prudential and critical attitude.

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THE MONOGENESIS CONTROVERSY: A HISTORICAL AND PHILOSOPHICAL INVESTIGATION

Session 31I

Congress section(s): B6, C3, C4

I give a general overview of the "unity of species" and "unity of origin" of human species controversy, in the 20th century known as discussions of mono and polygenesis, from Samuel Stanhope Smith's "Essay on the Causes of Variety of Complexion and Figure in the Human Species" (1810) to geneticist Francisco Ayala's biological "disproof" of "monogenesis," or the idea that human beings are one species with a common origin, and the biblical Eve of Genesis (c. 1995). It covers much ground, much of it having to do with describing one of the key biological and racial theories of the 19th century. This was "polygenesis" or the argument that human beings were distinct "racial types" and evolved in distinct locations, in the writings of Charles Darwin and in more detail, Paul Broca, Louis Agassiz, Josiah Nott and the "American school" in the United States. Arguments for polygenesis among biologists, sociologists and anthropologists were present through the "evolutionary synthesis." However, there is no real discussion of genetics until the last part of the paper, as I consider most of the distortions to be in scholars discussions of 19th century accounts of "unity" or "plurality" of human origins and species.
This will then be a historical account of the species concept as it pertains to humanity and a philosophical paper in that it attempts (particularly in its conclusion) to address why arguments surrounding the unity and plurality of species as well as the unity and plurality of origin have persisted, and the impact of differing kinds of evidence (genetic and theological) on theory change and conceptual discussions during the 19th and 20th centuries.

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MACHINE LEARNING, THEORY CHOICE, AND NON-EPISTEMIC VALUES

Session 13F

Congress section(s): B2, B5, C1, C6, C8

I argue that non-epistemic values are essential to theory choice, using a theorem from machine learning theory called the No Free Lunch theorem (NFL).
Much of the current discussion about the influence of non-epistemic values on empirical reasoning is concerned with illustrating how it happens in practice. Often, the examples used to illustrate the claims are drawn from politically loaded or practical areas of science, such as social science, biology, and environmental studies. This leaves advocates of the claim that non-epistemic values are essential to assessments of hypotheses vulnerable to two objections. First, if non-epistemic factors happen to influence science only in specific cases, perhaps this only shows that scientists are sometimes imperfect; it doesn't seem to show that non-epistemic values are essential to science itself. Second, if the specific cases involve sciences with obvious practical or political implications such as social science or environmental studies, then one might object that non-epistemic values are only significant in practical or politically loaded areas and are irrelevant in more theoretical areas.
To the extent that machine learning is an attempt to formalize inductive reasoning, results from machine learning are general. They apply to all areas of science, and, beyond that, to all areas of inductive reasoning. The NFL is an impossibility theorem that applies to all learning algorithms. I argue that it supports the view that all principled ways to conduct theory choice involve non-epistemic values. If my argument holds, then it helps to defend the view that non-epistemic values are essential to inductive reasoning from the objections mentioned in the previous paragraph. That is, my argument is meant to show that the influence of non-epistemic values on assessment of hypotheses is: (a) not (solely) due to psychological inclinations of human reasoners; and (b) not special to practical or politically loaded areas of research, but rather is a general and essential characteristic for all empirical disciplines and all areas of inductive reasoning.
In broad strokes, my argument is as follow. I understand epistemic virtues to be theoretical characteristics that are valued because they promote epistemic goals (for this reason, the epistemic virtues are sometimes just called "epistemic values"). For example, if simpler theories are more likely to satisfy our epistemic goals, then simplicity is epistemically valuable and is an epistemic virtue. I focus on one aspect of evaluation of hypotheses – accuracy, and I interpret accuracy as average expected error. I argue that NFL shows that all hypotheses have the same average expected error if we are unwilling to make choices based on non-epistemic values. Therefore, if our epistemic goal is promoting accuracy in this sense, there are no epistemic virtues. Epistemic virtues promote our epistemic goals, but if we are not willing to make non-epistemic choices all hypotheses are equally accurate. In other words, no theoretical characteristic is such that hypotheses which have it satisfy our epistemic goal better. Therefore, any ranking of hypotheses will depend on non-epistemic virtues.

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SCIENTIFIC FREEDOM AND SCIENTIFIC RESPONSIBILITY

Session 21

Congress section(s): B5

Recent statements of the responsibilities of scientists (e.g. the AAAS 2017 statement) have strengthened the responsibilities of scientists towards the societies in which they pursue their research. Scientists are now expected to do more than treat their experimental subjects ethically and communicate their results. They are also expected to benefit humanity. In a shift from the predominant views of the past 70 years, this responsibility is now tied to the freedom to pursue scientific research, rather than opposed to such freedom. The first half of this talk will describe this change and its drivers. The second half will address the fact that research institutions have not caught up with this theoretical understanding in practice. As exemplified by oversight of dual-use research, the responsibility to not cause foreseeable harm is not fully embraced by scientists and is not well supported by institutional oversight. Yet this is weaker than the responsibility to provide benefit. I will argue that scientists do in fact have a pervasive responsibility to provide benefit (and avoid foreseeable harm), but that this responsibility for individual scientists is different from the responsibility for the scientific community as a whole, and

that minimally acceptable practices are also different from ideals. These differences in the nature of responsibility have important implications for science policy.

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OPTIMIZING GROUP LEARNING OF PROBABILISTIC TRUTHS

Session 6D

Congress section(s): B2

There are distinctively social aspects to learning. Not only do we learn from others -- as when we learn from our parents or our teachers -- we also often can acquire new knowledge only if we collaborate with others, for instance, as members of the same laboratory or research group.
Once the communal aspects of knowledge acquisition are recognized, it becomes natural to ask how we might best organize a group of agents bent on a joint research goal, supposing we want to optimize the group's performance. This is a broad question, which could concern topics ranging from after-work socializing to decisions about how to allocate grant money or other resources within a group. The present paper, instead, focuses on some of the most fundamental epistemic mechanisms that groups might decide to put in place, or might be encouraged to adopt, for how their members ought to act and especially to interact. In particular, it focuses on mechanisms for how group members should respond to, or "update on", new evidence concerning probabilistic hypotheses while also being informed about the changing belief states of others in the group, who they know to acquire evidence pertaining to a common source.
The notion of optimizing group performance will also be understood in a strictly epistemic sense, that is, as relating to the question of how to get at the truth of whatever issue the group is working on. While truth is generally regarded as the overarching goal of science, it is equally acknowledged that science serves many practical purposes, too. For that reason, we are often forced to make a speed-accuracy trade-off: we do want to get at the truth, but we also want to get there fast, which may require that we settle on quickly becoming highly confident in the truth (even if we cannot completely rule out alternative hypotheses) or quickly getting close to the truth.
Thus, the question to be investigated is how members of a research group should update on the receipt of new evidence in a social setting, where they also have access to relevant beliefs of (some or all of) their colleagues, supposing the group wants to strike the best balance between speed (getting at the truth fast) and accuracy (minimizing error rates). The main methodological tool to be used is that of computational agent-based modelling. Specifically, we build on the well-known Hegselmann-Krause (HK) model for studying opinion dynamics in groups of interacting agents focused on a common research question, where in our case thhis question involves probabilistic truths.

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NEW VERSIONS OF THE MATHEMATICAL EXPLANATION OF THE CICADA CASE - AD HOC IMPROVEMENTS WITH UNCERTAIN OUTCOMES OR THE WAY TO A FULL EXPLANATION?

Session 17J

Congress section(s): C1

Platonism in mathematics is not often supported by explicit means. The so-called Enhanced Indispensability Argument is in that sense an exception. It is given in the form of a kind of modal syllogism, in which the second premise is an assertion of the indispensable explanatory role of mathematical objects in science. One example illustrating this claim is the "Cicada case". Taking into account philosophical literature, this example of mathematical explanation in science has been used, already, in Baker(2005). It seems that the disadvantages in this explanation have been an incentive for the emergence of new versions of the mathematical explanation of this scientific phenomenon. We will analyze suggestions for improving the explanation that are given in Baker(2016) and Dieveney(2018). Both versions of the explanation are an attempt to eliminate the shortcomings of previous versions in an improved mathematical context (new mathematical claims), using the results of modern biological research. We want to show that, however, at the same time, both new versions leave some drawbacks of the original version unresolved by opening additional space for the new controversial questions about the reliability of the explanation.
Although the new version of Baker's explanation (Baker 2016) alleviates the blade of criticism that goes to the original explanation, it fails to respond to a significant remark - the explanation relies on an unproved empirical assumption of the existence of a predator with periodic life cycles. In addition, there are some other shortcomings in the new explanation. Namely, it uses a new unproved empirical hypothesis (that the ecologically viable range for periodical cicadas is between 12- and 18-years) that was later only made more probable by the "4n + 1" hypothesis (Baker(2017, pp.782-787)). Also, certain technical details on the basis of which the central mathematical statement, used in the explanation, is derived remain unexplained. For example, the lower bound of one of the intervals referred to in Lemma 2 (Baker(2016), p.338), which is the formal basis of optimization described in the paper, is the fixed number, while all other bounds are variables. In addition, no argument has been given in favour of why the lower limit of this interval, intended for the approximation of the lifetime of the predator, would not be regarded as a variable. We shall show that in such a case (if all bounds were seen as variables), a heuristic consideration that is an intuitive introduction to the mentioned lemma would not be correct.
Improving the explanation given in Dieveney(2018) is an attempt to keep the power of the mathematical explanation from controversial empirical hypotheses. However, we are not sure if the author succeeded in this way. Namely, Dieveney has presented two relatively independent variants of explanatory improvements that are based on the same theory of number theory. In the first variant, an explanation given in Baker(2016) was improved using a weaker empirical requirement regarding viable cicada periods, but the author did not reject the unproven assumption about periodical cycles of predators. In another variant, he tried to use the observations from Koenig and Liebhold(2013) to link the primeness of life cycle periods of cicada with non-periodical avian-predator life cycles. We shall point out two things. Firstly, the mentioned biologists do not indicate the possibility of such a connection. They do not see anything relevant in the fact that the length of the cicada life is expressed by a prime. Secondly, it is true that avian-predators in the research of these biologists do not appear in periodic cycles, but their most massive appearance occurs in periodic cycles, expressed mainly by primes. On this basis, it can be said that Dieveney did not fully succeed in confirming the thesis, he had set, about the influence of non-periodical predators on the cicada life.
Using the above-mentioned but also some other arguments, we shall show that the two analyzed texts (Baker(2016) and Dieveney(2018)) increased more than they reduced the number of questions regarding mathematical explanation in the Cicada case.
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HISTORY AND PHILOSOPHY OF SCIENCE AFTER THE PRACTICE-TURN: FROM INHERENT TENSION TO LOCAL INTEGRATION

Session 10A

Congress section(s): B1, B6

Over the past several decades, a pernicious myth has taken hold. This is the myth that history and philosophy of science are intrinsically opposed to one another: as if the two fields have timeless essences that tug against each other in equally timeless

tension. The myth has a certain fascination for purists on both sides, and even speaks an important truth about the current configuration of disciplinary standards. But it is a myth nonetheless, and one that stands in the way of a more productive analysis of the value of history of science for philosophy of science.

My goal in this essay is to explode this myth, and in so doing to indicate a more fruitful way of analyzing the value of history of science for philosophy of science. Specifically, I wish to ask: what roles do historical sources and information play in the practice of philosophers of science? The purpose of framing the question in this way is to direct attention away from issues of global, disciplinary integration, and towards issues of local, problem- and method-based integration. This is where the action is, practically speaking—"in the trenches" where real philosophical research is done. To preview my conclusion, historical sources and information play a variety of roles in contemporary philosophical practice, each of them methodologically legitimate and philosophically well-motivated. In particular, I will show that different methodological approaches in philosophy of science (and specifically, in practice-based philosophy of science) use historical information in different ways, guided by different local ends. History of science matters to philosophy of science—but the mode of this mattering is plural, and so is the needed integration.

My argument is set out in three broad sections. In the first, I explore in greater detail the "myth of inherent tension": the notion that history of science and philosophy of science are intrinsically opposed to one another. The crux of this myth is the supposed opposition between philosophy's normative and universalist orientation and history's uncompromising particularism. Because these perspectives are set at cross-purposes, no reconciliation between the two disciplines is possible, or even desirable. I show that this argument is based on a faulty assumption, as well as a descriptively inadequate conception of philosophical practice. Replacing this with a more ecumenical conception is crucial to gaining purchase on the focal question: what is the value of history of science for philosophy of science? But the reigning conception is deeply ingrained, and for that reason difficult to see around. It is the task of Section 3 to say why this is so, and ultimately to point the way towards a more up-to-date conception of philosophical practice. Finally, in Section 4, I take a run at the focal question, first, by articulating three methodological approaches in practice-based philosophy of science, and second, by showing how each approach engages history of science in light of its philosophical goals. The paper concludes with a brief discussion of philosophical normativity, and how conceptualizing philosophy as a practice alleviates some lingering concerns.

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NEW THOUGHTS ON COMPOSITIONALITY. CONTRASTIVE APPROACHES TO MEANING: FINE'S SEMANTIC RELATIONISM VS. TARSKI-STYLE SEMANTICS

Session 24M

Congress section(s): A2

The paper is an assessment of compositionality from the vantage point of Kit Fine's semantic relationist approach to meaning. This relationist view is deepening our conception about how the meanings of propositions depend not only on the semantic features and roles of each separate meaningful unit in a complex but also on the relations that those units hold to each other. The telling feature of the formal apparatus of this Finean relationist syntax and semantics, viz. the coordination scheme, has some unexpected consequences that will emerge against the background of an analogy with the counterpart theoretic semantics for modal languages.

The semantic-relationist program defends 'referentialism' in philosophy of language; Fine holds that semantic relations that have to be added to the assigned intrinsic values in our semantic theory, especially the relation which he calls 'coordination', can do much of the work of the (Fregean) senses. A relationist referentialism has certain important explanatory virtues which it shares with the Fregean position, but the former is better off ontologically than the latter, since it is not committed to the existence of senses.

The paper is examining some philosophical presupposition of semantic relationism and discusses in a critical manner why and how the other semantic systems (notably Tarski's semantics) got wrong the specification of the truth conditions and of the meaning conditions for the sentences of the languages.

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COMPUTATIONAL RELIABILISM: BUILDING TRUST IN MEDICAL SIMULATIONS

Session 9D

Congress section(s): C8

Several philosophical issues in connection with computer simulations rely on the assumption that their results are trustworthy. Examples of these include the debate on the experimental role of computer simulations [Parker, 2009], the nature of computer data [Barberousse and Vorm, 2013], and the explanatory power of computer simulations [Durán, 2017]. The question that emerges in this context is, therefore, which methods could be implemented to increase the reliability and trust of computer simulations.

We claim that trust is warranted because computer simulations are deemed reliable processes, that is, there are methods exogenous to the computer simulation itself that confer trust to the results of the simulation. We call this computational reliabilism as a way to show its reliance on, but still independence from process reliabilism. Concretely, while process reliabilism is externalist concerning justifications, we argue that this kind of radical externalism is not possible for computer simulations. Instead, a subdued form of externalism is necessary which allows for at least one instance of the J→JJ principle. That is one can trust the results of a possibly epistemically opaque simulation if one has a method at hand which ensures its reliability. We discuss four such methods establishing reliability namely, verification and validation, robustness analysis for computer simulations, a history of (un)successful implementations, and the role of expert knowledge in simulations [Durán, Formanek 2018]. We conclude by arguing that the general skeptical challenge concerning the universal reliability of such methods is theoretically unsolvable but poses no threat to practicing science with computer simulations.

We illustrate these findings with examples in simulations in medicine. Of particular interest will be to analyze the reliability of two simulations of bone breakage [Keaveny et al. 1994]. The importance of these simulations is that there are at least two different types, namely, of a computerized image of a real hipbone, and of a computerized 3-D grid mathematical model. For both cases, the method implemented by the computer simulations is, in principle, to be trusted more than an empirical experiment. However, researchers need to find ways to grant that trust in the results. We submit that computational reliabilism advances in the right direction, that is, offering grounds for the reliability of computer simulation and trustworthiness of their results.

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IDEALIZATIONS AND THE DECOMPOSABILITY OF MODELS IN SCIENCE

Session 12G

Congress section(s): B1, C2, C3

Idealizations are a central part of many scientific models. Even if a model represents its target system in an accurate way, the model will not replicate the whole target system but will only represent relevant features and ignore features that are not significant in a specific explanatory context. Conversely, in some cases not all features of a model will have a representative function. One common strategy to account for these forms of idealizations is to argue that idealizations can have a positive function if they do not distort the difference-makers of the target system. (Strevens 2009, 2017)
This view about the role of idealized models has recently been challenged by Collin Rice. (Rice 2017, 2018) He claims that the strategy to account for idealizations in terms of a division between representative parts of a model and the parts which can be ignored fails for several reasons. According to him idealizations are essential for the mathematical framework in which models can be constructed. This idealized mathematical framework is, in turn, a necessary precondition to create and understand the model and undermines our ability to divide distorted from representative features of a model. His second reason for doubting the adequacy of the strategy to divide between relevant and not relevant model parts is the fact that many models distort difference-making features of the target system. Alternatively, he suggests a position he calls the holistic distortion view of idealized models. This position includes the commitment that highly idealized models allow the scientist to discover counterfactual dependencies without representing the entities, processes or difference-makers of their target systems. In my presentation I am going to argue against this position and claim that to explain and to show causal and non-causal counterfactual dependence relations with the help of a model is only possible if the model accurately represents the difference-makers within a target system. I will do this by explicating the notion of a mathematical framework of a model which Rice's argument heavily depends upon and reevaluate some of his examples of idealized models used in scientific practice like the Hardy–Weinberg equilibrium model in biology and the use of the thermodynamic limit in physics.

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HYPERINTENSIONS AS ABSTRACT PROCEDURES

Session 27J

Congress section(s): A3

Hyperintensions are defined in my background theory Transparent Intensional Logic (TIL) as abstract procedures that are encoded by natural-language terms. For rigorous definition of a hyperintensional context it is crucial to distinguish two basic modes in which a given procedure can occur, namely displayed and executed. When a procedure C occurs displayed, then C itself figures as an object on which other procedures operate; while if C occurs executed, then the product of C figures as an object to operate on. Procedures are structured wholes consisting of unambiguously determined parts, which are those sub-procedures that occur in execution mode. They are not mere set-theoretic aggregates of their parts, because constituents of a molecular procedure interact in the process of producing an object. Furthermore, this part-whole relation is a partial order. On the other hand, the mereology of abstract procedures is non-classical, because the principles of extensionality and idempotence do not hold.

A hyperintensional context is the context of a displayed procedure, and it is easy to block various invalid inferences, because different procedures can produce one and the same function-in-extension, be it mathematical mapping or PWS-intension. But blocking invalid inferences in hyperintensional contexts is just the starting point. There is the other side of the coin, which is the positive topic of which inferences should be validated and how these valid inferences should be proved. The problem is this. A displayed procedure is a closed object that is not amenable to logical operations. To solve this technical difficulty, we have developed a substitution method that makes it possible to operate on displayed procedures within and inside a hyperintensional context. Having defined the substitution method we are in a position to specify beta-conversion by 'value'. I am going to prove that unlike beta-conversion by name the conversion by value is validly applicable so that the redex and contractum are logically equivalent. However, though TIL is a typed lambda-calculus, the Church-Rosser theorem is not valid for beta-reduction by name, which is so due to hyperintensional contexts. Hence, I am going to specify a fragment of TIL for which the theorem is valid. On the other hand, the Church-Rosser theorem is valid in TIL for beta-reduction by value. Yet, in this case the problem is just postponed to the evaluation phase and concerns the choice of a proper evaluation strategy. To this end we have implemented the algorithm of context recognition that makes it possible to evaluate beta-conversion by value in a proper way so that the Church-Rosser T heorem is valid in all kinds of a context. There are still other open issues concerning the metatheory of TIL; one of them is the problem of completeness. Being a hyperintensional lambda-calculus based on the ramified theory of types, TIL is an incomplete system in Gödel's sense, of course. Yet, I am going to specify a fragment of TIL that is complete in a Henkin-like way.

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EPISTEMIC ASPECTS OF REVERSE MATHEMATICS

Session 30C

Congress section(s): C1

Reverse mathematics is a research programme in mathematical logic that determines the axioms that are necessary, as opposed to merely sufficient, to prove a given mathematical theorem. It does so by formalising the theorem in question in the language of second order arithmetic, and then proving first that the theorem follows from the axioms of a particular subsystem of second order arithmetic, and then "reversing" the implication by proving that the theorem implies the axioms of the subsystem (over a weak base theory corresponding to computable mathematics). The standard view of reverse mathematics holds that a "reversal" from a theorem to an axiom system shows that the set existence principle formalised by that axiom is necessary to prove the theorem in question. Most of the hundreds of ordinary mathematical theorems studied to date have been found to be equivalent to just five main systems, known as the "Big Five". The five systems have all been linked to philosophically-motivated foundational programmes such as finitism and predicativism.
This view of reverse mathematics leads to an understanding of the import of reverse mathematical results in ontological terms, showing what mathematical ontology (in terms of definable sets of natural numbers) must be accepted by anyone who accepts the truth of a particular mathematical theorem, or who wants to recover that theorem in their foundational framework. This is most easily seen in connection with "definabilist" foundations such as predicativism, which hold that only those sets of natural numbers exist which are definable "from below", without reference to the totality of sets of which they are a member.
In this talk, we will argue that this view neglects important epistemic aspects of reverse mathematics. In particular, we will argue for two theses. First, the connections between subsystems of second order arithmetic and foundational programmes is also, if not primarily, motivated by epistemic concerns regarding what sets can be shown to exist by methods of proof that are sanctioned by a certain foundational approach. In this context, reverse mathematics can help to determine whether commitment to a certain foundation warrants the acceptance of a given mathematical result. Second, reversals to specific subsystems of second order arithmetic track the conditions under which theorems involving existential quantification can be proved only in a "merely existential" way, or can provide criteria of identity for the object whose existence is being proved in the theorem in question. These epistemic aspects of reverse mathematics are closely related to computability-theoretic and model-theoretic properties of the Big Five systems.

We will conclude by arguing that in virtue of these features, the reverse mathematical study of ordinary mathematical theorems can advance our understanding of mathematical practice by making explicit patterns of reasoning that play key roles in proofs (compactness style arguments, reliance on transfinite induction, etc.) and highlighting the different virtues embodied by different proofs of the same result (such as epistemic economy, the ability to provide identity criteria for the object proved to exist, etc.).

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PROPORTIONAL CAUSES AND SPECIFIC EFFECTS

Session 4B

Congress section(s): B1

Standard methods of causal discovery take as input a statistical data set of measurements of well-defined causal variables. The goal is then to determine the causal relations among these variables. But how are these causal variables identified or constructed in the first place? In “Causal Inference of Ambiguous Manipulations “, Spirtes and Scheines (2004) show how mis-specifications of the causal variables can lead to incorrect conclusions about the causal relations. These results suggest that there is a privileged level of description for a causal relation, or at least, that not all levels of description are correct. It is tempting to conclude that the correct level of causal description is then at the finest possible level of description. But apart from the challenge that extant accounts of causality do not fit well with our finest level of description of physical processes, such a conclusion would also imply that the discussion of causation at the macro-level is at best elliptic. In this presentation we propose an account of a “correct” causal description that retains a meaningful notion of causality at the macro-level, that avoids the types of mis-specifications illustrated by Spirtes and Scheines, and that (in general) still permits a choice of the level of description. In this regard it offers a middle route between realist accounts of causation that maintain that there is an objective fact to the matter of what is “doing the causal work”, and pragmatic accounts of causation that reject the notion of some privileged level of causal description. As the title suggests, the proposal is closely related to notions of the “proportionality” of causes in the literature on the metaphysics of causation, and considerations of the “specificity” of variables as used in information theory.

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SEEING AND DOING, OR, WHY WE SHOULD ALL BE ONLY HALF-BAYESIAN

Session 29M

Congress section(s): A2, B1, B2, B4, C1, C9

In his 2001 paper, “Bayesianism and causality, or, why I am only a half-Bayesian” Judea Pearl claims that the language of probability is “not suitable for the task” of representing scientific knowledge. In doing so, Pearl not only implicitly eulogizes probabilistic accounts of causation (e.g. Reichenbach 1956, Suppes 1970) but also motivates his own account of the do-calculus. If Pearl is correct, then philosophers and practitioners of science who care about causation face a choice: supplement your formal language or abandon any formal representation of causation. The flourishing literature on causal modeling and Pearl’s do-calculus (see Hitchcock 2018 for an overview) is a testament to the promise of the former. However, one question remains unanswered: exactly what is missing from the language of probability that renders it unsuitable for capturing causal relationships?
In this paper we argue that the answer to this question is the language of probability is fundamentally propositional but causal structure is fundamentally non-propositional. Following Cartwright (1979) we argue that the role causal structure plays in effective action suggests that the formal language for causation is not descriptive, but prescriptive. In support of this position we develop a non-propositional imperative logic which takes the axioms of probability theory as a fragment and supplements them with an imperative logic operator. We introduce the operator and consider its metalogical properties. Finally, we compare our approach to Pearl’s do-calculus.

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SCIENTIFIC METAPHYSICS AND THE MANIFEST IMAGE

Session 17G

Congress section(s): B4, B6, C2

One of the most serious problems confronting the project of scientific metaphysics is the fact that some of our best scientific theories are compatible with several fundamentally different ways the world could be, and that there are no agreed-upon principles for identifying one of them as the actual way the world is. A striking instance of this is the disagreement between what is known as the “primitive ontology” approach and the “wave function realist” approach to quantum mechanics. Each of the sides can adduce plausible reasons for preferring its own approach, such that the debate seems to have reached a stalemate. I propose a way forward in this kind of debate by first making a small historical point, which I will then show to have significant systematic implications.
The debate on quantum ontology is sometimes framed with reference to Wilfrid Sellars’s distinction between the manifest and the scientific image: While primitive ontologists argue that their approach (by virtue of postulating matter in physical space) brings the scientific image closer to the manifest image and thereby facilitates the reconciliation of the two, wave function realists insist that we can very well achieve this reconciliation without the postulates of primitive ontology. My historical point is that the notion of the manifest image in play here is very different from the one employed by Sellars. The former amounts to little more than the naïve physics of macroscopic bodies, whereas Sellars’s manifest image is a sophisticated and comprehensive view of man-in-the-world. Taking Sellars’s notion seriously implies that reconciling the manifest and the scientific image involves more than just fitting together the common sense conception of physical objects with that of theoretical physics (as the quarrel between primitive ontologists and wave function realists would have it). In particular, it requires reflection on human intentions and interests, including those operative in constructing scientific or metaphysical theories.
To some extent, this point has already been appreciated in the recent debate on scientific metaphysics, when it was recognized that metaphysical positions should not be understood as mere doctrines but as stances, which, apart from beliefs, include commitments, values and so on. However, this recognition has not yet led to much progress with respect to the kind of stalemate described above. Indeed, Anjan Chakravartty (“Scientific Ontology”, OUP 2017) has argued that such stalemates are inevitable, because there is no stance-neutral criterion on which a non-question-begging case for the superiority of one stance over another could be based (as long as both of them are internally coherent). This is correct if one discusses (as Chakravartty does) the very general level of “the empiricist stance” versus “the metaphysical stance”. By contrast, if stances are understood as competing frameworks for the reconciliation of the scientific and the manifest image in the full (Sellarsian) sense, they will no longer fulfil the (stance-neutral) criterion of internal coherence to an equal degree. This opens a new way to evaluate metaphysical positions and therefore has the potential to transform the debate on scientific metaphysics.

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ARE POINTS (NECESSARILY) UNEXTENDED?

Session 25J

Congress section(s): C1

Ever since Euclid defined a point as “that which has no part” it has been widely assumed that points are necessarily unextended. It has also been assumed that, analytically speaking, this is equivalent to saying that points or, more properly speaking, degenerate segments—i.e. segments containing a single point—have length zero. In our talk we will challenge these assumptions. We will argue that neither degenerate segments having null lengths nor points satisfying the axioms of Euclidean geometry implies that points lack extension. To make our case, we will provide models of ordinary Euclidean geometry where the points are extended despite the fact that the corresponding degenerate segments have null lengths, as is required by the geometric axioms. The first model will be used to illustrate the fact that points can be quite large—indeed, as large as all of Newtonian space—and the other models will be used to draw attention to other philosophically pregnant

mathematical facts that have heretofore been little appreciated, including some regarding the representation of physical space.

Among the mathematico-philosophical conclusions that will ensue from the above talk are the following three.

(i) Whereas the notions of length, area and volume measure were introduced to quantify our pre-analytic notions of 1-dimensional, 2-dimensional and 3-dimensional spatial extension, the relation between the standard geometrical notions and the pre-analytic, metageometric/metaphysical notions are not quite what has been assumed. Indeed, what our models illustrate is that, it is merely the infinitesimalness of degenerate segments relative to their non-degenerate counterparts, rather than the absence of extension of points, that is implied both by the axioms of geometry and these segments null lengths.

(ii) As (i) suggests, the real number zero functions quite differently as a cardinal number than as a measure number in the system of reals. So, for example, whereas a set containing zero members has no member at all, an event having probability zero may very well transpire, and perhaps more surprisingly still, a segment having length zero may contain a point encompassing all of Newtonian space.

(iii) Physicists and philosophers alike need to be more cautious in the claims they often make about how empirical data determines the geometrical structure of space.

Although the mathematico-philosophical import of our paper can be fleshed out using any of the standard formulations of Elementary Euclidean Geometry, for our purposes, it will be especially convenient to employ Tarski's system P [1], in which only points are treated as individuals and the only predicates employed in the axioms are a three-place predicate B (where "Bxyz " is read y lies between x and z, the case when y coincides with x or z not being excluded) and a four-place predicate D (where "Dxyzu" is read x is as distant from y as z is from u).

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THE SEMANTIC FOUNDATIONS OF PHILOSOPHICAL ANALYSIS

Session 12M

Congress section(s): A2

The subject of this paper is a targeted reading of sentences of the form 'To be F is to be G,' which philosophers often use to express analyses, and which have occupied a central role in the discipline since its inception. Examples that naturally lend themselves to this reading include:

1. To be morally right is to maximize utility.
2. To be human is to be a rational animal.
3. To be water is to be the chemical compound H2O.
4. To be even is to be a natural number divisible by two without remainder.
5. To be a béchamel is to be a roux with milk

Sentences of this form have been employed since antiquity (as witnessed by 2). Throughout the ensuing history, proposed instances have been advanced and rejected for multitudinous reasons. On one understanding, this investigation thus has a long and rich history— perhaps as long and rich as any in philosophy. Nevertheless, explicit discussion of these sentences in their full generality is relatively recent. Recent advances in hyperintensional logic provide the necessary resources to analyze these sentences perspicuously—to provide an analysis of analysis.

A bit loosely, I claim that these sentences are true just in case that which makes it the case that something is F also makes it the case that it is G and vice versa. There is a great deal to say about what I mean by 'makes it the case that.' In some ways, this paper can be read as an explication of that phrase. Rather than understanding it modally (along the lines of 'To be F is to be G' is true just in case the fact that something is F necessitates that it is G and vice versa), I employ truth-maker semantics: an approach that identifies the meanings of sentences with the finely-grained states of the world exactly responsible for their truth-values.

This paper is structured as follows. I articulate the targeted reading of "To be F is to be G" I address, before discussing developments in truth-maker semantics. I then provide the details of my account and demonstrate that it has the logical and modal features that it ought to. It is transitive, reflexive and symmetric, and has the resources to distinguish between the meanings of predicates with necessarily identical extensions (sentences of the form 'To be F is to be both F and G or not G' are typically false); further, if a sentence of the form 'To be F is to be G' is true then it is necessarily true, and necessary that all and only Fs are Gs. I integrate this account with the λ-calculus—the predominant method of formalizing logically complex predicates—and argue that analysis is preserved through β-conversion. I provide two methods for expanding this account to address analyses employing proper names, and conclude by defining an irreflexive and asymmetric notion of analysis in terms of the reflexive and symmetric notion.

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THE INFLUENCE OF THE LATE SCHOOL OF ALEXANDRIA ON THE ORIGIN AND DEVELOPMENT OF LOGIC IN THE MUSLIM WORLD

Session 14M

Congress section(s): A2, A4

In order to promote the discussion surrounding the origins and background of Arabic Logic, we have to explore the Greek Logical traditions and the Logical introductions to Aristotle's works compiled at the end of the Roman Empire. The study demonstrates that the view of Logic adopted by many Greek thinkers and transmitted through translations of the commentaries on Aristotle into Arabic had a great impact on the genesis of Islamic theology and philosophy. A number of late philosophers are explored with a view to demonstrate this point.

By about 900, almost all of Aristotle's logical works had been translated into Arabic, and was subject to intensive study. We have texts from that time, which come in particular from the Baghdad school of Philosophy. The school's most famous logician was Al-Farabi (d. 950) who wrote a number of introductory treatises on Logic as well as commentaries on the books of the Organon.

The research aims at studying the influence of the late Alexandrian School of philosophy in the 6th century AD, in the appearance and development of Greek Logic in the Muslim World. In addition, the adaptation of its methodologies by the Islamic thinkers, and its impact on Muslim philosophical thought. The late Alexandrian school has been underestimated by many scholars who regard its production at the end of the Classical Age as mere interpretations of previous writings; delimiting its achievement to the preservation of ancient Logical and philosophical heritage. The research reviews the leading figures of the Alexandrian School and its production of Logical commentaries. It also traces the transmission of its heritage to the Islamic World through direct translations from Greek into Syriac first and then into Arabic. It also highlights the impact of the Alexandrian commentaries on Muslim recognition of Plato and Aristotle as well as its Logical teaching methodology starting with the study of Aristotle's Categories as an introduction to understand Aristotle's philosophy.

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FEASIBLE REDUCIBILITY AND INTERPRETABILITY OF TRUTH THEORIES

Session 27I

Congress section(s): A1

Let P[B] be an axiomatic theory of truth over a base theory B, e.g., P can be some flavor of typed or untyped truth theory, and B can be PA (Peano arithmetic) or ZF (Zermelo-Fraenkel theory of sets). P[B] is said to be *feasibly reducible* to B if there is a polynomial time computable function f such that: if p is a P[B]-proof of a sentence b formulated in the language of B, then f(p) is B-proof of b. This notion is known to be stronger than the conservativity of P[B] over B for finitely axiomatizable P[B]. A closely related notion is that of *feasible interpretability* of P[B] in B, which corresponds to the existence of a polynomial time deterministic Turing machine that verifies (via an appropriate translation) that P[B] is interpretable in B.
In this talk I will overview recent joint work with Mateusz Łełyk and Bartosz Wcisło on feasible reducibility of P[PA] to PA, and feasible interpretability of P[PA] in PA, for three canonical truth theories: P = CT^- (Compositional truth without extra induction), FS^- (Friedman-Sheard truth without extra induction), and KF^- (Kripke-Feferman truth without extra induction).
Our results show that not only the “logical distance”, but also the “computational distance” between PA and the aforementioned canonical truth theories is as short as one could hope for, thereby demonstrating that, despite their considerable expressive power, these truth theories turn out to be rather light additions to PA.

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GENERALIZED INTERPRETABILITY AND CONCEPTUAL REDUCTION OF THEORIES

Session 26L

Congress section(s): A1, B2, B4, C1

As a general notion in the reduction of formal theories, we investigate d-dimensional relative interpretations. In particular, we ask for a necessary condition under which these interpretations can be called “conceptual reductions”. Throughout the talk, we establish a condition for subdomains of f-definable models of models of interpreting theories. Due to this condition and a theorem of Swierczkowksi(1990), it can be shown that there is no d-dimensional relative interpretation of d-dimensional (d > 1) euclidian geometry into RCF, which satisfies the property of being a conceptual reduction in our sense. A similar result can be obtained with Pillay(1988) for interpretations of ACF0 into RCF.
At first, we give a characterization of d-dimensional relative interpretability in terms of models, which extends results of Montague(1965) and Hájek(1966). This characterization tells us, that for a d-dimensional relative translation f from L[S] to L[T], if f is a d-dimensional relative interpretation of S in T, then the f-definable models of T-models are all S-models. But even more important, if these f-definable T-models form a subset of Mod(S), then f is a d-dimensional relative interpretation of S in T. From here it seems natural to ask which interpretations f we can single out, if we require its f-definable T-models to form a particular subset of Mod(S). Especially, it will be interesting to think of conditions to restrict the class of interpretations to those interpretations, which could be called “conceptual reductions”.
Already by definition, an f-definable T-model A can share objects of its domain with the domain of a T-model B only if T allows B to include d-tuples of elements of A, which is not given in general. But for the question of conceptual change, this seems to be an important feature, since by some predominant semantic intuitions, the meaning of concepts will be determined (in one way or another) by denotations. With this in mind, we suggest a condition for the domain of f-definable models which enables us in a model A to talk about the same objects as in the model from which A was defined.
We are aware that this condition seems to be unusual and deserves more justification, since in first-order model theory we are used to talking about models of theories solely as models in a certain isomorphism-class for well-known reasons. Certainly, for any f-definable model A of B with a different domain than B we can always build an isomorphic copy i(A) of A having a subdomain of B. Nevertheless, we will see that our situation here is slightly different, since we are not considering models of first-order theories in general, but f-definable models of models of first-order theories. This changes the situation to that extend, that - as we prove - an isomorphic copy i(A) of A might not be itself an f-definable model of B and even further; might not be an f-definable model of B in Mod (T) for any f that is an interpretation in T.
Finally, we discuss the consequences of our condition for the famous reduction of euclidian geometry to RCF and related results. We argue, that our formal conclusions are supported by intuitions relevant in mathematical practice.
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EXPECTED UTILITY, INDUCTIVE RISK, AND THE CONSEQUENCES OF P-HACKING

Session 12J

Congress section(s): B1, C4, C7

P-hacking is the manipulation of research methods and data to acquire statistically significant results. It includes the direct manipulation of data and/or opportunistic analytic tactics. Direct manipulation involves experimental strategies such as dropping participants whose responses to drugs would weaken associations; redefining trial parameters to strengthen associations; or selectively reporting on experimental results to obtain strong correlations. Opportunistic analytic tactics include performing multiple analyses on a set of data or performing multiple subgroup analyses and combining results until statistical significance is achieved.
P-hacking is typically held to be epistemically questionable, and thus practically harmful. This view, which I refer to as the prevalent position, typically stresses that since p-hacking increases false-positive report rates, its regular practice, particularly in psychology and medicine, could lead to policies and recommendations based on false findings. My first goal in this paper is to formulate the prevalent position using expected utility theory. I express a hypothetical case of p-hacking in medical research as a decision problem, and appeal to existing philosophical work on false-positive report rates as well as general intuitions regarding the value of true-positive results versus false-positive ones, to illustrate the precise conditions under which p-hacking is considered practically harmful. In doing so, I show that the prevalent position is plausible if and only if (a) p-hacking increases the chance that an acquired positive result is false and (b) a true-positive result is more practically valuable than a false-positive one.
In contrast to the prevalent position, some claim that experimental methods which constitute p-hacking do play a legitimate role in medical research methodology. For example, analytic methods which amount to p-hacking are a staple of exploratory research and have sometimes led to important scientific discoveries in standard hypothesis testing. My second aim is to bring the prevalent position into question. I argue that although it is usually the case that refraining from p-hacking entails more desirable practical consequences, there are conditions under which p-hacking is not as practically perilous as we might think. I use the formal resources from expected utility theory from the first part of the paper, and lessons learned from the arguments surrounding inductive risk to articulate the conditions under which this is the case. More specifically, I argue that there are hypotheses for which p-hacking is not as practically harmful as we might think.

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DESIGN EPISTEMOLOGY AS INNOVATION IN BIOMEDICAL RESEARCH

Session 31D

Congress section(s): C4

The idea of design has reached our theories of epistemology: a field that, at first glance, seems to be quite far removed from the analysis of practical situations. However, we should bear in mind that epistemology has shifted from an a priori perspective to a naturalized one, in the sense that we cannot engage in epistemology without taking into account the empirical results of science when it comes to configuring methodological models. In addition, the philosophy of science has expanded its field of analysis beyond pure science, and this has made it necessary to consider the epistemology of applied science. At this point it is relevant design epistemology, as an alternative to analytic epistemology.
The objective is to explore just how far design epistemology (DE) can be adopted as a methodological framework for research in biomedical sciences and in some of their applications as public health. To this end, we will analyze different approaches to DE and related terms and expressions such as “design thinking”, “design theory” and “designedly ways of knowing”. One of the issues that we need to address is precisely the polysemy that exists in the field of design, related to many different concepts. Thus, it seems impossible not to engage in a certain amount of conceptual analysis before we can embark on the study of the role of DE in public health research.
Another of the questions that we will consider here is where to place biomedical sciences within the field of academic knowledge and research. The disciplines involved in this field of research range from biology to medicine and also to the applications of these knowledges, as in the case of public health. We will examine some of the definitions provided by international organizations and we will locate public health within the framework of healthcare services and their organization. Finally, we will see how DE can offer proposals and solutions to the challenges that a phenomenon as complex as public health currently faces. That is, we will measure up DE proposals against public health research needs.
Design epistemology asks a whole series of questions which, at one and the same time, constitute different perspectives and proposals concerning how to understand the subject of DE itself. On the one hand, we have DE as an alternative to classic epistemology, which is often described as “analytic” and juxtaposed with “synthetic,” which is how DE would be described as it would also cover the applied sciences. On the other hand, DE is said to have a series of defining characteristics, among which we can highlight interdisciplinary as a means of addressing dynamic and complex problems; and a prominent element of social concern expressed through “design thinking” that revolves around human-scale design. Around these principal axes, we are going to examine a series of proposals and considerations relevant to biomedical sciences.

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VARIABLE SHARING PRINCIPLES IN CONNEXIVE LOGIC

Session 25F

Congress section(s): A2

Connexive logics are motivated by certain ways in which connection between antecedent and consequent in a true conditional is perceived, and especially in one that is logically valid. According to Routley, if the connection that needs to happen between antecedent and consequent in a true conditional is understood as content connection, and if a connection in content is achieved if antecedent and consequent are mutually relevant, “the general classes of connexive and relevant logics are one and the same”. However, it is well known that, in general, connexive and relevant logics are mutually incompatible. For instance, there are results such as the logic resulting from adding Aristotle’s Thesis to B is contradictory, and that R plus Aristotle’s Thesis is trivial. Thus, even though there is a sense which is probably lenient of ‘connection between antecedent and consequent of a true (or logically valid) conditional’ that allows to group in the same family both connexive and relevance logics, what Sylvan called at some point ‘sociative logics’, the types of connection that each type of logic demands are very different one to the other, to the point of incompatibility.
A fruitful way to study logics in the relevant family was by means of certain principles about the form that theorems should have, especially the purely implicative ones, or the relevant characteristics that an acceptable logically valid proof should have. This paper is part of a broader ongoing project in which we are investigating if we can pin down the similarities and differences between connexivity and relevance through principles of this kind. The general hypothesis of this broader project is that we can indeed do so; in this paper, we will only show that some connexive logics imply principles that could be considered as extremely strong versions of the Variable Sharing Property. Furthermore, we will show that there are conceptual connections worthy of consideration amongst that type of principles and some characteristics of certain connexive logics, such as the number of appearances of a variable in an antecedent, in a consequent or in a theorem in general.
This is important because relevance logics and connexive logics have some common goals, such as finding a closer bond between premises and conclusions, but at the same time, as we have said, there are strong differences between both families of logics.

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RETHINKING CYBERNETICS IN PHILOSOPHY OF BIOLOGY

Session 25H

Congress section(s): C3

In recent years, the contributions of cybernetics to the development of evolutionary developmental (evo-devo) biology have increasingly been recognised. The particular theories and models developed during the flourishing of cybernetics in the early 20th century laid the foundation for the systems approach, which is nowadays widely and fruitfully employed in molecular biology, genetics, genomics, immunology, developmental biology, and ecology. Nevertheless, no philosopher or biologist seems to know what cybernetics is, and often what they think they know they dislike: cybernetics is often identified with a reductive ‘machine conception’ of the organism and an engineering view of biology. However, once we understand what cybernetics is really about, we see such conceptions are mistaken and moreover that a cybernetic perspective can shed significant light on major discussions in current biology and its philosophy: in particular, on the fate of the Modern Synthesis in light of later developments in biology, the purpose and nature of evolutionary developmental biology, and disputes between those who emphasize a mechanistic conception of biology and ‘processualists’. Thus, my current research has two objectives: the first is to clarify the relationship between cybernetics and reductionism, and the second is to demonstrate the relevance of cybernetics to evo-devo. To accomplish the first objective, I will provide positive arguments for the thesis that, in contrast to the predominant view, cybernetic explanations within biology, when properly understood, are non-reductionistic, and do not have, at their core, any heavyweight metaphysical commitment to the mechanistic nature of life. To accomplish the second objective, I will disentangle the nature of cybernetics and reappraise its history in order to show how it offers new tools for approaching well-known neo-Darwinian controversies that have emerged in recent years.

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TURING REDUX: AN ENCULTURATION ACCOUNT OF CALCULATION AND COMPUTATION

Session 24F

Congress section(s): C5, C6

Calculation and other mathematical practices play an important role in our cognitive lives by enabling us to organise and navigate our socio-cultural environment. Combined with other mathematical practices, calculation has led to powerful scientific methods that contribute to an in-depth understanding of our world. In this talk, I will argue that calculation is a

practice that relies on the embodied manipulation of numerical symbols. It is distributed across the brain, the rest of the body, and the socio-cultural environment.

Phylogenetically, calculation is the result of concerted interactions between human organisms and their socio-cultural environment. Across multiple generations, these interactions have led from approximate quantity estimations and object-tracking to the cumulative cultural evolution of discrete, symbol-based operations. Ontogenetically, the acquisition of competence in calculation is the result of enculturation. Enculturation is a temporally extended process that usually leads to the acquisition of culturally, rather than biologically, evolved cognitive practices (Fabry, 2017; Menary, 2015). It is associated with plastic changes to neural circuitry, action schemata, and motor programs.

With these considerations in place, I will describe the recent cognitive history of computation. Based on Turing's (1936) seminal work on computable numbers, computation can be characterised as a specific type of calculation. Computational systems, I will show, are hybrid systems, because they are realised by the swift integration of embodied human organisms and cognitive artefacts in different configurations (Brey, 2005). Classically, computations are realised by enculturated human organisms that bodily manipulate numerical symbols using pen and paper. Turing's (1936) work built on this observation and paved the way towards the design of digital computers. The advent of digital computers has led to an innovative way to carry out hybrid computations: enculturated human organisms can now complete complex computational tasks by being coupled to computational artefacts, i.e., digital computers. Some of these tasks would be very difficult or even impossible to complete (e.g., in statistical data analysis) if human organisms were denied the use of digital computational artefacts.

In sum, in this talk I will argue that computation, understood as a specific kind of calculation, is the result of enculturation. Historically, enculturated computation has enabled the development and refinement of digital computers. These digital computers, in turn, help enculturated human organisms complete computational tasks, because they can be recruited as a reliable component of coupled hybrid human-machine computational systems. These hybrid systems promise to lead to further improvements of digital computational artefacts in the foreseeable future.

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IS THE NO-MIRACLES ARGUMENT AN INFERENCE TO THE BEST EXPLANATION?

Session 20J

Congress section(s): B1

As is well known, scientific realism about our best scientific theories is usually defended with the No-miracles argument (NMA). In its simplest form the NMA states that it would be a miracle, if our best scientific theories, i.e., theories enjoying tremendous empirical success such as mod-ern atomism and the theory of evolution were false. The NMA is usually explicated as an infer-ence to the best explanation (IBE): "Given a body of data find potential explanations for the da-ta, compare them with regard to explanatory quality, and infer the approximate truth of the best explanation." The assumption then is that our intuitive grasp of the notion of "explanatory quali-ty" generally suffices to apply IBE in practice, but that it is also a task for philosophical investi-gation to explicate this notion and show its truth-conduciveness.

I aim to compare IBE with another explication of the NMA, namely an improved version of hypothetico-deductivism, which I call HD+: "If T is a reasonably simple theory, the data is ex-cellent, and T together with auxiliaries covers the data, then T is approximately true." The condi-tion that T be reasonably simple is a standard way to ward off most cases of underdetermination of theory by all possible evidence. The condition that T cover D allows for probabilistic relations between T and D, not just deductive ones, stretching the meaning of "deductive" in "hypotheti-co-deductivism", but so what. Covering of D by T will normally require assistance from suitable auxiliaries and test conditions. Finally, data counts as excellent, if it exhibits good making fea-tures such as diversity and accuracy to a high degree. For example, the theory of evolution is supported by many independent lines of data. The main aim of my talk is to show that HD+ is a better explication of the NMA than IBE.

The most interesting difference between the two principles is the following. IBE tells us to construct rival theories, and compare them with regard to explanatory quality, whereas HD+ al-lows us to infer the truth of T without considering any rival theories of T. I claim that HD+ is right: If the data is excellent, we need not examine any rival theories of T to infer the truth of T. This claim conflicts with received wisdom about the confirmation of theories, so I have to pro-vide arguments in its support.

My first argument alludes to scientific practice. When a theory is supported by excellent da-ta, scientists usually don't consider rivals theories. A telling example is Perrin's famous argu-mentation for the atomic hypothesis in his book Atoms (1916). Perrin marshals the relevant data, determines its relationship with atomism, and notes its good-making features, most importantly its diversity and accuracy. He famously states that the data comes from 13 entirely different phenomena such as Brownian motion, radioactive decay, and the blueness of the sky. He does not engage in anything resembling IBE: He does not construct any rival explanations of the 13 different phenomena, compare them with respect to explanatory quality, and judge atomism to be the best explanation. The whole book is solely concerned with working out the 13 different applications of atomism. Perrin obviously thinks that this suffices to show that atomism is true. Thus, his reasoning accords nicely with HD+, but not with IBE. I examine another example from scientific practice, namely discussions of the empirical support for the theory of evolution in biology textbooks.

My second argument is general. It aims to show that under certain plausible assumptions ex-cellent data automatically refutes all reasonably simple rival theories, and we can know this without having to formulate and consider these rivals explicitly. I show how the second argu-ment can be embedded and justified in a Bayesian framework.

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PREDICATION ELABORATION: PROVIDING FURTHER EXPLICATION OF THE CONCEPT OF NEGATION

Session 4M

Congress section(s): A2

Full Opposition Symmetric Evidence Logic (FOS-EL) further explicates the concept of negation with machinery for positivity and negativity, extending explication given previously ([1997], [2000], and [2008]). Thereby FOS-EL can address, through analysis of axiomatizable extensions differentially realizing domain-dependent epistemological aspects of the concept of negation, questions related to negative facts long considered by Russell and others.

Earlier developed and studied [2000] Evidence Logic (EL) extends Classical Logic by providing an Evidence Space E of size any n>1, together with, for any i-ary predicate symbol P and any e in E, both confirmatory and refutatory evidential predications Pc:e and Pr:e respectively. Extending EL, FOS-EL is equipped with the further elaborating poseme and negeme evidential predications (posP)c:e, (posP)r:e, (negP)c:e, and (negP)r:e. Semantically, for any model with universe A, each of these evidential predications is a partial map from Ai to E.

Soundness and Completeness Theorems for both EL and FOS-EL are easily obtained through interpretations in carefully constructed related languages of Classical Logic (for these proofs, see [2000]).

The full opposition symmetry to be found in the further explication of the concept of negation in FOS-EL can be elucidated as follows. Consider the finite extension of FOS-EL with axioms which assert that for any evidence value e in E,

(posP)c: e iff (negP)r: e

and

(negP)c: e iff (posP)r: e.

In this extension of FOS-EL evidence which confirms posP refutes negP (and conversely) and evidence which confirms negP refutes posP (and conversely). Indeed, in this extension of FOS-EL there is asserted one realization of 'full opposition symmetry', a full symmetry between the oppositional poseme and negeme predications posP and negP.

This perspective might yield helpful insights regarding the long contemplated "negative facts" considered by Russell and others: there need be no negative facts out there in the (as yet incompletely understood) real world; a negative fact is just

a linguistic construct we ourselves make, confirmed by what refutes its corresponding positive fact and refuted by what confirms its corresponding positive fact.
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SIMPLICITY IN ABDUCTIVE INFERENCE

Session 18J

Congress section(s): B1, B2

Abductive inference is often understood as an inference to the best explanation, where an explanation is better than another one if it makes the evidence more plausible and is simpler. The main idea is that given a set of alternative hypotheses $\{H_1,...,H_n\}$ to explain some phenomenon E one ought to choose that H_i which has highest likelihood Pr(E|H) and is most simple/least complex in comparison to the other hypotheses.
It is clear what the epistemic value of making evidence plausible consists in. E.g., if one can establish even a deductive relation between H and E (as suggested by the DN-model of explanation), then the likelihood is maximal; if one cannot establish such a relation, then, whatever comes close to it or approximates it better, is epistemically valuable. However, regarding simplicity, it is debatable whether it bears epistemic value or not. According to the approach on simplicity of Forster & Sober (1994), one can spell out the truth-aptness of simplicity via constraints put forward in the curve fitting literature which are directed against overfitting erroneous data. Therein simplicity is measured via the number of parameters of a model. However, it remains open how the notion of simplicity spelled out in these terms relates to the notion of simplicity as is often used in abductive inferences, namely as the number of axioms or laws used in an explanation. In this talk we show how the latter notion is related with the former by help of structural equations. By applying an idea of Forster & Sober (1994) we show how probabilistic axioms or laws can be reformulated as structural equations; these in turn can then be used to assign numbers of parameters to such axioms or laws, and hence allow for applying established complexity measures which simply count the number of paramaters. By this, one can provide an exact translation manual for the “number of parameters approach” to the “number of axioms and laws approach”; this can be employed, e.g., in transferring the epistemic value of simplicity granted for the former domain to the latter one.

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ON THE ELUCIDATION OF THE CONCEPT OF RELATIVE EXPRESSIVE POWER AMONG LOGICS

Session 14L

Congress section(s): C1

The concept of expressive power, or strength, is very relevant and frequent in comparisons of logics. Despite its ubiquity, it still lacks a conceptual elucidation and this is manifested in two ways. On the one hand, it is not uncommon to see the notion of expressiveness employed in comparisons of logics on imprecise and varying grounds. This creates confusion in the literature and hardens the process of building upon other’s results. On the other hand, when care is taken to specify that the formal criterion of expressiveness being used is a certain E, there is generally no further comment on it, e.g. intuitive motivations or why E was chosen in the first place (E.g. in [KW99], [Koo07], [AFFM11] and [Kui14]). This gives the impression that the concept of expressiveness has been thoroughly elucidated, and it’s clearest and best formal counterpart is E. This is also misleading, since there are prima facie plausible but conflicting formal ways to compare expressiveness of logics. This work is intended to tackle these issues.
Formal comparisons of expressiveness between logics can be traced back to [Lin69], where a certain formal criterion for expressiveness (to be referred as EC) is given. No conceptual discussion or motivations are offered for EC, perhaps because it issues directly from Lindström’s concept of logical system (a collection of elementary classes). In [Ebb85] there is a very brief discussion in which a pair of intuitions for expressiveness is given, and it is argued that one would be captured by EC, and another by a new criterion EQ. Shapiro questions the adequacy of EC in [Sha91] due to its strictness and gives two broader criteria (PC and RPC). One motivation for the latter is that, as opposed to EC, they allow the introduction of new non-logical symbols in expressiveness comparisons. For example, in some logics the concept of infinitely many is embedded in a logical constant whereas in others, it must be “constructed” with the help of non-logical symbols. Thus, PC and RPC consider also the latent expressive power of a logic, so to speak.
Up to now, four formal criteria of expressiveness were mentioned. When comparing logics, all of EC, PC and RPC can be seen as mapping formulas in the source logic, to formulas in the target logic, with respective restrictions on the allowed mappings. This might be seen as too restrictive, as there are cases where a concept can be expressed in a logic but only using a (possibly infinite) set of formulas (e.g. the concept of infinity in first-order logic). If we allow that formulas in one logic to be mapped to a (possibly infinite) set of formulas in the target logic, we get three new criteria for expressive power: EC-D, PC-D and RPC-D.
Thus we have at least seven formal criteria for expressiveness, but in order to be able to choose between them, we need to select some intuitions for what it can mean for a logic to be more expressive than another. It will be seen that the seven criteria can be divided into two groups capturing each some basic intuition as regards expressiveness. In order to clarify what we mean by “the logic L‘ is more expressive than the logic L”, firstly we have to select some basic intuitions regarding expressive power, and then choose among the rival formal criteria intended to capture them. In order to do this, some adequacy criteria will be proposed, and the material adequacy of the formal criteria will be assessed.
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RIGIDITY AND NECESSITY: THE CASE OF THEORETICAL IDENTIFICATIONS

Session 20K

Congress section(s): A2

Kripke holds the thesis that identity statements containing natural kind terms are if true, necessary; he denominated those statements theoretical identifications. Kripke alleges that the necessity of theoretical identifications grounds on the rigidity of natural kind terms. Nevertheless, I will argue that the conception of natural kind terms as rigid designators, in one of their most natural views, hinders the establishment of the truth of theoretical identifications and thus of their necessity.
According to Kripke, one of the similarities between natural kind terms and proper names is that both sorts of expressions appear in identity statements that, if true, are necessary. Kripke exemplifies theoretical identifications by the statement "Water is H2O". Nevertheless, Kripke claims that this similarity follows from another one, namely that both sorts of expressions are rigid designators.
Kripke's definition of rigid designation for proper names is the following: a designator is rigid if it designates the same object with respect to all possible worlds. Since in the third lecture of Naming and Necessity he extends the notion of a rigid designator to natural kind terms, I will extend that definition to natural kind terms. The most literal extension, and the only one I will take as a basis for my considerations, is the following: A natural kind term is rigid if it designates the same kind with respect to all possible worlds. In this regard I will follow Kripke's assertions (see Naming and Necessity 1980: 135-136), which suggest that he conceives a natural kind as a type of universal instantiated in particular entities. The view of natural kinds as universals makes it possible for natural kind terms to be rigid designators. On this matter, I will adopt the following condition for the identity of natural kinds: two natural kinds are identical if and only if the instances of each kind are the same in all possible worlds.
The theoretical identity "Water is H2O" will be true if and only if the natural kinds water and H2O are identical. However, according to the condition for the identity of natural kinds proposed, this will hold if and only if the instances of both natural kinds are the same in all possible worlds, i.e., if and only if the terms "water" and "H2O" are coextensive in all of them. Notwithstanding, even if we concede that the extension of those terms is the same in the actual world, we cannot take into consideration each and every possible world to ascertain whether the instances of the natural kinds designated by those two terms are coextensive in all of them. Furthermore, I will allege that from the rigidity of the terms "water" and "H2O" and their coextensiveness in the actual world it does not follow their coextensiveness in all possible worlds, which is the condition to be satisfied for the truth – and necessity – of the statement "Water is H2O".
In other words, even if we accept that the terms "water" and "H2O" are rigid designators and that the extension of those terms is the same in the actual world, this does not lead to the conclusion that their referents – the universals designated by them – are identical, i.e., that the theoretical identity "Water is H2O", conceived as expressing an identity between natural kinds (universals), is true, although if it were true, it would also be necessary.

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ORGANISATIONS AND VARIABLE EMBODIMENTS

Session 29A

Congress section(s): A2, A3, C6, C8

Organisations are peculiar entities: during their lifetime, they can incur many changes, like acquiring or losing members, changing their organisational chart, changing the rules that regulate their activities and even changing the goal at which such activities aim. Nevertheless, there are many situations in which we refer to these apparently different entities as "the same organisation". What are the criteria that allow us to re-identify organisations through time and changes? Can such criteria be considered as characterising the identity of organisations? These are the questions at the basis of an ontological analysis of the identity and persistence of organisations. In this contribution, we first analyse organisations and their elements and components at a given time and then we enquire some of the possible organisational changes.

We leverage Kit Fine's theory of rigid and variable embodiment and see how such notions may be used to represent organisations, at a time and through change respectively.
In the attempt of specifying Fine's theory to characterise the case of organisations, we propose the (history of the) decisions taken by its members as what "glues together" successive states of an organisation (an organisation at different times) to form the organisation as an evolving whole, thus making this the element that drives its re-identification through time.
Finally, to exemplify our approach, we sketch a simple model in situation calculus.

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PARITY CLAIMS IN BIOLOGY AND A DILEMMA FOR INFORMATIONAL PARITY

Session 27H

Congress section(s): C3

The causal parity thesis states that there is a "fundamental symmetry" between genetic and non-genetic factors: "There is much to be said about the different roles of particular resources. But there is nothing that divides the resources into two fundamental kinds. The role of genes is no more unique than the role of many other factors." (Griffiths & Gray 1994: 277). Arguments for causal parity are typically intertwined with considerations regarding the received view of genes according to which they are information carriers, and are thus connected to what I will call "informational parity": "in any sense in which genes carry developmental information, nongenetic developmental factors carry information too." (Sterelny & Griffiths 1999: 101). The idea of an informational parity between genes and non-genetic factors is brought up in the literature in two related but distinct contexts:
1) In addressing the informational talk in biology: as a consequence of appealing to information theory, as opposed to some other approach to information.
2) In supporting the causal parity thesis: as a case, specification, or instantiation of the causal parity thesis.
While currently literature revolves around defending and refuting the causal parity thesis (e.g. Weber forthcoming), the aim of this paper is to examine informational parity in particular. I scrutinize the sort of reasoning leading to informational parity in both cases (1) and (2), and argue that it is grounded on a conflation between information and causation, in the sense that the two concepts are assumed to be undisputedly related. This underlying conflation has a twofold origin: (a) an implicit Millean view of causation, and (b) a misreading of information theory.
In relation to (a), I argue that if more fine-grained causal analysis can be used to refute the causal parity thesis, then this drags down any idea of informational parity that relies on causal considerations. Regarding (b), I argue that information theory does not entail any causal notion of information per se. The upshot is that no talk of informational parity as a "case" of causal parity is sufficiently motivated to the date, and parity following from a non-causal reading of information theory is uninteresting, on the other.
Informational parity, then, faces a serious dilemma:
Providing a satisfying defense of informational parity on causal grounds (i.e., based on the endorsement of the causal parity thesis) requires first determining what causal contribution is shared by DNA and other factors (where causation is understood in more fine-grained terms), and then pin down in what sense does this have anything to do with information (and what it is meant by this polysemantic term). There is no obvious way to achieve this.
Arguing for informational parity on non-causal grounds, in turn, is problematic for two reasons. First, because some of the available non-information-theoretic accounts of information in biology do rely on causal considerations, which brings us back to the problem above. Secondly (and more importantly), because many of them are precisely shaped to reject parity claims, which not only defies informational parity directly, but furthermore opens up the issue of ad hoc conceptualizations.

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ON CONTINUITY IN BOLZANO'S 1817 REIN ANALYTISCHER BEWEIS

Session 9C

Congress section(s): C1

1. Ancient Greek characteristic of continuity was encapsulated in the statement: continuous thing is divisible into parts that are infinitely divisible (see [1]). It was equally applied to space, time and motion. In modern science, space and time are represented by real numbers, assuming Rn models space and (R,<) models time, while motion is represented by function. Accordingly, continuity splits into continuous order (continuity of real numbers) and continuous function. We argue that this split of the meaning of continuity started in [2] and resulted from a duality of geometric line and function.
2. In [2], Bolzano seeks to prove Intermediate Value Theorem for polynomials (IVTp). Although he also proves Intermediate Value Theorem for continuous function (IVTf), it plays but a role of lemma in the proof of IVTp. Bolzano attempts to prove Cauchy Completness (CC) and then derives greatest lower bound principle (GLB) from it. Leaving aside possible circularity or mistakes, we argue that his most insightful contribution is the very formulation of GLB and defnition of the continuity of the function (DfCF).
3. Modern calculus adopted the following part of the Bolzano's proof. Since IVTp easily follows from IVTf , it is no longer considered to be a crucial proposition. However, IVTp does not depend on GLB as it holds in real closed fields.
4. In Preface, Bolzano criticizes "mechanical" and "geometrical" proofs of IVTp. The first is flawed by its appeal to "the concept of the continuity of a function with the inclusion of the concepts of time and motion". Therefore he presents his DfCF. The second is circular, since it relies on the "general truth, as a result of which every continuous function" has IVT property. We argue that there is the taken-for-granted assumption in Bolzano's argument that any line can be represented by some function. Marked by that duality of line and function, [2] belongs to the tradition (initiated by [2], developed in [4]) under which an analytic formula represents function, diagram represents line, and the relation between function and line is guaranteed by some nonmathematical conditions. That duality was covered by the arithmetisation of analysis, and then completed in the set-theoretic foundations of mathematics. Under the set-theoretic definition of function, a line is the graf of a function; when line is identified in Rn its continuity is related to GLB. On the other hand, the continuity of function is related to DfCF and echoes its "mechanical" provenance.

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MEDIEVAL DEBATES OVER THE INFINITE AS MOTIVATION FOR PLURALISM

Session 5J

Congress section(s): A4, B6, C1

A flourishing area of debate between medieval philosophers of the thirteenth and fourteenth centuries focused on paradoxes of the infinite and whether or not any notion of the actual infinite was permissible within natural science and mathematics. The fourteenth century in particular brought about a variety of views that attempted to provide both space for some form of the actual infinite and for rules that govern comparisons of size between infinities, such as the views of Henry of Harclay and Gregory of Rimini (Murdoch 1982). It has been suggested recently that these views provide historical evidence that there are natural intuitions behind comparing the size of infinite sets of natural numbers and against the inevitability of the Cantorian notion of cardinality (Mancosu 2009). In this paper I take this claim a step further and suggest that, upon charitable analysis of the medieval positions, one should not only accept that Cantorian notions were not historically inevitable, but one should take a pluralist attitude towards views of the infinite, with respect both to rules that govern comparisons of size for actual infinities and views that accept more restricted notions of the potential infinite.

To defend this view, I first set the medieval context by defending an interpretation of the relation between the Aristotelian view of the potential infinite and the medieval logical distinction between syncategorematic and categorematic uses of 'infinite.' I then look at a sample of views within two debates where the infinite plays a key role: (i) debates over the eternity of the world (and the type of infinity associated with an infinite past), and (ii) debates over the composition of continua (and the type of infinity associated with the parts of continua). In each debate, disagreements focus on which, if any, actual infinities are permissible in natural science, and how to avoid the corresponding paradoxes of the infinite. Background assumptions from theology, mathematics, metaphysics, and physics all play a role in the range of medieval arguments, and I extract from these a set of historically representative natural intuitions about the infinite. While these background assumptions might be incompatible, I argue that one should not rule them out based on their success or failure in the development of later mathematical views of the infinite. Rather, one can find in them reasonable epistemological and metaphysical criteria that can motivate multiple different philosophical views of the infinite, which naturally leads to a pluralist view of the infinite.

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A THEOREM OF ORDINARY MATHEMATICS EQUIVALENT TO ADS

Session 27K

Congress section(s): A1

In 1980 Ivan Rival and Bill Sands proved that for each infinite poset P with finite width (i.e. such that there is a finite bound on the size of each antichain in P) there is an infinite chain C such that each vertex of P is comparable to none or to infinitely many vertices of C. We are interested in analysing the strength of this statement, RS-po, restricted to countable posets, from the point of view of reverse mathematics. We proved that RS-po restricted to partial orders of width three is equivalent to ADS over the base theory RCA_0.

We think that this result is interesting for at least two reasons:

i) as far as we know, RS-po, restricted to partial orders of width three, is the first theorem of ordinary mathematics proved to be equivalent to ADS. In reverse mathematics ADS received attention as an easy consequence of Ramsey theorem, which is nonetheless strictly weaker than Ramsey theorem for pairs, but nor computably true nor provable from WKL_0. ADS shares this behaviour with many other statements, which are quite close, yet non equivalent, one to the other. This behaviour contrasts with that of the so called Big Five of reverse mathematics, which are characterized by a sort of robustness and by the equivalence to numerous theorems from different areas of mathematics;

ii) the original proof of RS-po goes trough in Π^1_1-CA_0. In fact, the proof in ADS is entirely different from the original one, which is on the other hand more general since it takes care of posets of arbitrary cardinality. Our proof requires a much more careful construction of the chains which witness that an ascending or descending chain is not a solution of RS-po.

We suspect that ADS is also equivalent to RS-po for posets of width n, for each $n \geq 3$. However, it is likely that the combinatorial complexity of the proof grows with n. On the other hand, we already know that ADS implies RS-po for posets of width two, but we do not have any reversal to ADS in this case. Thus RS-po for posets of width two may be a weaker principle.

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PLURALISM AND RELATIVISM FROM THE PERSPECTIVE OF SIGNIFICANCE IN EPISTEMIC PRACTICE

Session 6J

Congress section(s): B1, B2, B4

My paper examines a recent concept of scientific pluralism introduced by Hasok Chang (2012). I focus on Chang's (2015) response to the relativist critique based on Sociology of Scientific Knowledge (SSK) by Martin Kusch (2015). Furthermore, I discuss the separation of pluralism from relativism in general. My argument is that both positions have major deficits on a social-practical level. Therefore, I suggest improvements to Chang's pluralism, which might also be of interest to relativists.
Kusch's main argument (2015) against Chang (2012) is that the chemical revolution happened because there was no acceptable reason for the scientific group to consider phlogiston theory. He elaborates that there has never been a coherent phlogiston theory itself. In contrast, there have been good social reasons to accept oxygen theory. Chang does not agree. According to him, we should not seek "literal truth" (Chang 2012, p.219) in form of correspondence theory, but other possible ways "to maximize our learning from reality" (Chang 2012, p. 220). What distinguishes Chang from relativism is his way of defending pluralism as an important ideal of scientific research necessary to understand additional aspects of reality. His "active scientific realism" disagrees with the concept of theory unification. Chang argues for the coexistence of competing but somehow practical successful theories instead. Relativism based on SSK, in contrast, supports "good social reasons" as an acceptable argument for unification (Kusch 2015, p.76, 78).
From my point of view, Chang is correct if he argues against unification based on social normativity. (See for a parallel discussion on moral relativism by David Velleman (2015) also.) A relativist theory, which does not accept pluralism as a general aspect of scientific research, would not differ from scientific realism, as the kind of critical rationalism, on a practical level. The debate between realism and anti-realism would be empty if realism as well as relativism shared the same concept of theory unification. In contrast, Chang's version of pluralism obviously offers practical impact. Unfortunately, its position about social standards of research is unsatisfactory. Furthermore, I'm not convinced by Chang's (2018) newer claim of metaphysical pluralism as a concept which is coherent to realism.
Therefore, I suggest a more confident form of relativism, inspired by Nelson Goodman (1995 [1978]), which includes pluralism. I am convinced, that there is good reason to argue that nature as well as possibilities to interpret it are quite complex but constrained. However, from my perspective social-normative considerations play a major part in epistemology. As a result, pluralism must emphasize e.g. socially inducted aims of research as a reason for theory pluralism. I agree, that constrains of what we call nature influence the possible variability of theories. However, it does not mean that pluralism cannot constitute a convincing answer to the complexity of different social communities and their way of epistemology.
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DISPOSITIONS AND CAUSAL BAYES NETS

Session 24I

Congress section(s): B2

In this talk we develop an analysis of dispositions on the basis of causal Bayes nets (CBNs). Causal modeling techniques such as CBNs have already been applied to various philosophical problems (see, e.g., Beckers, ms; Gebharter, 2017a; Hitchcock, 2016; Meek & Glymour, 1994; Schaffer, 2016). Using the CBN formalism as a framework for analyzing philosophical concepts and issues intimately connected to causation seems promising for several reasons. One advantage of CBNs is that they make causation empirically tangible. The CBN framework provides powerful tools for formulating and testing causal hypotheses, for making predictions, and for the discovery of causal structure (see, e.g., Spirtes, Glymour, & Scheines, 2000). In addition, it can be shown that the theory of CBNs satisfies standards successful empirical theories satisfy as well: It provides the best explanation of certain empirical phenomena and can, as a whole theory, be tested on empirical grounds (Schurz & Gebharter, 2016).
In the following we use CBNs to analyze dispositions as causal input-output structures. Such an analysis of dispositions comes with several advantages: It allows one to apply powerful causal discovery methods to find and specify dispositions. It is also flexible enough to account for the fact that dispositions might change their behavior in different circumstances. In other words, one and the same disposition may give rise to different counterfactual conditionals if its causal environment is changed. The CBN framework can be used to study such behavior of dispositions in different causal environments on empirical grounds. Because of this flexibility, our analysis can also provide novel solutions to philosophical problems posed by masks, mimickers, and finks which, one way or another, plague all other accounts of dispositions currently on the market. According to Cross (2012), the "recent literature on dispositions can be characterized helpfully, if imperfectly, as a continuing reaction to this family of counterexamples" (Cross, 2012, p. 116). Another advantage of our analysis is that it allows for a uniform representation of probabilistic and non-probabilistic dispositions. Other analyses of dispositions often either have trouble switching from non-probabilistic dispositions to probabilistic dispositions, or exclude probabilistic dispositions altogether.
The talk is structured as follows: In part 1 we introduce dispositions and the problems arising for classical dispositional theories due to masks, mimickers, and finks. Then, in part 2, we present the basics of the CBN framework and our proposal for an analysis of dispositions within this particular framework. We highlight several advantages of our analysis. In part 3 we finally show how our analysis of dispositions can avoid the problems with masks, mimickers, and finks classical accounts have to face. We illustrate how these problems can be solved by means of three prominent exemplary scenarios which shall stand proxy for all kinds of masking, mimicking, and finking cases.
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PERSPECTIVES ON PROOFS

Session 13A

Congress section(s): C1

In this talk, we want to illustrate how to apply a general concept of perspectives to mathematical proofs, considering the dichotomy of formal proofs and textual presentation as two perspectives on the same proof.
We take *perspective* to be a very general notion that applies to spatial representation, but also phenomena in natural language syntax known as *perspectivation* and related to diathesis (grammatical voice) or semantically partially overlapping verbs such as *sell*, *buy*, *trade*; to phenomena in natural language semantics (e.g., prototype effects) and in narrative texts (Schmid, 2010 distinguishes perspective of characters or narrators in six dimensions, from perception to language). In most applications of the concept of perspective, a central question is how to construct a superordinate 'meta'perspective that accommodates given perspectives, while maintaining complementary information.
Perspectival phenomena intuitively have in common that different perspectives share some information and are partially 'intertranslatable' or can be seen as projections from a more complete and more fine-grained metaperspective to less informative or more coarse perspectives.
In our approach, modelling is done bottom-up starting from specific instances. We advocate a formal framework for the representation of perspectives as frames and using feature structures, a data structure well known in linguistics. With feature structures, it becomes easier to model the interaction of frames and approach compositionality, and connects to formal models of (unification-based) linguistic grammar like Berkeley Construction Grammar (cf., e.g., Boas & Sag, 2012), but also recent work on frame semantics (see, e.g., Gamerschlag, Gerland, Osswald, & Petersen, 2015). Metaperspectives are constructed using decomposition of features and types into finer structures (see Fisseni, forthcoming), organized in the inheritance hierarchies typical of feature structure models (see, e.g., Carpenter, 1992; Pollard & Sag, 1994). Using this formal model of perspectives, it can be shown that occasionally, e.g. in the case of metaphors, *partial* perspectives are used, i.e. that perspectives contain semantic material that is to be disregarded, for instance splitting notions semantic verb classes into different properties like *involving an agent* or *most prominent participant*.
Similar to syntactic perspectivation (active – passive, *buy* – *sell*), where the same event can be conceptualized differently (e.g., as an action or as a process) mathematical texts and formal proofs can be seen as describing 'the same proof' as a process and a state of affairs, respectively. The talk will show how to elaborate this analogy, and will discuss the construction of a metaperspective, i.e. merging both perspectives in a way that their common core will be distilled.
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THE TOPOLOGY OF INTERTHEORETIC REDUCTION

Session 13I

Congress section(s): B2, C2

Nickles (1973) first introduced into the philosophical literature a distinction between two types of intertheoretic reduction. The first, more familiar to philosophers, involves the tools of logic and proof theory: "A reduction is effected when the experimental laws of the secondary science (and if it has an adequate theory, its theory as well) are shown to be the logical consequences of the theoretical assumptions (inclusive of the coordinating definitions) of the primary science" (Nagel 1961, 352). The second, more familiar to physicists, involved the notion of a limit applied to a primary equation (representing a law) or theory. The result is a secondary equation or theory. The use of this notion, and the subsequent distinction between so-called "regular" and "singular" limits, has played a role in understanding the prospects for reductionism, its compatibility (or lack thereof) with emergence, the limits of explanation, and the roles of idealization in physics (Batterman 1995; Butterfield 2011).
Despite all this debate, there has been surprisingly no systematic account of what this second, limit-based type of reduction is supposed to be. This paper provides such an account. In particular, I argue for a negative and a positive thesis. The negative thesis is that, contrary to the suggestion by Nickles (1973) and the literature following him, limits are at best misleadingly conceived as syntactic operators applied to equations. Besides not meshing with mathematical practice, the obvious ways to implement such a conception are not invariant under substitution of logical equivalents.
The positive thesis is that one can understand limiting-type reductions as *relations* between classes of models endowed with extra, topological (or topologically inspired) structure that encodes formally how those models are relevantly similar to one another. In a word, theory T reduces T' when the models of T' are arbitrarily similar to models of T -- they lie in the topological closure the models of T. Not only does this avoid the problems with syntactically focused account of limits and clarify the use of limits in the aforementioned debates, it also reveals an unnoticed point of philosophical interest, that the models of a theory themselves do not determine how they are relevantly similar: that must be provided from outside the formal apparatus of the theory, according to the context of investigation. I stress in conclusion that justifying why a notion of similarity is appropriate to a given context is crucial, as it may perform much of the work in demonstrating a particular reduction's success or failure.
I illustrate both negative and positive theses with the elementary case of the simple harmonic oscillator, gesturing towards their applicability to more complex theories, such as general relativity and other spacetime theories.
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THE PHILOSOPHY AND MATHEMATICAL PRACTICE OF COLIN MACLAURIN

Session 9C

Congress section(s): C1

Colin Maclaurin was an 18th century mathematician, who was an important scientific figure during the Scottish enlightenment. Of particular note are his contributions in calculus and physics. He promoted Newtonian physics in Scotland, and also defended Newton's calculus from Berkeley's infamous philosophical objections (1734). Newton's philosophy of science currently draws much historical-philosophical interest. To fill in the mathematical-scientific landscape, research has branched into figures related to Newton, such as Galileo, Boyle, Du Châtelet, Clarke, etc. As the most successful contemporary defender of Newton's calculus, Maclaurin's philosophy is an important component of this ongoing research program.
In this talk I contrast two strands found in Maclaurin's work. On the one hand, he provides a rather sophisticated philosophical defense of calculus – one that is very similar to current structuralist philosophies of mathematics. This emerges when Maclaurin directly addresses Berkeley's philosophical questions regarding the nature of mathematics. One question is ontological: what are fluxions and infinitesimals? Another question is methodological: given that we seem not to know what fluxions or infinitesimals are, how can their use lead to truth; in other words, why should we trust the results of calculus?
Maclaurin's answer to Berkeley, and defense of Newton, is that it doesn't matter what fluxions are, or indeed whether or not they exist at all. What matters in mathematics are its relations, rather than its objects. That is, Maclaurin argues that mathematics is not about objects but relations between objects. Thus, what matters in accepting calculus is that its method

yields consistent, clear results not that its singular terms refer to objects that we can understand or know. In deflecting attention away from basic concepts and entities (infinitesimals, fluxions, etc.) and towards the method and consistency of calculus, Maclaurin's position is strikingly similar to that of early "methodological structuralists" such as Dedekind, Poincaré and others.

On the other hand, Maclaurin's mathematical work does not seem as modern as his philosophy. His notebooks, intermingle entries on pure math, applied math, physics and applied physics, suggesting a fairly casual approach to mathematical justification. Indeed, Sageng argues that Maclaurin was "Baconian", an empiricist about mathematics, which seems in direct conflict with any "structuralist" hypothesis. One way to ease the tension between these two strands, and defend Maclaurin, would be to rely on the distinction between justification and discovery, arguing that his more concrete reasonings are limited to the context of discovery. But this is not so clearly the case. He seemed equally comfortable reasoning via equations, mathematical diagrams and physical methods, sometimes moving between the three "modes" in one problem. The different approaches thus seem to overlap in his work. There thus appears to be a tension between his pragmatic approach to mathematical practice and his structuralist philosophy regarding the calculus.

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ABOUT THE WORLD DESCRIBED BY QUANTUM CHEMISTRY

Session 30K

Congress section(s): C2

One of the oldest problems associated with the interpretation of Quantum Mechanics is the role of the wave function in the ontology of the theory. Although Schrodinger himself posed the problem from the beginning of the theory, even today the meaning of the wave function remains the subject of debate. In this context, the problem of the 3N dimensions of the wave function is of particular interest in the philosophy of physics. As the wave function associated with a system of N particles is written in a space of 3N dimensions, it is necessary to ask about the meaning of this space. The debates around the issue have an important impact on the way in which we conceive the world around us. This is clearly manifested by the intense discussions that have taken place in recent years (Albert 2013, Allori 2013, Monton 2006).

In this work we will introduce a new perspective, coming from the way in which the wave function is used in scientific practice. Our objective is to investigate the ontology of quantum chemistry emerging from the analysis of the use of the wave function when quantum mechanics is applied to specific cases in the chemical domain. In the field of quantum chemistry there was not much discussion about the meaning of the wave function, and for this reason we consider that it can offer a fruitful context for a philosophical analysis. The typical many body system in this context is a molecule, and the typical problem is to find the energy spectrum of an electron that is in interaction with many other particles. To find such a spectrum, the so called orbital approximation is usually appealed to, which makes it possible to write the total wave function of the system as a product of mono-electron wave functions (Atkins & de Paula 2006). Under this approximation, the wave function of a given electron depends only on the variables of this electron; therefore, it evolves in the space of three dimensions (Lowe & Peterson 2006). In this presentation we will show that the procedure performed by chemists when they use the orbital approximation can be formalized as the result of the application of two mathematical operations: first, a projection onto a subspace of the Hilbert space, and second, a change of variables. With the help of this formalization, we will go beyond the approximation itself and propose an ontology specific to quantum chemistry.

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TRUTH-VALUES FOR TECHNICAL NORMS AND EVALUATIVE JUDGEMENTS: A COMPARATIVE ANALYSIS

Session 9D

Congress section(s): C8

The notion of technical or technological knowledge is philosophically interesting insofar as it differs from the forms of descriptive knowledge studied extensively in epistemology and philosophy of science. Whatever the differences between various forms of such knowledge – say knowledge of empirical facts and knowledge of laws or theories – discussion seems overwhelmingly structured by the concepts of truth-values and truth conditions. As for technical knowledge, in contrast, a wide variety of different forms has been proposed – including means-ends knowledge, procedural knowledge, tacit knowledge, know-how – which have in common that it is controversial whether they allow to be analysed through the concepts of truth-values and truth conditions. This is in particular the case for the more specific form of technical norms – which have the form 'If your goal is to realize A, you should perform action B', and which have been proposed by Ilkka Niiniluoto as a central constituent of technical or practical research as opposed to scientific research. It has been claimed by Niiniluoto that technical norms have a truth-value, whereas this was earlier denied by Georg Henrik von Wright, who was one of the first philosophers to emphasize the significance of claims with this structure for all disciplines which deal with action. See for a recent discussion Zwart, Franssen & Kroes 2018. In this talk I aim to shed light on this controversy by placing it in a comparative analysis with the work on normative statements by Judith Jarvis Thomson (2008). What makes her work interesting for this comparative analysis is that according to Thomson there is no significant difference between instrumental and moral evaluative claims – say 'This is a good person' and 'This is a good umbrella' – an approach quite similar to the one adopted earlier by Von Wright. But like Niiniluoto, Thomson claims that such statements have truth-values, a claim she extensively defends in her book. Her defence hinges on an interpretation of evaluative judgements like 'This is a good K' where the property 'a good K' can only be meaningfully understood as 'good as a K' or 'good qua K', and by extension 'good for what Ks are good for'. In addition Thomson claims that all evaluative judgements commend something. When combining these two elements, we have moved considerably in the direction of a judgement of the form 'If your goal is A, then you should perform action B'. Thus it may be worthwhile to see which of the arguments developed by Thomson, or ascribed to her opponents, can be transferred to the context of philosophy of technology. This in order to argue, finally, much more systematically than has been done so far, the relevance of work from core 'traditional' analytic philosophy for outstanding problems in the philosophy of technology and engineering.

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POPPER AND THE QUANTUM CONTROVERSY

Session 11C

Congress section(s): B6

It is almost a truism to say that the philosophy of science systematized by Karl Popper was heavily influenced by the physics intellectual landscape. Indeed, the most conspicuous working example of his methodology of falsifiability as a criterion to discern science from other forms the knowledge was Einstein's predictions drawn from his general relativity. While familiar with the great physical theories elaborated till the beginning of the 20th century, Popper kept a lasting fascination for the quantum theory. In addition there was among physicists a controversy over the interpretation and foundations of

this scientific theory, which further attracted Popper. However, the very technical aspects of this controversy kept Popper far away from this controversy as some of his early incursions in the subject were target of criticisms. It was only from the 1960s on, with the blossoming of the interest in this scientific controversy and the appearance of a younger generation of physicists interested in the subject, that Popper could fulfill his early desire to take part of this controversy. Most of his ideas on the subject are gathered in the volume Quantum Theory and the Schism in Physics. Popper's ideas may be encapsulated in the statement that he fully accepted the probabilistic descriptions and suggested his propensity interpretation to deal with it, thus without attachment to determinism, while criticized the introduction of subjectivist approaches in this scientific domain, thus aligned with the realist position in the quantum controversy. Less known is that Popper went further in his engagement with the debates over the meaning of the quanta. He could make this through the collaboration with physicists such as Jean-Pierre Vigier and Franco Selleri, who were hard critics of the standard interpretation of quantum physics. From this collaboration emerged a proposal of an experiment to test the validity of some presumptions of quantum theory. Initially conceived as an idealized experiment but eventually led to the lab benches by Yanhua Shih, it spurred a debate which survived Popper himself. We present an overview of Popper's concerns on quantum mechanics as well as an analysis of the debates about the experiment he had suggested.

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SOME RECENT APPLICATIONS OF MODEL THEORY

Session 17A

Congress section(s): A1

After some general remarks we will explain recent applications of model theory which use, in an essential way, structural results coming from stability theory.
The first application centers around automorphic functions on the upper half plane, for instance, the j-function mapping the generator of a lattice to the j-invariant of the associated elliptic curve. The central problem of interest involves understanding which algebraic varieties V have the property that j(V) is an algebraic variety. We call such varieties bi-algebraic. The philosophy is that the bi-algebraic varieties should be rare and reveal geometric information about the analytic function. At least two general sort of approaches using model theory have emerged in the last decade. The first involves o-minimality and the second involves the model theory of differential fields, applied to the algebraic differential equations satisfied by the analytic functions. We concentrate on the second approach in this talk.
The second application is related to machine learning. In the last several years, the dividing line between learnability/non-learnability in various settings of machine learning (online learning, query learning, private PAC learning) have proved to be related to dividing lines in classification theory. By using structural results and inspiration from model theory, various new results in machine learning have been established. We will survey some of the recent results and raise a number of open questions.

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HETEROGENEOUS MATHEMATICAL PRACTICES: COMPLEMENTING OR TRANSLATION?

Session 5C

Congress section(s): C1

Taking a look on how braid theory was researched between the 1925 and 1950s, one may say that there was a growing tendency towards formalization as well as separating the topological definition of braid group from the algebraic one. This resulted in a distorted narrative where diagrams and diagrammatic reasoning had at best illustrative role (presenting visually known results) but were not prompting any discoveries, that is, were not considered as epistemological tools. However, a closer look discovers that these diagrams were having between the 1920s and the 1940s a dual role (see: Friedman 2018), being both illustrative and epistemological, both aspects could hardly be separated – and both were operating with the algebraic-symbolical reasoning.
A similar situation occurs when one examines the research of complex curves during the last quarter of the 19th century and the first quarter of the 20th century. As it was difficult to visualize these curves (being naturally embedded in a 4-dimensional real space), different methods were used to help the student as well as the researcher to "see" these curves: either by constructing three-dimensional models from different materials, coloring these models to visualize the fourth dimension to sketching various diagrams, which illustrated other aspects of these curves. These visual and material aspects were – as in the case of braid theory – having several roles at the same time: they were not only illustrating already known properties but also giving rise to new discoveries, acting as if they have their own reasoning.
Taking these two case studies into account, the question that stands at the center of my paper is how this variety of mathematical practices is to be analyzed together: how these heterogeneous practices – symbolical, diagrammatical and material – interact together? Do they complement each other or prompt the concept of proof as a hybrid, when several practices are interwoven? Or may one practice be translated into another? Focusing on the concept of "translation" in the broader sense of the word, can one characterize the interaction between these practices and its development as "translation" between practices? More concretely, I propose that insights from translation studies may be employed – such as "translation up" and "down" (Bellos 2011) – to explore the relationship between these different practices. That is, as argued by David Bellos, translation can be seen as an expression of a specific set of cultural values, but it can also seek actively to hide its nature as a translation, as something foreign. The question that arises is whether "translation" from one mathematical practice to another (diagrammatical to symbolical, material to diagrammatical etc.), can be (or was) seen as a "translation up", "down", or just as non-functioning.

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DISTURBING TRUTH

Session 27D

Congress section(s): B1

The title is deliberately ambiguous. I wish to disturb, what I take to be, common conceptions about the role of truth held by philosophers of science and taxonomically inclined scientists. 'Common' is meant in the statistical sense the majority of institutionally recognised philosophers of science and scientists.

The common view is that the various pronouncements of science are usually true, and a few are false. We eventually discover falsehoods and expel them from science. The purpose of scientific research is to discover new truths. The success of science consists in discovering truths because it is through them that we are able to make true predictions and, through technology, control and manipulate, nature. Fundamental science consists in finding fundamental truths in the form of laws. These are important because they allow us to derive and calculate further truths. With this view, there are obvious problems with the use of defective information in science. We can address these problems in a piecemeal way. And the fact that defective information does not prevent scientists from carrying out their scientific enquiries suggests the piecemeal approach.
I shall present a highly idiosyncratic view which by-passes the problems of defective knowledge. The view is informed by what we might call "uncommon" scientists. According to the uncommon view: all "information" (formulas or purported truths of a theory), in science is false or defective. The purpose of their research is to understand the phenomena of science. Provisionally, we can think of 'understanding' as truth at a higher level than the level of propositions of a theory. The success of science is measured by opening new questions, and finding new answers, sometimes in the form of deliberately incorrect or counter-scientific information. Fundamental science does not stop with laws, but digs deeper into the logic, mathematics, metaphysics, epistemology or language of the science.

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IF KILLING IS FORBIDDEN, DO I HAVE TO ENSURE THAT NO ONE IS KILLED?

Session 9L

Congress section(s): A2

Often general obligations are what Humberstone in [3] called agent-implicating: the subject of the obligation is also the agent of the action in question. Consider, for instance, the claim
(A) Killing is forbidden.
The most natural interpretation of (A) is that (1) for all x, it is obligatory for x that x does not kill, and not (2) for all x, it is obligatory for x that for all y, y does not kill. What the latter would come to is that everybody is under an obligation that nobody is killed, and this is not what we normally mean by (A). Compare this, however, with a quantified version of an example in [4]:
(B) Managers are under an obligation that their company's financial statement is reported to the board.
As McNamara points out, a manager fulfills this obligation even if her assistant files the report. Therefore, unlike in the case of (A), an interpretation of (B) that is not agent-implicating is most plausible. Another important difference between both obligations is that, whereas (A) applies to everybody in the domain, (B) only applies to those who satisfy a certain condition. We shall call the latter obligations role-specific.
In this talk we will present a term-modal deontic logic (TMDL) that distinguishes between and can capture reasoning with the above-mentioned agent-relative obligations: general obligations that are role-specific or not, and that are agent-implicating or not.
TMDL is based on the epistemic term-modal logics from the work of Fitting, Thalmann and Voronkov. In [1] they extend first-order logic with modal operators indexed by terms. This allows them to quantify both over the objects in the domain and over accessibility relations that are linked to those objects. They give sound and complete sequent-style and tableau-based proof systems with respect to an increasing domain semantics.
We will argue that increasing domain semantics are needlessly complicated when it comes to capturing deontic reasoning, and show that the constant domain semantics of TMDL is more appropriate for capturing deontic reasoning with agent-relative obligations. We will also include identity and illustrate how this increases the expressivity of the logic and its ability to capture deontic reasoning. Partly based on [5], a sound and strongly complete Hilbert-style axiomatization of TMDL will be given.
We will compare our approach to different accounts for agent-relative obligations from the literature (such as Humberstone's agent-implicating obligations from [3], McNamara's personal obligations from [4], and Hansson's general obligations from [2]), and show that we are not only able to combine the available notions in a natural way, but that we can also model them in a more fine-grained way. We will argue that precisely because of this, we can do better justice to the various kinds of general obligations in natural language.
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PARTICLES, FIELDS, OR BOTH? A REEVALUATION OF THE RITZ-EINSTEIN DEBATE

Session 19I

Congress section(s): C2

According to its standard interpretation classical electrodynamics is a theory with a dualist ontology: a particle-field theory according to which electromagnetic fields mediate interactions among charged particles. Similarly, in quantum electrodynamics photons are treated as fields while electrons are treated as discrete particles. Even though the particle-field ontology is widely accepted, the dual ontology results in both conceptual and mathematical problems, chiefly among them the problem of self-interaction. In this talk I will review some of these problems as they emerge in classical electrodynamics and survey potential solutions to them. One pair of solution strategies, of course, involves getting rid of either particles or fields in favor of a monistic ontology (see the contributions by other speakers in this symposium). As a historical background for the other talks in this symposium I will reexamine a debate surrounding one particular attempt at developing an action-at-a-distance theory of classical electromagnetism: Walter Ritz's theory and his debate with Albert Einstein in the first decade of the twentieth century. While Ritz's theory ultimately was not successful, the debate sheds important light on some of the conceptual problems of field-theoretic frameworks, on the one hand, and action-at-distance theories on the other, and puts into focus how the debate between the competing frameworks is shaped by philosophical or methodological presuppositions as well as by the frameworks' formal or mathematical problems.

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RETHINKING THE TRANSFORMATION OF CLASSICAL SCIENCE IN TECHNOSCIENCE: ONTOLOGICAL, EPISTEMOLOGICAL AND INSTITUTIONAL SHIFTS

Session 16K

Congress section(s): B1, B6

The key tendency in the development of science in the present-date society is that the scientific knowledge produced in Academia by scientists and academic society is losing its privileged position; moreover, the science as an institution is losing its monopoly to the production of knowledge that is considered powerful, valuable and effective. This process of deep transformation has been partially reflected in such concepts as technoscience, post-academic science, transdisciplinarity and can be found in such practices as deprofessionalization of scientific knowledge, civil science (expertise), informal science

exchange in social media. In our presentation we aim to put for further consideration some ideas discussing not so much causes but purposes and effects of this transformation – epistemological, institutional and social. In particular we will focus on new subject (entity, person) of knowledge and its position in society and on the crucial change in the mechanisms of the scientific knowledge production that may lead to replacement of scientific knowledge by technologies (complex machines, techniques, skills, tools, methods) and innovations.

The key theses we will develop in our presentation:

1. Basically, the concepts of technoscience, post-academic science and transdisciplinarity register and show various aspects of science transformation into something new, which we continue to call “science” only due to institutional and cultural reasons. Indeed, science is a project of the Modern Time, which was artificially extended by historization of scientific rationality; and apparently it has come to its end as any historical formation. It seems that “technoscience” is probably the best general term (though subject to a number of restrictions) to denote what we still call “science” but what, in fact, is not science anymore, however it is consistently taking the place /position of science in the society.

2. The term “technoscience” emphasizes an entanglement of science and technology and it was mainly raised to distinguish a “new” type of scientific activities from “traditional” ones with a different epistemic interest producing different objects with a different ontological status.

Yet, for us it is important, that the concept enables us to capture the drastic changes in the means of production of knowledge and its organization. We claim that scientific knowledge is gradually turning into a component of innovative development and this means that scientific knowledge and academic science as an institution are becoming conformed to the principles and rules of functioning of other social spheres – economics, finance, and industry. The governance, business and society require the science to produce not the true / veritable knowledge to understand and explain the (real) world but give information and efficient ‘‘knowledge’’ to create a world, a specific environment, and artefacts.

Indeed, we can see that the key functions of natural sciences are crucially changed as they become the part of capital circle-flow: the main task is production of potentially commercialized findings which can be constantly reinvested with the purpose of getting innovation. At the same time ‘‘innovation’’ has been shifted from new idea in form of device to provision of more-effective products (technologies) available to markets to a cycle of capital extended development, which uses new technology as permanent resource for its growing.

3. Apparently, the development of scientific knowledge will go in the direction of further synthesis with the technologies, the strengthening of the latter component, and partially the substitution of scientist by machines and scientific knowledge with technologies in two forms: producing artefacts (artificial beings and substances) and machines as well producing skills (techniques, algorithms) of working with information or giving and presentation of information. Now we can clearly see it on the examples of the explosion of emerging technosciences (e.g. artificial intelligence, nanotechnology, biomedicine, systems biology and synthetic biology) or the intervention of neuroscience based on wide use of fMRI brain scans into various areas of human practice and cognition which results in the formation of the so-called ‘‘brain culture’’.

In general, transformation of science into “technoscience” implies that the production of information, technologies and innovations is its key goal. Thus, we can claim that science task is considerably narrowing as it implies the loss of scientific knowledge key function to be the dominant world-view (Weltanschauung). This loss may provoke other significant transformations.

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ITERATED BELIEF REVISION AND DP POSTULATES

Session 5L

Congress section(s): A2

It is well-known that the AGM theory is for one-step revision and does not provide a suitable account for iterated belief revision. Darwiche and Peral proposed some postulates (DP postulates, for short) to deal with the possible modes of a sequence of new information rather than a single one. But it surprised us that DP postulates conflict with AGM postulates in some aspects, for example, Lehmann pointed out one of the DP postulates, say DP2 conflicts with AGM postulates. Furthermore, Nayak et al. proved that the problem remains even though the DP2 has been weakened to the form as ¬x is consistent and ¬x is the logical consequence of y then the result of belief state K revised by x then revised by y would be identical with K revised by y alone. In order to block the inconsistency between DP postulates and AGM postulates, Nayak et al. proposed an additional postulate, i.e. Conjunction Postulate to dissolve the conflict. Unfortunately, though Nayak et al. are correct to show that DP postulates are too permissive, the Conjunctive Postulate they proposed is too radical on the other hand. In this paper, I will examine the strategy proposed by Jin and Thielscher and provide a different way to reconcile the DP postulates and AGM postulates.

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AUTOMATED REASONING WITH COMPLEX ETHICAL THEORIES – A CASE STUDY TOWARDS RESPONSIBLE AI

Session 14F

Congress section(s): A3, C8

The design of explicit ethical agents [7] is faced with tough philosophical and practical challenges. We address in this work one of the biggest ones: How to explicitly represent ethical knowledge and use it to carry out complex reasoning with incomplete and inconsistent information in a scrutable and auditable fashion, i.e. interpretable for both humans and machines. We present a case study illustrating the utilization of higher-order automated reasoning for the representation and evaluation of a complex ethical argument, using a Dyadic Deontic Logic (DDL) [3] enhanced with a 2D-Semantics [5]. This logic (DDL) is immune to known paradoxes in deontic logic, in particular “contrary-to-duty” scenarios. Moreover, conditional obligations in DDL are of a defeasible and paraconsistent nature and thus lend themselves to reasoning with incomplete and inconsistent data.

Our case study consists of a rational argument originally presented by the philosopher Alan Gewirth [4], which aims at justifying an upper moral principle: the “Principle of Generic Consistency” (PGC). It states that any agent (by virtue of its self-understanding as an agent) is rationally committed to asserting that (i) it has rights to freedom and well-being; and that (ii) all other agents have those same rights. The argument used to derive the PGC is by no means trivial and has stirred much controversy in legal and moral philosophy during the last decades and has also been discussed as an argument for the a priori necessity of human rights. Most interestingly, the PGC has lately been proposed as a means to bound the impact of artificial general intelligence (AGI) by András Kornai [6]. Kornai’s proposal draws on the PGC as the upper ethical principle which, assuming it can be reliably represented in a machine, will guarantee that an AGI respects basic human rights (in particular to freedom and well-being), on the assumption that it is able to recognize itself, as well as humans, as agents capable of acting voluntarily on self-chosen purposes.

We will show an extract of our work on the formal reconstruction of Gewirth’s argument for the PGC using the proof assistant Isabelle/HOL (a formally-verified, unabridged version is available in the Archive of Formal Proofs [8]). Independent of Kornai’s claim, our work exemplarily demonstrates that reasoning with ambitious ethical theories can meanwhile be successfully automated. In particular, we illustrate how it is possible to exploit the high expressiveness of classical higher-order logic as a metalanguage in order to embed the syntax and semantics of some object logic (e.g. DDL enhanced with quantication and contextual information) thus turning a higher-order prover into a universal reasoning engine [1] and allowing for seamlessly combining and reasoning about and within different logics (modal, deontic, epistemic, etc.). In this sense, our work provides evidence for the flexible deontic logic reasoning infrastructure proposed in [2].

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ON BOLZANO'S EARLY REJECTION OF INFINITESIMALS

Session 18B

Congress section(s): B6

Bernard Bolzano introduced the notion of variable quantities ω in his work on the binomial theorem as an alternative to the "selbst widersprechenden Begriffe unendlich kleiner Grössen" (1816: XI). The latter, he wrote, postulated quantities that were de facto smaller instead of quantities that could be smaller than another, and it compared those quantities with any alleged or conceivable and not merely with any given ones (cf. 1816: V). Because of such notion and the procedures associated with it, the Rein analytischer Beweis (1817) of Bolzano has been traditionally considered as an "epoch-making paper on the foundations of real analysis" (Ewald, 1999: 225). That way, his definition of a continuous function as that for which "der Unterschied f(x+ω)-fx kleiner als jede gegebene Grösse gemacht werden könne, wenn man ω so klein, als man nur immer will, annehmen kann" (1817: 11-12), is usually interpreted as equivalent to the later definitions of Cauchy and Weierstrass.

In this paper we will examine Bolzano's mathematical diaries written until 1818 in order to provide a better understanding of his careful definition of quantities ω, which he used in his published mathematical works from 1816-1817. As will be shown, despite the fact that Bolzano's mathematics hinted at some ground-breaking features and concerns, there is an intrinsic difference between his notion and the Weierstrassian ε. In particular, there seems to be enough evidence to sustain that his alternative concept was rooted in a certain distinction made between actual and potential infinite. That way, in a note dated around December 1814, he stressed that quantities ω should be understood as the assertion "dass man zu jedem schon angenommenen [Grösse] ein noch kleineres (grösseres) annehmen kann" (BGA 2B7/1: 79). As Bolzano's Reine Zahlenlehre shows, his later developments involved a "major change" (Russ & Trlifajová, 2016: 44) on the notion of infinity, as he went on to accept what he described in a note from 1818 as the "noch zweifelhaft" concept of infinitely small quantities (BGA 2B9/2: 126).

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SPACETIME AND FUNDAMENTAL PARTS

Session 17H

Congress section(s): C2

According to contemporary physics, there are given many different pictures about what is spacetime. For example, general relativity deals with spacetime as if it were a dynamically interacting and interacted structure rather than an entity (Dorato 2000). This idea is structural realism applied to spacetime and not spacetime points but metric relationally given by them is fundamental in realism of spacetime. That is to say, it is not relata but relations which are ontologically fundamental. This picture has a new understanding of ontology about theoretical entities even classical physics can provide (Esfeld &Lam 2006). This structuralist picture of macro spacetime resembles that of micro quantum particles (Fujita 2018). It's because in quantum mechanics, while each of micro particles composing a macro matter can be regarded as an individual, particles are also interpreted as a physical field. Ontological structural realism can solve this underdetermination by asserting there are only structures which exist in our world (Ladyman and French 2003). Structural spacetime realism is a third position resolving traditional discussions between substantivalism and relationism (Dorato 2000).

In addition, quantum theory of gravity can give other pictures. String theory regards spacetime as a fixed background not as a dynamical structure. Moreover, loop quantum gravity predicts that spacetime consists of more fundamental parts, which can be given by quantizing metric itself, through so called a spin network. This idea implies spacetime is substance-like and has some parts in micro regions. Even if these pictures quantum theories of gravity provide are not compatible with a structural interpretation in macro regions, it wouldn't be strange in a sense. For general relativity and quantum theories of gravity are different physical theories. General relativity may be an imcomplete and approximate theory of another comprehensive one. If so, macro structural interpretation from philosophical viewpoints would be obliged to be revised by the comprehensive one.

But loop quantum gravity directly quantizes Einstein-Hilbert action calculated from metric field. This quantum theory admits general relativity in macro regions and describes quantum effects in micro regions. Hence an interpretation about micro spacetime derived from loop quantum gravity should be consistent with one about macro spacetime. These quantum effects are very important in discussing a very early small universe resulted from the standard big bang cosmology. Based on a structural viewpoint, at the beginning of our universe, which was ever as small as planck scale, there were the only structure of spacetime and it doesn't mean elastic space was very small that time (Fujita 2017). Structural spacetime never has substance-like parts. This picture apparently conflicts with spin networks, in which three dimensional space is described with some discrete nodes connected by edges in a new phase space. Nodes and edges can be interpreted to represent three-dimensional volumes and two-dimensional surfaces in physical space respectively meaning our actual space has the minimum size and consists of many atoms respectively. However, the correspondence from an abstract phase space to real physical space involves some emergency (Huggett Wuthrich 2018). I want to assert that in micro regions, spin networks are not space itself despite that they must be ingredients of space and that a structural interpretation about macro spacetime never conflicts with a micro substance-like (meaning with a minimum atomic unit) picture. Spacetime is a structure without no primitive identity on spacetime points and doesn't have a size nor a shape, but a picture of "atom of space" is independent from the status of spacetime.

If the above two macro and micro interpretations hold water, structural spacetime is emergent from fundamental spin networks. This means substance-like entities bear a structural spacetime. Surely whether the atom of space can be regarded as substance or not needs a deep metaphysical discussion as well as quantum particles. Likewise, the structural interpretation may also be applied to the phase space described by nodes and edged. I am willing to consider how structural spacetime is based on more fundamental entities.

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THE ANTHROPIC TECHNOLOGICAL PRINCIPLE

Session 16D

Congress section(s): C8

The idea that humankind is something transient is widely discussed in the modern philosophy. The nightmare of both humanistic and trans-humanistic philosophy is that humanity will inevitably face some insuperable limits for further development, or even worse, can appear an evolutionary dead-and. In public consciousness this idea is often represented by the scenarios like artificial intelligence will “conquer the world”.
K. Popper sais that there is only one step from amoeba to Einstein. He implies concrete methodological significance in this rhetorical statement. But this significance can be ontological. Approximately 3 billion years ago cyanobacterium started to produce oxygen. They just needed energy, so they received it from the water and filled the Earth atmosphere with oxygen. These bacterium didn‘t have the “aim” to produce oxygen, they satisfied their needs.
Nowadays homo sapiens produces the wide range of technical devices, including artificial intelligence, in order to satisfy its needs in communication, safety, energy supply and political power. Like in the case with cyanobacterium oxygen production, the technical development is not the aim, but just the instrument for humanity. If we continue the analogy between cyanobacterium and homo sapiens from the “Universe point of view”, we can suppose, that technical sphere as a co-product of human activity may be the precondition of a new form of being genesis, like oxygen atmosphere was the precondition of the more complex aerobic organisms genesis. But in this case the transience of humankind must be fixed ontologically.
So, we can develop the anthropic technological principle if we attempt to save the special ontological status of homo sapiens. It claims that technological development must be controlled in order to prevent complete human elimination in principle. Every technical device must be constructed in the way that makes impossible it’s full-fledged functioning, including self-reproduction, without human sanction. This principle is based on the strong supposition that we really can control the technical development.
The anthropic technological principle, in contrast with the anthropic cosmological principle, is pure normative. We can‘t use it in the explanatory scheme. It should be understand as the evolutionary defensive mechanism of homo sapiens.

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MARIO BUNGE: A PIONEER OF THE NEW PHILOSOPHY OF SCIENCE

Session 9F

Congress section(s): B6

Mario Bunge anticipated many of the ‘post-positivist’ arguments of Hanson, Kuhn, Feyerabend and the Edinburgh Strong Programme that were used to promote a skeptical view of science, a view that became entrenched in the final decades of the twentieth century giving rise to the ‘New Philosophy of Science’ (Brown 1977). But Bunge used the arguments to defend the veracity and value of scientific knowledge. Several years before the irruption of the new philosophy of science, Bunge was developing his own view in a place far away from the most important centers of study of philosophy of science. The result of this work was the publication in 1959 of the first edition of Causality (Bunge 1959). This was a striking event because it was not common for an Argentine philosopher to write a book in English and have it published by a prestigious publisher. But the most important thing is the novelty of the ideas expressed in that book.
At this point it is worthwhile to re-evaluate the image of Bunge. Many believe that Bunge is a physicist who has become a philosopher that defends a positivist doctrine. The reality is quite the opposite. According to his own statements, since a young age he rejected the subjective interpretations of of quantum mechanics and devoted himself to the study of physics in order to obtain the necessary elements to support his position. Bunge defended scientific realism and he argued against naive empiricism as well as against more sophisticated versions that focus knowledge on the activity of the subject.
Quantum mechanics also favoured the questioning of the concept of causality and the validity of determinism. Bunge then undertakes a double task: separating science from a narrow empiricism and reformulating causality and determinism in an adequate way. He proposes to differentiate causation from the causal principle that guides our beliefs and from the formulation of causal laws. It also separates causation from determinism to give rise to non-causal determinisms.
Bunge’s position regarding causality explains both his distancing from the interpretation of quantum mechanics provided by some theorists as well as from empiricist and Kantian conceptions that understood causality as a projection of conditions of the cognizing subject. His criticism of empiricism is based on considerations that advance ideas later exploited by anti-realists like Kuhn. However, Bunge’s arguments are aimed at rescuing, along with plausible versions of causality and determinism, a realist view of science.
One of the merits of Bunge’s Causality book is the prominence that he early gave to ideas that are usually attributed to the “new philosophy of science”: the thesis of the theory-ladeness of observation; the conviction that no scientific statement have meaning outside a theoretical system; and the belief that scientific development follows a pattern similar to that of biological evolution, so that scientific progress does not represent a progressive reduction but a progressive differentiation. According to Bunge, this differentiation, pace the “new philosophers of science”, means a genuine cognitive improvement rather than a change of beliefs.

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HIGHER METRISABILITY IN HIGHER DESCRIPTIVE SET THEORY

Session 26B

Congress section(s): A1

The problem of generalising the real line continuum has a long standing history in mathematics. Mathematicians have proposed different generalisations of the real line for many different purposes, see, e.g., [2]. Particularly interesting in this context is the work of Sikorski, see, e.g., [5]. His idea was that of generalising the real line by using a version of the classical Dedekind construction of the real line in which he substituted the natural numbers with sufficiently closed ordinal numbers. In particular, for an infinite cardinal κ Sikorski’s construction allows to define a real closed field extension of the real line which we will call ordinal real numbers over κ.
Fixing a regular cardinal κ and substituting in the classical definition of metric the real line with the ordinal real numbers over κ, one obtains a very well-behaved theory which naturally generalises the classical theory of metric spaces. We will call this theory the theory of κ-metric spaces. Using the theory of κ-metric spaces one can naturally generalise many notions and results from the classical theory of metric spaces, see, e.g., [6].
Descriptive set theory is one of the main branches of set theory whose main goal is the study of particularly well behaved subsets of the real line. One of the main tools of descriptive set theory Baire space, i.e., the space of countable sequences of natural numbers.
In the last few years set theorist have started a systematic study of generalisations of Baire spaces to uncountable cardinals, see, e.g., [4]. One of the first steps in this process is of course that of generalise classical notions and results from descriptive set theory. Particularly important in this context is the notion of Polish space, i.e., completely metrisable separable spaces. In this talk we will show preliminary results from [3] on how the theory of κ-metric spaces can be used in generalising the notion of Polish space. To do so we will first introduce the theory of κ-metric spaces. Then we will give the definition of κ-Polish space and present some basic results about these spaces. Finally we will consider the game theoretical generalisation of Polish space introduced by Coskey and Schlicht in [1] and we will show that, contrary to the classical case, the game theoretical and the metric notions do not coincide in general.
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WHAT IS IT LIKE TO BE FIRST ORDER? LESSONS FROM COMPOSITIONALITY, TEAMS AND GAMES

Session 27M

Congress section(s): A2

Is standard First Order Logic (FOL) with the usual Tarskian semantics the strongest possible finitary logic that permits quantification only over individuals? As pointed out by Hintikka and Sandu, building on Henkin's work on Branching Quantifiers, the answer is arguably negative. Indeed, it is possible to add to FOL "slashed quantifiers" (∃ y / x), meaning "there is some y, chosen independently from x". Such quantifiers can be found both in mathematical practice and in ordinary language; and the formalism obtained by adding them to FOL, called Independence-Friendly Logic (IF-Logic for short), is more expressive than FOL and has properties that are different from those of FOL but are nonetheless very interesting (for example, the law of the excluded middle fails in IF-Logic; but on the other hand, IF-Logic can define its own truth predicate). The original semantics for IF-Logic is a variant of the Game-Theoretic Semantics (GTS) of FOL; and, later, Hodges showed how Tarskian Semantics may be also adapted to it, essentially by allowing formulas to be satisfied or not satisfied by sets of assignments rather than by single assignments. This transition from single assignments to sets of assignments cannot be however avoided without losing compositionality. This result thus exposes the higher order quantification hidden in GTS's truth definition: indeed, even though the semantic games of the GTS for IF Logic or for FOL involve merely agents picking elements from the model, truth in GTS (for both IF Logic and FOL) is defined in terms of the existence of a winning strategy. And a strategy, in general, is not a first order individual.
Later, Vääanänen studied in detail the properties of Hodges' semantics, observing in particular that slashed quantifiers can be replaced with "dependence atoms" whose semantics corresponds precisely to database-theoretic functional dependencies. This sparked a programme of exploration of the logics obtained by adding other types of dependencies to Hodges' semantics (called also "Team Semantics") - or, equivalently, by adding the corresponding constraints over strategies the GTS of FOL. Aside from being of considerable theoretical interest, the resulting formalisms have seen applications to social choice theory, database theory, doxastic dynamics, physics, and more recently even to the study of causality.
However, it is not always the case that adding (in)dependency notions/operators to FOL (or, equivalently, adding strategy constraints to its GTS) brings the expressive power of the resulting formalism beyond the mere first order: for some of these extensions of FOL, like for FOL proper, the higher order quantification implicit in the definition of truth in GTS is merely apparent. In this talk I will discuss a number of results, from recent and not-so-recent work, that attempt to characterize such extensions; or, in other words, that explore the boundary between first order and higher order logics "from below", by adding more and more possible constraints over strategies to the GTS of FOL without plunging the entire formalism into higher order territory.

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EXPLANATION AND ONTOLOGY

Session 17J

Congress section(s): B4, C1

If statements about a kind of object feature in our best explanation of a phenomenon, does this mean that those objects exist? Some realists in ontological debates think so, arguing for their view on the grounds that the objects in question are indispensable for our best explanations. Others object that even if those objects did not exist, statements about them could still feature in our best explanations. This paper argues that there is some merit to the objection, but that explanations nevertheless have ontological implications.
§1 argues that even if we accepted inference to the best explanation, which is typically used to justify indispensability arguments, this would still not be sufficient reason to think that the objects involved in our best explanations exist. For, inferences to the best explanation are usually inferences from the explanatory power of a hypothesis to its truth, not the existence of the objects involved. So even if we assumed the legitimacy of inference to the best explanation, this only validates indispensability arguments for realism about truth in a domain, not indispensability arguments for the existence of objects.
This does not mean, however, that explanations are completely without ontological implications—§2 argues that explanations put constraints on our overall ontology. Indispensability arguments would be valid insofar as we can infer the existence of an object from the truth of statements concerning it (call such inferences truthmaker inferences). Which truthmaker inferences we accept seems to be affected by our ontological commitments: we expect someone who accepts an ontological commitment to one kind of object to also affirm the validity of truthmaker inferences for objects whose existence is no more contentious (to us) than that kind of object. Therefore, if an indispensability argument for one kind of object is sound, this implies the validity of indispensability arguments for objects whose existence we accept at least as readily.
§3 suggests a further implication that explanations have on our overall ontology. Our readiness to accept the existence of an object seems to vary with its epistemic accessibility to us—that is, if understanding of an object (were it to exist) would be more available to us, then claims about its existence seem less strange. At the same time, for us to recognise an explanation, the explanans has to play such an epistemic role for us that it elicits in us understanding of the explanandum, so objects in the explanans have to be at least as epistemically accessible to us as objects in the explanandum. Therefore, if we see that a statement about X-objects explains a statement about Y-objects, then we should judge an ontological commitment to X-objects as being at least as acceptable as an ontological commitment to Y-objects. So when certain objects are indispensable for our best explanations, this implies that given the existence of objects in the explanandum, those objects would exist.
The upshot for indispensability arguments is that they should be seen as arguments for realists about one kind of object to accept the existence of another.

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MEASURABLE EPISTEMOLOGICAL COMPUTATIONAL DISTANCES IN MEDICAL GUIDELINES PEER DISAGREEMENT

Session 11I

Congress section(s): A3, B1, B2, C4

The study of medical guidelines disagreement in the context of the epistemology of disagreement (Goldman, 2011, Christensen & Lackey, 2013) may strongly contribute to the clarification of epistemic peer disagreement problems encoded

in scientific (medical) guidelines. Nevertheless, the clarification of peer disagreement under multiple guidelines may require further methodological development to improve cognitive grasp, given the great magnitude of data and information in them, as in the case of multi-expert decision-making (Garbayo, 2014, Garbayo et al., 2018). In order to fill this methodological gap, we propose an innovative computational epistemology of disagreement platform for the study of epistemic peer evaluations of medical guidelines. The main epistemic goal of this platform is to analyze and refine models of epistemic peer disagreement with the computational power of natural language processing to improve modeling and understanding of peer disagreement under encoded guidelines, regarding causal propositions and action commands (Hamalatian & Zadrozny, 2016). To that effect, we suggest to measure the conceptual distances between guidelines terms in their scientific domains with natural language processing tools and topological analysis to add modeling precision to the characterization of epistemic peer disagreement in its specificity, while contrasting simultaneously multiple guidelines.

To develop said platform, we study the breast cancer screening medical guidelines disagreement (CDC) as a test case. We provide a model theoretic treatment of propositions of breast cancer conflicting guidelines, map terms/predicates in reference to the medical domains in breast cancer screening and investigate the conceptual distances between them. The main epistemic hypothesis in this study is that medical guidelines disagreement of breast cancer screening, when translated into conflicting epistemic peers positions, may represent a Galilean idealization type of model of disagreement that discounts relevant peer characterization aspects thereof, which a semantic treatment of contradictions and disagreement may further help to clarify (Zadrozny, Hamatialam, Garbayo, 2017). A new near-peer epistemic agency classification in reference to the medical sub-areas involved may be required as a result, to better explain some disagreements in different fields such as oncology, gynecology, mastology, and family medicine. We also generate a topological analysis of contradictions and disagreement of breast cancer screening guidelines with sheaves, while taking in consideration conceptual distance measures, to further explore in geometrical representation continuities and discontinuities in such disagreements and contradictions (Zadrozny & Garbayo, 2018).

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THE ROLE OF TV SERIES IN THE DEMOCRATIZATION OF SCIENCE

Session 10G

Congress section(s): B5, C7

The objective of this paper is to analyze the possibility of finding in the TV series a form of democratization or, at least, of divulgation of scientific knowledge. To develop this idea, we will focus on the proposal of democratization of science of Philip Kitcher and we will show this democratizing capacity of TV series through the case of Black mirror.

Kitcher (2001) proposes to us to think of ways in which alternative institutions can allow us to arrive at their ideal of “well-ordered science”. This model pretends to establish certain moral and political norms so that the scientific options are the result of an ideal of conversation or deliberation. In this sense, the TV series can be understood as a part or element of the information and education system that Kitcher talks about, that is, these can create narrations and transmit the content that should be disclosed by the scientific or political community itself.

In recent years, has emerged a new panorama of television audience. TV Series as Black Mirror, Westworld or Philip K. Dick’s Electric Dreams, among other, have incentivated a sophisticated viewer profile, which is interested in generic innovation, current themes and dystopian and scientific plots. The impact generated by this type of series in the viewer makes him reflect on the direction of scientific and technological developments. Through the case of Black Mirror, we will analyze how this kind of media product let to the spectator the opportunity to reflect on topics that are usually not familiar, such as the technological development linked to transhumanism or the moral problems that arise from scientific development.

In conclusion, this paper aims to show that through the different plots projected in TV series, the public can understand scientific controversies and incorporate the needs of others in the reflection on scientific and technological development.

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JUSTIFICATION OF BASIC INFERENCES AND NORMATIVE FREEDOM

Session 10L

Congress section(s): A2

In this paper, I consider some questions in the current research on the nature of logical inference, specially what is the justification for basic rules of deductive reasoning, such as modus ponens. After presenting the problem, I briefly explore Boghossian’s inferentialist solution and expose Wright’s objections. I examine the feasibility of Wright’s non-inferentialist approach on the basis of considering how a weaker notion of justification or entitlement allows us to speak about basic knowledge. Finally, I consider Broome’s dispositional analysis and the question about what kind of normativity it could be suitable in the case of basic logical knowledge.

Broome's proposal seems a viable solution to the problems found in Wright's approach, specifically, the difficulty of the 'Simple Proposal', namely, basic inferences can assume patterns that are not very solid and we sometimes make mistakes. However, unlike Broome and Boghossian, we believe that while rule-following implies inferring, inferring is not always reducible to following a rule. For Wright, the Simple Proposal contains the way in which we make an inference, considering it a mental action; but all action has a 'directivity', it is directed to something beyond the action itself. If we do not want to fall back into the regression to infinity produced by the recourse to intentionality, this can be understood as a guiding disposition. Finally, if we understand the inference in that way, we would have a rational warrant in a cognitive project. It is a kind of warrant that is not incompatible with knowledge, but is prior to it. Ultimately, I contend that it is necessary to take the notion of normativity in its broadest sense and stand for a certain normative freedom. The objectivity of logic is not affected if we could accept normativity 'in a broad sense'. In the case of rules as MP, we assume MP as a correct rule of reasoning because rationality allows it, not because it requires it. The question to clarify is what special kind of reasons are rules.

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SCIENTIFIC LAWS AND CLOSENESS TO THE TRUTH

Session 11G

Congress section(s): B2, B4

Truthlikeness is a property of a theory or a proposition that represents its closeness to the truth of some matter. In the similarity approach, roughly, the truthlikeness of a theory or a proposition is defined according to its distance from the truth measured by an appropriate similarity metric. In science, quantitative deterministic laws typically have a real function representation $F(x_1,...,x_n)$ in an n-dimensional mathematical space (sometimes called the state-space). Suppose law A is represented by F_A(x) and the truth in question (the true connexion between the magnitudes) is represented by F_T(x). Then, according to Niiniluoto (1982, 1985, 1987, 1998), Kieseppä (1996, 1996) and Kuipers (2000), among others, we can define the degree of truthlikeness of a law A with the Minkowski metric for functions:

Tr(A)=d(A,T)=(∫|F_A(x)-F_T(x)|^k)^(1/k)

We will expose a counterexample to this definition presented by Thom (1975), Weston (1992) and Liu (1999) and a modification of it that we think is much more clear and intuitive. We will argue then that the problem lies in the fact that the proposal take Tr to be just a function of accuracy, but an accurate law can be completely wrong about the actual "causal structure" of the world. For example, if y=x then y'=x+sin(x) is a highly accurate law for many purposes, but totally wrong about the true relation between x and y. We will present a modification of d into a new metric d_an that defines the truthlikeness for quantitative deterministic laws according to two parameters: accuracy and nomicity. The first parameter is correctly measure by the Minkowski metric. The second parameter can be measure by the difference of the derivatives. Therefore (for some interval n, m):

d^an (A,T)=(∫|F_A(x)-F_T(x)|^2)^(1/2)+(m-n)(∫|F_A'(x)-F_T'(x)|^2)^(1/2)

Where Tr(A)=d_an(A,T) and Tr(A)>Tr(B) if and only if d_an(A,T)<d_an(B,T).

Once defined in this way we can represent all possible laws regarding some phenomenon in a two dimensional space and extract some interesting insights. The point (0, 0) will correspond to the truth in question and each point will correspond to a possible law with a different degree of accuracy and nomicity. We can define level lines (sets of theories equally truthlike) and represent scientific progress as the move from a determinate level line to another closer to (0, 0), where scientific progress may be performed by a gain of accuracy and nomicity but in different degrees. We can define some values "a" of accuracy and "n" of nomicity under which we can consider laws to be truthlike in an absolute sense. We will see how can we rationally estimate this values according to the scientific practice.

Finally, we will apply our proposal d_an to a real case study. We will estimate the degrees of truthlikeness of four laws (Ideal gas model, Van der Waals model, Beattie–Bridgeman model and Benedict–Webb–Rubin model) regarding Nitrogen in its gas state. We will argue that

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A NATURALIZED GLOBALLY CONVERGENT SOLUTION TO THE PROBLEM OF INDUCTION

Session 19M

Congress section(s): B2

The problem of induction questions the possibility of justification of any rule of inductive inference. Besides, avoiding the paradoxes of confirmation is a prerequisite for any adequate solution to the problem of induction. The confirmation relation has traditionally been taken to be a priori. In this essay a broader view of confirmation is adopted. It is shown that evidence itself must be interpreted on empirical grounds by bridge-hypotheses. Thus I develop an interpreted inductive scheme which solves both paradoxes of confirmation as well as the problem of induction.

Since distinct interpreted partitions corresponding to the same evidence can be related by means of a unique testable bridge-hypothesis, the confirmatory relations can be univocally determined by assessing the latter. Only the partitions corresponding to adequate hypotheses stabilize into a nomic chain which reflects the admissible bridge-hypotheses. A duality thesis is deduced, according to which any alteration in the relations of inductive support produced as a consequence of changes in some partition of the inductive basis can be neutralized by restating the inductive basis in terms of a corresponding dual partition. Therefore, the two paradoxes of confirmation are rendered solvable dual problems. The

interpretative inductive schema also avoids Norton's "no-go" results, because inductive strengths will not only depend on the deductive relations within the algebra of propositions but also on the semantic relations thereof.
I invoke the formal methods of lattice theory and algebraic logic to reformulate the problem of induction. Thus, the application of the interpreted inductive scheme to the data of experience yields a system of hypotheses with a lattice structure, the ordering of which is given by a relation of material inductive reinforcement. In this framework the problem of induction consists in determining whether there is a unique stable inductively inferable system of hypotheses describing the totality of experience. The proof of the existence of such system results from the application of Knaster-Tarski fixpoint theorem. Proving the existence of this fixed point is tantamount to a formal means-ends solution to the problem of induction.
In this approach induction is locally justified, i.e. based on matters of fact; and globally justified, i.e. in the sense of topological closure. This avoids the problem of regress of inductive justifications in material theories of induction.
Finally, it is proved that interpretatively supplemented inductive schemes are globally convergent; that is, converge to an asymptotically stable solution without a priori knowledge of preferred partitions.
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INTIMATE DIARY OF AN AIDS PATIENT. AN APPROXIMATION TO THE "MEDICAL GAZE" WITH FOUCAULT AND GUIBERT

Session 18I

Congress section(s): C4

The work of Michel Foucault, Naissance de la clinique. Une archeologie du regard medical is a point of support to introduce us in the emergence and the procedure of the "medical gaze" as a criterion of truth and rationality of modern medicine, from which to find an objective knowledge of the disease supported by the body. These processes of objectification and typification of diseases have a great importance in the medical sciences. The same happens in all applied sciences and also in the social and human sciences. Taking Foucault's analyses as starting point, the objective of this work is to illustrate, through the literary activity of Hervé Guibert with his diary as an AIDS patient, the process of the "medical gaze" in the objectification of the disease.
The literary works of Guibert illustrated how, in the 80s and 90s of the last centuries, a whole series of resources, efforts, and people are deployed in the process of objectifying AIDS as a disease. At that moment, the nature and functioning of this disease were unknown, involving AIDS in the most extravagant rumors. It became necessary, therefore, the establishment and grouping of symptoms, to obtain all objective knowledge with which to identify the disease, the diagnosis, and prognosis from which to establish a medical treatment. The process of objectification of the AIDS as disease turns into a paradigm significant of analyzing, from the perspective of bioethics and biopolitics, the procedure of the "medical gaze". The study of specific cases of biographical types, such as the one analyses in this paper through the works of Guibert, is an essential source of relevant evidence. This analysis allows us to enter in questions such as the inhuman and instrumental relationship of the doctor/patient, the objectification and stigmatization of the homosexual body and the social panic.
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A STRUCTURALIST FRAMEWORK FOR THE AUTOMATIC ANALYSIS OF MATHEMATICAL TEXTS

Session 14A

Congress section(s): C1

As a result of the "practical turn" in the philosophy of mathematics, a significant part of the research activity of the field consists in the analysis of all sorts of mathematical corpora. The problem of mathematical textuality (inscriptions, symbols, marks, diagrams, etc.) has thus gained an increasing importance as decisive aspects of mathematical knowledge have been shown to be related to regularities and emergent patterns identifiable at the level of mathematical signs in texts.
However, despite the fruitfulness of text-driven approaches in the field, the concrete tools available for the analysis of actual mathematical texts are rather poor and difficult to employ objectively. Moreover, analytical techniques borrowed from other fields, such as computational linguistics, NLP, logic or computer science, often present problems of adaptability and legitimacy.
Those difficulties reveal a lack of clear foundations for a theory of textuality that can provide concrete instruments of analysis, general enough to deal with mathematical texts. In this work, we intend to tackle this problem by proposing a novel conceptual and methodological framework for the automatic treatment of texts, based on a computational implementation of an analytical procedure inspired by the classic structuralist theory of signs. Guided by the goal of treating mathematical texts, our approach assumes a series of conditions for the elaboration of the intended analytical model. In particular, the latter should rely on a bottom-up approach; be unsupervised; be able to handle multiple sign regimes (e.g. alphabetical,

formulaic, diagrammatical, etc.); be oriented towards the identification of syntactic structures; capture highly stable regularities; and provide an explicit account of those regularities.
A major obstacle the vast majority of existing NLP models present to match those requirements resides in the primacy accorded to words as fundamental units of language. The main methodological hypothesis of our perspective is that basic semiological units should not be assumed (e.g. as words in a given dictionary) but discovered as the result of a segmentation procedure. The latter not only allows to capture generic units of different levels (graphical, morphological, lexical, syntactical, etc.) in an unsupervised way, but also provides a more complex semiological context for those units (i.e. units co-occurring with a given unit within a certain neighborhood). The task of finding structural features can thus be envisaged as that of identifying plausible ways of typing those units, based on a duality relation between units and contexts within the segmented corpus. More precisely, two terms are considered of the same type if they are bi-dual with respect to contexts. The types thus defined can then be refined by considering their interaction, providing an emergent complex type structure that can be taken as the abstract grammar of the text under analysis.
In addition to providing a conceptual framework and concrete automated tools for textual analysis, our approach puts forward a novel philosophical perspective in which logic appears as a necessary intermediary between textual properties and mathematical contents.
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MODELING CREATIVE ABDUCTION BAYES NET STYLE

Session 24I

Congress section(s): B1, B2

In contrast to selective abduction and other kinds of inferences, creative abduction is intended as an inference method for generating hypotheses featuring new theoretical concepts on the basis of empirical phenomena. Most philosophers of science are quite skeptical about whether a general approach toward such a “logic of scientific inquiry” can be fruitful. However, since theoretical concepts are intimately connected to empirical phenomena via dispositions, a restriction of the domain of application of such an approach to empirically correlated dispositions might be more promising. Schurz (2008) takes up this idea and differentiates between different patterns of abduction. He then argues for the view that at least one kind of creative abduction can be theoretically justified. In a nutshell, his approach is based on the idea that inferences to theoretical concepts unifying empirical correlations among dispositions can be justified by Reichenbach’s (1956) principle of the common cause.
In this talk we take up Schurz’ (2008) proposal to combine creative abduction and principles of causation. We model cases of successful creative abduction within a Bayes net framework that can, if causally interpreted, be seen as a generalization of Reichenbach’s (1956) ideas. We specify general conditions that have to be satisfied in order to generate hypotheses involving new theoretical concepts and describe their unificatory power in a more fine-grained way. This will allow us to handle cases in which we can only measure non-strict (probabilistic) empirical dependencies among dispositions and to shed new light on several other issues in philosophy of science. We consider our analysis of successful instances of creative abduction by means of Bayes net models as another step toward a unified Bayesian philosophy of science in the sense of Sprenger and Hartmann (in press).

The talk is structured as follows: We start by introducing Schurz’ (2008) approach to creative abduction. We also explain how it allows for unifying strict empirical correlations among dispositions and how it can be justified by Reichenbach’s (1956) principle of the common cause. We then briefly introduce the Bayes net formalism, present our proposal how to model successful cases of creative abduction within this particular framework, and identify necessary conditions for such cases. Next we investigate the unificatory power gained by creative abduction in the Bayesian setting and draw a comparison with the unificatory power creative abduction provides in the strict setting. Subsequently, we outline possible applications of our analysis to other topics within philosophy of science. In particular, we discuss the generation of use-novel predictions, new possible ways of applying Bayesian confirmation theory, a possible (partial) solution to the problem of underdetermination, and the connection of modeling successful instances of creative abduction Bayesian style to epistemic challenges tackled in the causal inference literature.
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UNWITTING COMPLICITY: WHEN SCIENCE COMMUNICATION BREEDS SCIENCE DENIALISM

Session 5B

Congress section(s): B1, B5

The problem of science denialism, in recent years, is widely thought to have taken on a new sociopolitical urgency. While decision-makers have always been highly selective in how and when to defer to experts (and to whom), overt denial of scientific facts has moved from being a fringe phenomenon to being a determining factor in national policies (at least in some countries), e.g. concerning climate change and healthcare.
Certain segments of the public, too, seem to be susceptible to the allure of science denialism. Yet not all scientific topics seem to be equally affected by denialist movements. Instead -- similar to other pathologies of public communication, such as ‚fake news‘ -- science denialism seems to find an audience especially amongst those who consider certain scientific facts a threat to their deeply held convictions or self-image.
Dismissing such scientific findings, then, may be an (epistemologically flawed) attempt at reducing cognitive dissonance. Merely reiterating the scientific facts in the face of such attitudes is unlikely to change people’s minds and may even backfire. In order for science denialists to become willing to consider scientific evidence afresh, trust needs to be regained -- even where it was wrongly withheld.
Certain common modes of science communication, I argue, fail to do just that, and may even worsen overall attitudes towards science-at-large. In this paper, I offer a tentative taxonomy of some common ‚mistakes‘ (for want of a better term) in the communication of science and technology, which -- against the intentions of their communicators -- tend to elicit science-denialist attitudes.

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ADOLF GRÜNBAUM ON FREUD

Session 4C

Congress section(s): 0

Karl Kraus famously quipped that psychoanalysis is the disease for which it purports to be the cure. Adolf Grünbaum developed a more trenchant and detailed critique of psychoanalysis which to this day has not been adequately answered.

This presentation will briefly summarize some key points of that critique and provide some anecdotes about how psychoanalysis cured Adolf and how Adolf cured others of psychoanalysis.

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PROGRESSIVE METHODS FOR CAUSAL DISCOVERY

Session 4B

Congress section(s): B1, B2

Although conventional wisdom still holds that it is foolhardy to infer causal relationships from non-experimental data, the last two decades have seen the development of methods that are proven to do what was previously thought impossible. Typical methods proceed by a simplicity-guided schedule of
conjectures and refutations. Simpler models posit fewer causal relationships among variables. Algorithms infer the presence of causal relationships by iteratively refuting sharply-testable statistical hypothesis of conditional independence. Such methods are routinely proven to converge to the true equivalence class of causal models as data accumulate. Crucially, however, that convergence is merely pointwise — it is not possible to calculate ex-ante the amount of data necessary to have reasonable assurance that the output model is approximately correct. Furthermore, there are infinitely many alternative methods that would have similar limiting performance, but make drastically different conclusions on finite samples. Some of these methods reverse the usual preference for simple graphs for arbitrarily many sample sizes.
What justifies the seemingly reasonable procedures that are prominent in the literature? Some have responded to the dilemma by searching for stronger a priori assumptions that would guarantee that the search methods converge uniformly [Zhang and Spirtes, 2002]. But these assumptions are implausible, and amount to insisting that causal discovery is easier than it really is [Uhler et al., 2013]. What is needed is a success criterion for justifying causal discovery that is stronger than mere pointwise convergence, but does not insist on short-run bounds on the chance of error.
Say that a method is *progressive* if, no matter which theory is true, the objective chance that the method outputs the true theory is strictly increasing with sample size. In other words: the more data the scientist collects, the more likely their method is to output the true theory. Although progressiveness is not always feasible, it should be our regulative ideal. Say that a method is α-progressive if, no matter which theory is true, the chance that it outputs the true theory never decreases by more than α as the sample size grows. This property ensures that collecting more data cannot set your method back too badly. We prove that, for many problems, including the problem of causal search, there exists an α-progressive method for every $\alpha > 0$. Furthermore, every α-progressive method must proceed by systematically preferring simpler theories to complex ones. That recovers and justifies the usual bias towards sparse graphs in causal search.

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ON THE UNIFYING CHARACTER OF DISPOSITIONAL REALISM

Session 7F

Congress section(s): B4

Faced with the antirealist threat represented by the pessimistic meta-induction -one of the main arguments against traditional realism-, new versions of realism cease to conceive the truth of theories as a block but instead they address specific aspects of them. If it were possible to show that, through theoretical change, certain parts of past theories survived in our current theories, the realist could avoid the drastic consequences of the antirealist argument. This is exactly the direction assumed by selective realisms, among which it can be mentioned entity realism (Hacking 1983; Cartwright 1983), structural realism (Worrall 1989; Ladyman 1998; French 1999) and dispositional realism (Chakravartty 1998; 2007; 2013; 2017). The latter was previously labeled semirealism, which is the topic of the present paper.
According to Chakravartty, dispositional realism -unlike entity realism and structural realism- is more promising regarding the direction that contemporary scientific realism should take from now on. Indeed, semirealism not only is immune to the criticisms raised against both entity realism and structural realism, it also offers, among others, two additional virtues, namely: (a) it allows the unification of entity realism and structural realism, two positions originally presented as incompatible views; (b) it offers an integrated picture of three metaphysical concepts: causation, laws of nature and natural kinds. We will focus on these two explanatory virtues.
Chakravartty claims that the unifying and explanatory character of dispositional realism is the main argument in favor of the existence of dispositions. Dispositions are those causally relevant properties that make things behave the way they do under certain circumstances. Dispositions are essentially modal properties.
The first virtue is based on a distinction made between detection properties, on the one hand, and auxiliary properties, on the other. We examine and discuss the foundations of this distinction. We conclude that the alleged unification of entity realism and structural realism does not work: on the one hand, dispositional realism fails to overcome the objections formulated to entity realism and, on the other hand, it leaves out the most compelling feature of structural realism.
The second virtue attributed to dispositional realism, as we have just remarked, is the unification of three metaphysical concepts: causal powers, laws of nature and natural kinds. Chakravartty holds that this metaphysical unification contributes to give plausibility and economy to his dispositional view of realism. We critically analyse the epistemological function of each concept and conclude that the first two (causal powers and laws of nature) manage to perform the satisfactory work that a compelling version of selected realism requires. But regarding natural kinds we offer epistemological reasons to show that, in the framework of dispositional realism, they are dispensable. In other words, all we explain through natural kinds, we can also explain by applying only dispositions and causal laws. So, the postulation of natural kinds far from helping to the conceptual economy of dispositional realism produces an ontological inflation instead.
We end by some final remarks about the possibility of defending a more deflationary conception of realism which can satisfy, anyway, the main demands of a scientific realist.
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SCIENTIFIC REALISM AND THE REALITY OF PROPERTIES

Session 20G

Congress section(s): B4

Scientific realism is a claim about the reality of properties (Anjan Chakravartty). According to epistemological scientific realism we have more reasons to believe in the reality of some unobservable properties rather than to doubt their existence. Moreover, we have more reasons than not to believe that some laws, which connect properties such as volume, pressure and temperature (in the perfect gases law) are (approximately) true. Coherent scientific realists should understand truth in

a correspondence sense. What is the kind of reality that makes scientific statements true, what is the nature of their truth-makers? Then, we enter the territory of metaphysics.
I argue that what makes a statement true is a fact, namely the instantiation of a property in particular entities or, shortly, particulars. Particulars are individual property bearers which exemplify some properties. Instantiation should be construed as primitive. Attempts to analyze this notion leads to insuperable difficulties such as the Bradley regress. According to ontological scientific realism some instantiated unobservable properties exist and are involved in (approximately) true scientific laws.
Justified belief in some instantiated unobservable properties is grounded on an analogy with ordinary perceptual experience in agreement with moderate empiricist requirements. First, the reality of a property, such as solidity, is attested by its actual presence in perception, through various sensory modalities (seeing, touching, hearing...). Repeated perceptions of the same property must give invariant results. Second, we must have reasons to believe that there is a causal link (in the minimal Millian sense) between the instantiated property and its perception. Third, inference of the existence of something which is not actually observed, such as a mouse (cf. van Fraassen), can only be based on perceptions of properties, such as producing noises, losing hair etc. In fact, this is not an inference to the best explanation, but the empirical verification of the compresence of a set of instantiated properties by a "something", a property bearer that we call a "mouse".
Similarly, we are justified to believe in unobservable scientific properties if analogous requirements are satisfied. First, observations of the same property by means of various experimental methods must deliver concordant results. This is the requirement of invariance. Second, we must have reasons to believe that there is a causal relation between the observed property and the observation results. This is the requirement of causality. Third, the properties must be observable in principle, that is, they could be brought to our perception by means of adequate instruments, such as microscopes or telescopes. This is the requirement of observability in principle. To these three requirements, we must add a fourth, namely the requirement of measurability, in order to make possible the satisfaction of the first. Various methods of measurements of the same property should deliver concordant measurement results.
Summarizing, I argue that compliance with these four requirements give us strong reasons to believe that the truth makers of some scientific statements are properties whose instances in particulars are observable in principle.

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THE PRACTICE OF PROVING A THEOREM: FROM CONVERSATIONS TO DEMONSTRATIONS

Session 12H

Congress section(s): C1

In this talk, I will focus on mathematical proofs "in practice" and introduce as an illustration a proof of the equivalence of two presentations of the Poincaré homology sphere, which is taken from a popular graduate textbook (Rolfsen, 1976) and discussed in De Toffoli and Giardino (2015). This proof is interesting because it is given by showing a sequence of pictures and explaining in the text the actions that ought to be performed on them to move from one picture to the other and reach the conclusion. By relying on this example, I will propose to take into account Stone and Stonjic (2015)'s view of demonstrations as practical actions to communicate precise ideas; my objective is to evaluate whether such a suggestion can be of help to define what the mathematical "practice" of giving a poof is. Stone and Stonjic consider as a case study an "origami proof" of the Pythagorean theorem and base their analysis on certain aspects of the philosophy of language of David Lewis. According to Lewis (1979), communication naturally involves coordination; in principle, any action could be a signal of any meaning, as long as the agent and her audience expect the signal to be used that way; a conversation happens only when a coordination problem is solved. Formal reasoning is a particular form of coordination that happens on a conversational scoreboard, that is, an abstract record of the symbolic information that interlocutors need to track in conversation. Stone and Stonjic conclude that the role of practical action in a conversation is explained in terms of coherence relations: meaning depends on a special sort of knowledge— convention—that serves to associate practical actions with precise contributions to conversation; interpretive reasoning requires us to integrate this conventional knowledge—across modalities—to come up with an overarching consistent pattern of contributions to conversation. On this basis, I will discuss the pros of considering proofs as conversations: if this is the case, then non-linguistic representations like diagrams have content and mathematics is a distributed cognitive activity, since transformations in the world can be meaningful. However, some general problems arise in Lewis' framework when applied to mathematical proof: (i) does a convention of truthfulness and trust exist really?; (ii) how can we coordinate and update our conversational scoreboard table when we read a written demonstration? The interest of the talk will be to investigate the possibility of a link between philosophy of the mathematical practice and philosophy of language.
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CATEGORY THEORY AS A FORMAL LANGUAGE OF THE MECHANISTIC PHILOSOPHY

Session 24H

Congress section(s): C3

In this paper, I aim to recommend category theory to mechanists in the philosophy of science, particularly as a mathematical formal language for some basic ideas in the mechanistic philosophy. Category theory was introduced to capture commonly preserving structures between different mathematical objects. (MacLane 1998) I firstly show that category theory can be employed to formalize the fundamental characteristics of biological mechanisms such ontological dualism (entities and activities), the hierarchical relation between a mechanism and its components, and spatiotemporally organizational features of mechanisms. (Machamer, Darden, and Craver 2000; Bechtel and Abrahamsen 2005) Further, I am arguing that category theory is a more promising formal framework in that it easily overcomes some skeptical views of the so-called a quantitative approach based on Bayesian nets or causal graphs theory. (Casini et al. 2011; Gebharter 2014; Gebharter and Schurz 2016; Casini 2016) Proponents of this quantitative approach emphasize that their tool is useful to capture two characteristics of mechanisms, hierarchy and causal connectivity of components of the mechanisms. However, there had been some critics to point out that this approach fails to provide causal processes that involve complex spatial and chemical-structural relations (Kaiser 2016) and to defend the property of modularity. (Weber 2016) In contrast, Category theory admits to formalize spatial and chemical information by assigning morphisms from an object to another. And, each category is not only discrete but also connected by functors so that category theory provides us a way to grasp modularity. By Applying category theory to the mechanism of protein synthesis I will defend a new qualitative formal framework.
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APPLICABILITY PROBLEMS GENERALIZED

Session 31H

Congress section(s): B1, C2

The effectiveness of mathematics in physics has been topic of debate in the philosophy of science in the last decennia (see for example Steiner 1998; Pincock 2012; Bangu 2012). In their attempt to clarify the applicability of mathematics to physics, philosophers usually only focus on cases of applicability of mathematics to physics and ignore other kinds of application of (or to) mathematics. However, since the application of mathematics to physics is just part of the more complex interrelation between physics and mathematics, it might be that such an approach is actually too narrow. Maybe, if we better understand how this kind of application
(from mathematics to physics) compares to other kinds of application, we might be able to better understand the applicability of mathematics to physics as well.
A kind of applicability, which is usually not taken into account when dealing with the problem of math-to-physics application, is the application of physics to mathematics. This subject has been broadly neglected by the philosophical debate on the applicability of mathematics (to my best knowledge, Urquhart 2008a and Urquhart 2008b are the only relevant exceptions). Actually, in contemporary physics and mathematics there is a fruitful circulation of methods and representative strategies, in which not only mathematics can be effectively employed to modelize physics, but also physics can be fruitfully “applied” to mathematics to generate new
strategies of mathematical analysis. This (unreasonable?) effectiveness of physics in mathematics is still unheeded by the philosophical community and awaits to be explored.
The presupposition that these kinds of applicability are completely different from (and therefore not relevant for) the understanding of the applicability of mathematics to physics might well be wrong. If there were analogies between these three kinds of application, then we might exploit these analogies in order to offer a generalized account for mathematical application, and to better understand the complex relationship between physics and mathematics.
In this talk I am going to develop this suggestion. I will present some examples of math-to-physics, math-to-math, and physics-to-math application. Then I will make some considerations about the possible analogies that can be traced among them, and I will analyze whether these analogies might be of any help in clarifying the applicability problems and the relationship between physics and mathematics.
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ADOLF AND NATURAL RELIGION

Session 3C

Congress section(s): 0

Adolf Grunbaum was a great character and a great presence. Anyone who knew him well had stories about him. I will tell some, and recount his arguments against Natural Religion.

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SOME SEMANTIC PROPERTIES OF TYPED AXIOMATIC TRUTH THEORIES BUILT OVER THEORY OF SETS

Session 27I

Congress section(s): A1

Abstract. The study of axiomatic truth theories over set theoretical base theories was pioneered by [Krajewski(1976)] who proved the conservativity of $CT^{-}[ZF]$ over ZF.Many years later, his conservativity result was independently refined by [Enayat and Visser(2018)] and [Fujimoto(2012)] so as to yield the conservativity of the much stronger theory $CT^{-}[ZF] + Sep^{+}$ over ZF, where Sep^{+} is the natural extension of the separation scheme to formulae with the truth predicate. In our talk, we will focus on the semantic (model-theoretic) properties of theories of the truth predicate taken with set theory ZF or ZFC taken as the base theory. The model-theoretic study of truth theories was initiated in the classical papers [Krajewski(1976)] (over arbitrary base theories that include PA and ZF) and [Kotlarski et al.(1981)Kotlarski, Krajewski, and Lachlan] (over PA as the base theory). Soon thereafter, in a remarkable paper [Lachlan(1981)], it was shown that if a nonstandard model M of PA is expandable to a model of $CT^{-}[PA]$, then M is recursively saturated. It can be proved that the same result holds for ω-nonstandard models of ZF, so consequences of Lachlan's theorem impliy that not every model of PA (ZF) is expandable to a model of the compositional truth theory $CT^{-}[PA](CT^{-}[ZF])$. The above imply together that if $M \models$ is a countable ω-nonstandard model, then the following are equivalent:

1. M admits an expansion to a model $(M, Tr) \models {}^{-}[]$.
2. $\mathcal{M}$ is recursively saturated.

This characterisation, taken together with remarkable construction of [Gitman and Hamkins(2010)] shows that for the class of countable ω-nonstandard models of set theory admitting a compositional truth predicate is equivalent to belonging to the so-called natural model of the Multiverse Axioms. During the talk we intend to demonstrate the abovementioned results in more detail and explore their philosophical dimensions. Last, but not least, we will also show some relevant (to the topic of the symposium) properties of models of the so-called disquotational theories of truth (such as the so-called locally disquotational theory TB) over set theories, which has some philosophical implications in the debate on deflationism w.r.t. the concept of mathematical truth.

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MODAL QUANTIFIERS, POTENTIAL INFINITY, AND YABLO SEQUENCES

Session 10M

Congress section(s): A2

When properly arithmetized, Yablo's paradox results in a set of formulas which (with local disquotation in the background) turns out consistent, but ω-inconsistent. Adding either uniform disquotation or the ω-rule results in inconsistency. Since the paradox involves an infinite sequence of sentences, one might think that it doesn't arise in finitary contexts. We study whether it does. It turns out that the issue turns on how the finitistic approach is formalized. On one of them, proposed by M. Mostowski, all paradoxical sentences simply fail to hold. This happens at a price: the underlying finitistic arithmetic itself is ω-inconsistent. Finally, when studied in the context of a finitistic approach which preserves the truth of standard arithmetic (developed by AUTHOR), the paradox strikes back -- it does so with double force, for now inconsistency can be obtained without the use of uniform disquotation or the ω-rule.

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SECOND PATTERN EXISTENCE AND TRUTH-MAKING

Session 4H

Congress section(s): B4

Our general aim is to extend the applicability of the metametaphysics project (Tahko 2015). The new era within the metaphysics of science debate started from the D. Dennet's real patterns concept (1991). The possibility to use the real patterns to develop the proper theory of existence has shown by D. Ross (2000). Following this understanding J. Ladyman has developed his Information-Theoretic Structural Realism (2007). We will show how the using of an ongoing notions like ontological dependency, grounding or modal epistemology may help us to extend the Ladyman's concept and might even to solve some of it's particular problems.

Discussing his new interpretation of Ross's definition of existence, Ladyman noted: "in the new formulation of clause (ii) [pattern is real iff it encodes information about at least one structure of events] we want to go on saying something like what that clause now says about the information 'in' real patterns. But 'encode' is surely misleading;... we must no longer say that real patterns carry information about structures. So what does a real pattern carry information about? The answer can only be: about other real patterns. ... This just means, to put matters as simply and crudely as possible: it's real patterns all the way down" (2007, 227–8). And after the two pages full of the hard-hitting tries of explanation he concludes: "We have now explained the basis for replacing this [clause (ii)] by the following: 'It carries information about at least one pattern P'" (230). As far as we agree with the conclusion we think that something more needs to be (or at least might to be) said here about the metaphysical interpretation of the existence of the "second pattern". More than, what does it mean from the metaphysical perspective to be a second pattern of the second pattern? We find nothing wrong with the Ladyman's understanding of the role of projectability notion in the scale relative non-individuals-existent ontology. But following the traits of metametaphysics project we may note at least two directions of further discussion. First, it is tempting to assess the notion of projection via the truth-making perspective. Following B. Smith's understanding, truth-maker for a given truth should be part of what that truth is about: "roughly, it should fall within the mereological fusion of all the objects to which reference is made in the judgment" (1999, 6). He identifies the projection of a judgement with the fusion of all these things to which it refers (singularly or generically), and then defines what is to be a truth-maker in terms of this notion: a truth-maker for a judgement p is something that necessitates p and falls within its projection. What would such a truth-making projection mean for the "second pattern" existence within the Ross-Ladyman understanding of "projectability under the physically possible perspective"? The projection of a real (first) pattern needs to be made far more relevant to what the "physically possible perspective" is intuitively about. Second, we could make a next step and define truth-making in terms of

grounding. As J. Schaffer suggest: “if a proposition p is true at a world w, then p’s truth at w is grounded in the fundamental features of w. A truth-maker x for a proposition p at w is something such that (i) x is fundamental at w and (ii) the truth of p at w is grounded in x” (2008, 17). Is it possible to extend the Schaffer’s combining Ladyman’s and Tahko’s understandings of fundamentality? By the definition, grounding is an asymmetric, irreflexive and transitive relation, it thus induces a partial ordering, whose minimal elements are the “fundamental entities”. We think that proper interpretation of the truth-making account of the “second pattern” existence is possible. More than, if we are to succeed we will have a pretty metametaphysical explanation of the Dennett inspired search for the “real patterns all the way down”.

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WHY DO OUTCOMES IN A LONG SERIES OF ROLLING A FAIR DICE APPROXIMATELY FOLLOW THE UNIFORM DISTRIBUTION?

Session 4J

Congress section(s): C2

The talk will outline a new account of “probability” employed in statistical mechanics and in gambling games. The main ideas will be illustrated on the example of rolling a dice.

Suppose you roll a fair dice a large number of times. It is a robust physical fact that each outcome will occur with approximately the same, 1/6, relative frequency. The question I’d like to pose is: what is the physical explanation of this physical fact?

Consider the mechanical description of rolling a dice in terms of phase space. Each of the six outcomes carves out a region of phase space containing those initial conditions of rolling the dice that lead to the particular outcome. If the dice is fair, each of these six phase space regions has the same phase volume (Lebesgue measure) (cf. Kapitaniak et al. 2012). The rolling of the dice picks a point in phase space — the one corresponding to the initial condition realized by the roll. Suppose you roll the dice a large number of times: this picks a large sample from phase space. Now, in terms of phase space, the fact that the relative frequencies of outcomes are all around 1/6 means that the initial conditions realized in the series of rolls are approximately uniformly distributed among the six phase space regions in question. So if we are to understand why outcomes occur with approximately the same frequencies, what we have to ask is: what is the physical explanation of the fact that the initial conditions in the series of rolls are approximately uniformly distributed among the six phase space regions?

In the first part of the talk I shall review possible answers to this question based on discussion in the literature (e.g. Strevens 1998, 2011; Goldstein 2001; Frigg and Hoefer 2010; Maudlin 2011; Myrvold 2016). These answers invoke the principle of indifference, the method of arbitrary functions, the notion of typicality, and appeal to objective randomness provided by quantum mechanical phenomena. I will find all of these answers wanting.

In the second part of the talk I will outline a new answer to our question. My analysis will be based on two components: 1) Pitowsky’s (2012) result that the phase volume (Lebesgue measure) is a natural extension of the counting measure on finite sets; and 2) the Principle of the Common Cause, phrased in the form that if in a statistical ensemble two properties A and B are causally independent — that is, for any member of the ensemble that facts as to whether it possesses properties A and B have no direct nor common causal connection —, then the distribution of these two properties over the ensemble must be approximately statistically independent, with respect to the counting measure or its natural extension.

My account, which can easily be shown to extend to statistical mechanics, will have two main characteristics: 1) it essentially invokes unanalyzed causal terms; on the other hand, 2) nowhere it makes any reference to an irreducible notion of “probability.”

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TIME, CAUSALITY AND THE TRANSITION FROM TREE TO NETWORK DIAGRAMS IN THE LIFE SCIENCES

Session 15H

Congress section(s): C3

How we conceptualize and depict living entities has changed throughout the ages in relation to changing worldviews. Wheels of time originally depicted cycles of life or the cyclic return of the seasons that associated with circular notions on time. Aristotle introduced the concept of a great chain of being that became foundational for Judeo-Christian theorizing on scala naturae that originally associate with a-historic depictions of nature and later with linear timescales. Scales of nature in turn formed the basis for phylogenetic trees as they were introduced by the natural history scholars of the 19th century. And these trees are set in two-dimensional Cartesian coordinate systems where living entities are tracked across space and time. Today, the various disciplines that make up the evolutionary sciences often abandon tree typologies in favor of network diagrams. Many of these networks remain historically unrooted and they depict non-linear causal dynamics such as horizontal information exchange. We investigate these transitions and hone in on how networks introduce new concepts of causality. We end by delineating how a reconstruction of the genealogy of these diagrams bears larger consequences for how scientific revolutions come about.

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ORGANIZATIONAL ETIOLOGICAL TELEOLOGY: A SELECTED-EFFECT APPROACH TO BIOLOGICAL SELF-REGULATION

Session 17D

Congress section(s): C3

According to selected-effects theories (for instance, Neander 1991; Griffiths 1993), selection is a source of teleology: purposes are effects preserved or promoted through a selective process. Selected-effects theories are favored by several authors who want to claim that Darwinian evolution introduces teleology in the biological world.

For the purposes of this presentation, we take selected-effects theories for granted, although we will provide some motivation for them by appeal to certain response-dependent meta-normative views about value, more specifically, views according to which value is generated by evaluative responses. While most selected-effects theories concentrate on natural selection (for an exception, see Garson 2017), our goal is to argue that there are other types of selective processes in biology and that such processes should be seen as giving rise to distinctive types of evaluative standards. More specifically, we suggest that biological self-regulation (the mechanisms by which organisms monitor and regulate their own behavior and that has been the object of careful study in the biological sciences) can be seen as a selective process.

In general, biological organisms include dedicated regulatory mechanisms that compensate for possible perturbations and keep the state of the system within certain ranges (Bich et al 2015). The pressures that such self-regulatory submechanisms exercise on the states of the organism are a form of discriminatory reinforcement, as a result of which certain tendencies are inhibited while others are promoted. It is reasonable, therefore, to characterize biological self-regulation as a selective process.

So, those who accept selected-effects theories of teleology should also grant that biological self-regulation is a source of teleology – at least to the same extent that selected-effects theories are taken to vindicate the view that biological teleology is generated by natural selection. The purposes and evaluative standards introduced by self-regulation are independent of -and arguably sometimes conflicting with- the standards associated with natural selection. Given that self-regulation is ubiquitous in the biological world, it is to be expected that the evaluative standards generated by it will play a prominent role in our

explanations of biological phenomena. We think that the approach sketched in this paper offers an appealing integrative picture of the evaluative dimension of biology.
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LOGIC-BASED STRATEGIC REASONING IN SOCIAL CONTEXT

Session 24E

Congress section(s): A2

Reasoning in social context has many important aspects, one of which is the reasoning about strategic abilities of individuals (agents) and groups (coalitions) of individuals to guarantee the achievement of their desired objectiveswhile acting within the entire society. Various logical systems have been proposed for formalising and capturing such reasoning, starting with Coalition Logic (CL) and some extensions of it, introduced the early 2000s.
Coalition Logic provides a natural, but rather restricted perspective: the agents in the proponent coalition are viewed as acting in full cooperation with each other but in complete opposition to all agents outside of the coalition,which are treated as adversaries.
The strategic interaction in real societies is much more complex, usually involving various patterns combining cooperation and competition. To capture these, more expressive and refined logical frameworks are needed.
In this talk I will first present briefly Coalition Logic and then will introduce and discuss some more expressive and versatile logical systems, including:
i. the Socially Friendly Coalition Logic (SFCL), enabling formal reasoning about strategic abilities of individuals and groups to ensure achievement of their private goals while allowing for cooperation with the entire society;
ii. the complementary, Group Protecting Coalition Logic (GPCL), capturing reasoning about strategic abilities of the entire society to cooperate in order to ensure achievement of the societal goals, while simultaneously protectingthe abilities of individuals and groups within the society to achieve their individual and group goals.
Finally, I will take a more general perspective leading towards a unifying logic-based framework for strategic reasoning in social context, and will associate it with the related concepts of mechanism design (in game theory)and rational synthesis (in computer science).

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WHAT IS THE EXPLANATORY ROLE OF THE STRUCTURE-FUNCTION RELATIONSHIP IN IMMUNOLOGY?

Session 24H

Congress section(s): C3, C4

According to a common slogan in the life sciences, "structure determines function". While both ‚structure' and ‚function' are ambiguous terms that denote conceptionally different things at various levels of organization, the focus has traditionally been on the three-dimensional shapes of individual molecules, including molecular patterns that play a central role in immunology. The specificity of binding sites of antibodies and pattern recognition receptors has given rise to the so-called "Janeway paradigm", according to which the three-dimensional shape of molecules is the key to understanding immunological function: microbial pathogen-associated molecular patterns (PAMPs) bind specifically to pattern recognition receptors (PRRs) of the host and by "recognizing" signature molecular motifs of pathogens an immunological reaction is triggered. If correct, these molecular structures would be crucial for solving the riddle of how the immune system is able to distinguish between self and non-self – and between harmful and beneficial commensals.
However, this narrative faces a major challenge, as molecular motifs are being shared among pathogens and symbiotic commensals alike. Both express a similar set of molecular patterns that are specific for prokaryotes. Other instances are known in which one and the same molecular motif can trigger opposing immune reactions, depending on the presence or absence of additional signals in the cellular context. It is speculated that a second "danger" signal might be needed in order to trigger an immune response. Whatever the nature of this second signal might be, it will require stepping away from the fixation on molecular patterns. I argue that it is rather structural motifs of networks which carry the explanatory weight in these immunological processes. I suggest to distinguish between different meanings of ‚structure' and ‚function', to which separate explanatory roles can be attributed. While the three-dimensional shape of signature molecules (structure1) can be used to explain their function1 – understood as biochemical properties and activities – their immunological function2 – biological roles, like immunogenicity – can only be explained with respect to higher-level structures2, i.e. the interaction networks of molecules and cells. These different explanatory roles also imply different explanatory accounts. The former remains within a physico-chemical framework, whereas the latter rather calls for mechanistic and topological explanations. Studying the interaction topology and dynamics of structures2 with mathematical tools, modeled as signaling games, promises to shed new light on these interaction processes that increasingly get to be described as equilibrium states between multiple interaction partners by immunologists. Rather than focusing only on the presence or absence of molecular signatures, topological properties explain the features of these networks and their activities beyond the molecular interactions between PAMPs and PRRs. This way, opposing effects resulting from the same kind of molecular structure1 can be explained by differences in their "downstream" organizational structure2. While still preserving the centrality of structure-function relationships, I suggest to keep these conceptually different notions of ‚structure' and ‚function' and their respective explanatory roles apart.

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FUZZY SEMANTICS FOR GRADED ADJECTIVES

Session 10M

Congress section(s): A2

After decades of being considered unsuitable, the fuzzy approach to vagueness was reexamined and vindicated in Smith (2008) in the form of what he calls "fuzzy plurivaluationism". Roughly speaking, this approach takes, instead of a single fuzzy model, a set of various fuzzy models for the semantics of a vague predicate. While this proposal appears to have revived the interest in fuzzy logic as a tool for vagueness to some extent, its potential in formal semantics remains virtually unexplored. My aim in this talk is to vindicate and develop some aspects of the application of fuzzy plurivaluationism in the semantics of graded adjectives.
Firstly, in way of a rationale, I briefly argue that the fuzzy approach offers a desirable treatment of the Sorites Paradox. Secondly, I respond to some general objections typically raised against it – i.e. the artificial precision objection and that of the counterintuitive results of a degree-functional approach to the logical connectives. If successful, my answers show, against common belief, that the prospects of fuzzy accounts of vagueness are not doomed from the very beginning.
In the third (and last) place, I work out some of the details of the fuzzy analysis of graded adjectives. First, I show that we have available a variety of algebraic models which give us the means to analyse various kinds of graded adjectives and the constructions where they appear. For instance, we can give models not only for vague adjectives, but also for different sorts of absolute graded adjectives (i.e. those which display degrees of applicability, but have sharp boundaries, such as "acute"). Next, I argue that the fuzzy approach would avoid some of the problems of what can be considered the mainstream take on graded adjectives – i.e. the degree-based account stemming from Cresswell (1977) and developed, for example, in Kennedy (2007). One of the advantages of the fuzzy approach is that it simplifies the analysis of the positive unmarked form (i.e. the adjectival construction found, for instance, in "The woman is tall") by simply identifying it with the corresponding adjective and thus taking it to denote a fuzzy property (by contrast, the usual approach to the positive unmarked form requires either a type-shifting mechanism or the positing of a null degree morpheme). Finally, I provide support for the claim that a fuzzy

semantics can be used to analyse comparative statements successfully. In particular, I show that two alleged problems for fuzzy semantics in relation with these (i.e. the problem of comparative trichotomy and that of non-borderline comparatives) pose no real threat.

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HOW SCIENCE IS KNOWLEDGE

Session 4L

Congress section(s): B1

Truth is commonly required from a proposition to count it as knowledge. On the other hand, the hypothetical character of scientific laws and the ubiquity of idealization and the ceteris paribus clause pose the problem of how science is knowledge. Thus Ichikawa and Steup (2012) claim that scientific knowledge consists in knowing the contents of theories rather than knowing some truths about the world. Some, e.g. Cartwright (1983) suggest that general scientific laws, being false, constitute an instrument of knowledge rather than knowledge themselves. Contrary to that, Popper considers science as the superior kind of knowledge. Even outdated science, one may add, radically differs in epistemic status from prejudice or mere error and may have quite an extensive scope of application.

To solve this tension, I propose to adopt a version of contextualism that is in many respect close to that of Williams's (1996). On this view knowledge is a context-dependent notion, where a context is determined by some presuppositions, often only tacit, that are accepted by entitlement. Some presuppositions of the past, like Newton's absolute simultaneity, are not accepted any longer. Still, Newton's laws constitute knowledge in the context of his presuppositions and preserve a vast scope of applications. Idealizations and the ceteris paribus clause also count as presuppositions in Stalnaker's pragmatic sense. This explains how, in some contexts but not in others, one is entitled to ignore, e.g., friction.

The version of contextualism on offer departs in some significant respects from that of Williams's. First, presuppositions themselves are not included into knowledge. Instead, they form what Wittgenstein calls, in the face of the ultimate groundlessness of our believing, "a (shifty) river-bed of thought". Once it is shifted, however, in a novel, more comprehensive context one can come to know the denial of some presuppositions of a former, less sophisticated context. Second, the truth-requirement for knowledge is withdrawn. Instead, knowledge in a context is defined as belief that is superwarranted relatively to that context, i.e. it is warranted without defeat at some stage of inquiry and would remain so at every successive stage of inquiry as long as the presuppositions that constitute the context are kept unchallenged.

Apart from explaining how science, including outdated science, is knowledge, the account on offer pictures an aspect of the growth of knowledge that consists in falsifying some presuppositions. In a broader, epistemological perspective, it is also applicable to the general skeptical problem like the question of (im)plausibility of the brains-in-a-vat hypothesis.

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DIFFERENCES OF DISCOURSE UNDERSTANDING BETWEEN HUMAN AND SOFTWARE AGENTS

Session 24J

Congress section(s): A3

We are interested in the differences between how a human agent and a logic-based software agent interpret a text in natural language. When reading a narrative, the human agent has a single interpretation model. That is the preferred model among the models consistent with the available information. The model is gradually adjusted as the story proceeds. Differently, a logic-based software agent works with a finite set of many models, in the same time. Of most interest is that the number of these models is huge, even for simple narratives. Let the love story between Abelard and Heloise, with the text "Abelard and Heloise are in love". Assume during natural language processing, the statement is interpreted as Abelard is in love and Heloise is in love. The formalisation in First Order Logic is:

(A1) $\exists x$, love(abelard,x)
(A2) $\exists x$, love(heloise,x)

How many models does a model generator find for axioms (A1) and (A2)? Using MACE4 [McC03], with the domain closed to 4 individuals, there are 278528 models. All these models are equally plausible for the software agent.

To reduce this number, the agent needs to add several constraints. First, the unique name assumption can be added: (A3) abelard != heloise. Still, there are 163840 models. Second, we assume that the love relation is not narcissistic: (A4) $\forall x$, $\neg$love(x,x). That leads to 5120 models. Third, we add the somehow strong constraint that someone can love only one person at a time. That is (A5) love(x,y) $\wedge$ love(x,z) $\rightarrow$y=z. The remaining models are 80. Unfortunately, love is not a symmetric relation. Hence, we cannot add the axiom $\forall$x,y love(x,y)$\leftrightarrow$love(y,x). Instead we exploit the fact that some of these models are isomorphic. After removing isomorphic interpretations, we keep 74 non-isomorphic models. Note that there are 2 Skolem constants after converting axioms (A1) and (A2). If we are not interested in the love relations of individuals represented by these constants, we can ignore them. This would result in 17 models.

Some observations follow. First, the order in which we apply the reductions is computationally relevant. For instance, it would be prohibitively to search for isomorphic models in the initial two steps, when there are 278528 and 163840 models. Hence, the strategy is to add domain knowledge to the initial narrative discourse, and then to search for the isomorphic structures. Second, which domain knowledge to add is subject to interpretation. For instance, axiom (A5) might be too strong. Third, for some reasoning tasks (e.g. solving lateral thinking puzzles [DBZ10]) keeping all possible models might be desirable. Fourth, we argue that text models built with machine learning applied on big data, would benefit from some crash diet. In this line, we try to extract as much as we can from each statement, instead of statistically analysing the entire corpus. That is, the model of the story is built bottom-up and not top-down as machine learning does.

Both the human reader and the software agent aim to keep the story more intelligible and tractable. But they apply different reduction strategies. On one hand, humans understand stories by inferring the mental states (e.g. motivations, goals) of the characters, by applying projections of known stories into the target narrative [Her04], by extensively using commonsense reasoning [Mue14], or by closing the world as much as possible. On the other hand, logic-based agents reduce the models by formalising discourse representation theories [KR13], by adding domain knowledge, or by identifying isomorphisms.

We also analyse how the number of interpretation models vary as the story evolves. Sentences introducing new objects and relations increase the number of models. Sentences introducing constraints on the existing objects and relations contribute to the removal of some models. Adding domain knowledge also contributes to model removal. One question is how to generate stories that end with a single interpretation model for software agents. Another issue regards the amount of domain knowledge and commonsense knowledge to add, and which reduction strategy is better to keep the number of models computationally feasible.

To sum up, we compare the reduction strategies of humans and software agents to keep the discourse more intelligible and tractable.

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THE PRIVILEGE PROBLEM FOR SEMANTIC DISPOSITIONALISM

Session 30L

Congress section(s): C5

Semantic dispositionalism is the view that meaning can be analyzed in dispositional terms. Classic instances of the view are due to Dretske (1981), Fodor (1990), and Heil and Martin (1998). Following Kripke (1981), its enemies focus on three arguments: the finitude argument, the mistake argument, and the normativity argument. I describe a fourth anti-dispositionalist argument, which I dub "the privilege problem".
The best way to introduce the privilege problem is on the background of the finitude and mistake arguments, since while the finitude argument says that dispositionalism fails because we don't have enough dispositions and the mistake argument says that it fails because we have the wrong dispositions, the privilege problem claims that dispositionalism fails because we have too many dispositions. In particular, my strategy will be to first describe a careful implementation, due to Warren (forthcoming), of the standard way to deal with the mistake argument and then show how the privilege problem naturally arises from a gap in that answer. For simplicity's sake, I'll focus on the case of "+".
The dispositionalists' standard approach to the mistake argument has two steps. Dispositionalists start by arguing that there's a kind of dispositions k such that our dispositionsk track addition; then, they put forward a dispositional analysis according to which the meaning of "+" depends only on our dispositionsk. Warren develops this strategy by arguing that the right dispositionsk are our "general dispositions to stably give a certain response in normal conditions".
However, dispositionalists themselves grant that there are kinds of dispositions j such that our dispositionsj don't track addition. Of course, they also assure us that this doesn't matter, since the meaning of "+" depends only on our dispositionsk. But why should we privilege our dispositionsk over all the other dispositions of ours? The privilege problem is precisely the problem of answering this question.
I discuss four strategies to deal with the privilege problem: the answer I'm disposedk to give is privileged because of something we do, the answer I'm disposedk to give is privileged because it's the right answer, the answer I'm disposedk to give is privileged because it's the answer I'd give in ideal conditions, and the answer I'm disposedk to give is privileged because it's the answer I'd give in standard conditions.
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A PRACTICE-ORIENTED LOGICAL PLURALISM

Session 13L

Congress section(s): C1

I conceive logic as a formal presentation of a guide to undertaking a rational practice, a guide which itself is constituted by epistemic norms and their consequences. The norms themselves may be conceived in a non-circular manner with a naturalistic account, and we use Hilary Kornblith's: epistemic norms are "hypothetical imperatives" informed by instrumental desires "in a cognitive system that is effective at getting at the truth" ([1]). What I mean by "formal" is primarily what John MacFarlane refers to in his PhD thesis [2] as the view that logic "is indifferent to the particular identities of objects", taken together with MacFarlane's intrinsic structure principle and my own principle that logic is provided by the norms that constitute a rational practice.
The view that logic is provided by constitutive norms for a rational practice helps us respond to a popular objection to logical pluralism, the collapse argument ([3], chapter 12). Logic here has been misconceived as starting with a given situation and then reasoning about it. Instead we start with our best known practice to suit an epistemic goal, and ask how to formalise this practice.
This view of logic provides a starting point for an account of the normativity of logic: assuming we ought to follow the guide, we ought to accept the logic's consequences. If we cannot, we must either revise either the means of formalisation or some of the epistemic norms that constitute the guide. Revision might be performed either individually or on a social basis, comparable to Novaes' conception in [4]. Mutual understanding of differences emerges from the practice-based principle of interpretive charity: we make the best sense of others when we suppose they are following epistemic norms with maximal epistemic utility with respect to our possible interpretations of what their instrumental desires could be.
One might ask what the use is of logic as a formalisation of good practice rather than good practice in itself. Indeed Teresa Kouri Kissel in [5] takes as a motto that "we ought not to legislate to a proper, functioning, science". Contrary to this, my response is that logic provides evidence for or against our conception of good practice, and can thus outrun our own intuitions of what good practice is. Implementations of intuitionistic logic manifested in proof assistants such as Coq have proved themselves capable of outrunning intuitions of good mathematical practice in the cases of particularly long proofs (see for instance [6]).
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CAN NEUROPSYCHOANALYSIS SAVE THE LIFE OF PSYCHOANALYSIS?

Session 8F

Congress section(s): C5

Neuropsychoanalysis is a new school of thought attempting to bridge neuroscience and psychoanalysis. Its aim is to correlate the psychodynamics of the mind with neurodynamic processes (Fotopoulou, Pfaff & Conway, 2012; Solms & Turnbull, 2002 & 2011). Neuropsychoanalysis suggests that psychoanalysis should be related to neuroscience such that psychoanalytical hypotheses can be put to rigorous test and be confirmed, modified, or falsified. In addition, adherents of neuropsychoanalysis think that the psychoanalytic perspective, with its detailed studies of human experience and subjectivity, can contribute to our knowledge about mind and behavior. However, I will present two problems that indicate that neuropsychoanalysis in its present state is not able to save the life of psychoanalysis. The focus is on Freudian psychoanalysis, but the argument may also apply to other psychodynamic theories.
First, by focusing exclusively on the relation between psychoanalysis and neuroscience, neuropsychoanalysis ignores the psychological level, that is, psychological theories and hypotheses that may be better able than psychoanalysis to explain certain phenomena. In fact, most of the explanations that compete with the psychoanalytic ones come from psychology and not neuroscience. Thus, in order to confirm psychoanalytic hypotheses it is not sufficient to show that they are consistent with neuroscience; they must also be at least as corroborated as the competing psychological hypotheses. I illustrate this point by referring to research on the defense mechanism projection. A tentative conclusion is that psychoanalytic constructs may be superfluous, because it is often sufficient to take into consideration only highly corroborated psychological theories in order to explain the phenomena referred to by psychoanalysis.

Second, neuropsychoanalysis has so far not been very successful in generating new knowledge about mind and behavior. It has mostly been concerned with correlating the psychodynamics of mind, described in psychoanalytic terms, with brain processes. These correlation may show that some Freudian ideas are similar to ideas in neuroscience, but this is not the same as generating new knowledge about mind and behavior. Even though some well-respected scientists, such as Jaak Panksepp, Eric Kandel, Joseph LeDoux, and Antonio Damasio have shown some interest in psychoanalysis and neuropsychoanalysis, they (in common with most scientists in psychology, neuroscience, and psychiatry) very seldom refer to psychoanalytic ideas in their publications. They show a remarkable caution in their display of support and prefer to use concepts prevalent in cognitive neuroscience instead of psychoanalytic concepts (Ramus, 2013). Anyway, the burden of proof is on the psychoanalysts to show that psychoanalysis can contribute to our growing knowledge about mind and behavior, but then they need to address the challenges mentioned above.

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LEARNING SUBJUNCTIVE CONDITIONAL INFORMATION

Session 27J

Congress section(s): A2, A3, B2, C5

If Oswald had not Killed Kennedy, someone else would have. What do we learn when we learn such a subjunctive conditional? To evaluate the conditional, Stalnaker (1968) proposes, we move to the most similar possible world from our actual beliefs where Oswald did not kill Kennedy, and check whether someone else killed Kennedy. Based on Stalnaker's semantics, Günther (2018) has developed a method of how a rational agent learns indicative conditional information. Roughly, an agent learns ‚if A then B' by imaging on the proposition expressed by the corresponding Stalnaker conditional. He goes on to generalize Lewis's (1976) updating method called imaging to Jeffrey imaging. This makes the method applicable to the learning of uncertain conditional information. For example, the method generates the seemingly correct predictions for Van Fraassen's (1981) Judy Benjamin Problem.

To the best of our knowledge, there is no theory for the learning of subjunctive conditional information. Psychologists of reasoning and philosophers alike have almost only tackled the learning of indicative conditionals. (See, for example, Evans and Over (2004), Oaksford and Chater (2007) and Douven (2015).) Here, we aim to extend Günther's method to cover the learning of information as encoded by subjunctive conditionals.

On first sight, Günther's method of learning indicative conditional information seems to be applicable to the learning of subjunctive conditionals. From ‚If Oswald had not killed Kennedy, someone else would have" you learn that the most similar world in which Oswald did not kill Kennedy, is a world in which someone else did. However, it is widely agreed upon that the meaning of this subjunctive is different from its corresponding indicative conditional ‚If Oswald did not kill Kennedy, someone else did". You can reject the former, while you accept the latter.

The pair of Oswald-Kennedy conditionals suggest that we might learn different propositions. More specifically, the propositional content of a subjunctive might differ from its corresponding indicative conditional. To account for the difference, we aim to amend Stalnaker's possible worlds semantics for conditionals. The idea is that the mood of the conditional may influence which world is judged to be the most similar antecedent world. In the case of indicative conditionals, the world we move to is just the most similar antecedent world to the actual world. In the case of subjunctive conditionals, the world we move to is the most similar antecedent world to the actual world as it has been immediately before the time the antecedent refers to. The evaluation of subjunctives thus involves mental time travel, while the evaluation of indicatives does not. (The idea to fix the past up until the time to which the antecedent refers can be traced back to Lewis (1973). We will address the accounts of antecedent reference due to Bennett (1974), Davis (1979) and, more recently, Khoo (2017).) As a consequence, when you move to the most similar antecedent world in the subjunctive case, you are not restricted by the facts of the actual world in-between the reference time of the antecedent and the now. We show how this simple amendment to Stalnaker's semantics allows to extend Günther's method to the learning of subjunctive conditionals.

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EFFECTIVENESS, EXEMPLIFICATION, AND FACTIVITY

Session 11K

Congress section(s): B1

The view that scientific representations bear understanding insofar as they capture certain aspects of the objects being represented has been recently attacked by authors claiming that factivity (veridicality) is neither necessary nor sufficient for understanding. Instead of being true, partially true, or true enough, these authors say, the representations that provide understanding should be effective, i.e. they should lead to "useful scientific outcomes of certain kind" (de Regt & Gijsbers, 2017) or should "exemplify features they share with the facts" (Elgin, 2009). In this paper I'll try to show that effectiveness and exemplification are neither alternatives nor independent complements to factivity insofar as an important aspect of these conditions cannot be construed without referring to a certain kind of truthfulness.

Although Elgin's and de Regt and Gijsbers' non-factive accounts of understanding differ in the details, they share an important common feature: they both stress the link between understanding and inference. Thus, according to De Regt and Gijsbers, the understanding-providing representations allow the understander to draw "correct predictions", and according to Elgin, such representations enable "non-trivial inference" which is "responsive to evidence". If we take this inferential aspect of understanding seriously, we should be ready to address the question what makes the conclusions of the alleged inferences correct. It seems as if there is no alternative to the view that any kind of inference could reliably lead to correct, i.e. true (or true enough) conclusions only if it is based on true (or true enough) premises. Indeed, it can be shown that the examples, which the critics of the factivity of understanding have chosen as demonstrations of non-factive understanding could be successfully analyzed in terms of true enough premises endorsing correct conclusions. Thus the ideal gas model, although based on a fiction (ideal gases do not exist), "exemplifies features that exist", as Elgin herself has noticed. Similarly, the fluid model of electricity, discussed by de Regt and Gijsbers, gets right the directed motion of the electrical current, which is essential for the derivation of Ohm's law and for its practical applications.

To sum up, the non-factivists have done a good job by stressing the inferential aspects of understanding. However, it should be recognized that there is no way to make reliably correct predictions and non-trivial inferences, if the latter are not based on true, partially true, or true enough premises. The understanding-providing scientific representations either contain such premises or serve as "inference tickets" bridging certain true or true enough premises to true or true enough conclusions.

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HOW TO DESCRIBE REALITY OBJECTIVELY: LESSONS FROM EINSTEIN

Session 25A

Congress section(s): C2

I use Einstein's theory of relativity to draw out some lessons about the defining features of an objective description of reality. I argue, in particular, against the idea that an objective description can be a description from the point of view of no-one in particular.

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CAN SET-THEORETIC MEREOLOGY SERVE AS A FOUNDATION OF MATHEMATICS?

Session 23

Congress section(s): 0

Mereology, the study of the relation of part to whole, is often contrasted with set theory and its membership relation, the relation of element to set. Whereas set theory has found comparative success in the foundation of mathematics, since the time of Cantor, Zermelo and Hilbert, mereology is strangely absent. Can a set-theoretic mereology, based upon the set-theoretic inclusion relation $\subseteq$ rather than the element-of relation $\in$, serve as a foundation of mathematics? Can we faithfully interpret arbitrary mathematical structure in terms of the subset relation to the same extent that set theorists have done so in terms of the membership relation? At bottom, the question is: can we get by with merely $\subseteq$ in place of $\in$? Ultimately, I shall identify grounds supporting generally negative answers to these questions, concluding that set-theoretic mereology by itself cannot serve adequately as a foundational theory.

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ON VOPĚNKA'S ULTRAFINITISM

Session 4K

Congress section(s): A4

It is perhaps unsurprising that the somewhat remote research area of ultrafinitism in the foundations of mathematics cautiously embraces the work of Petr Vopěnka on his Alternative Set Theory (AST) as a relevent effort. Membership in the "ultrafinitist club" is largely self-proclaimed, so one becomes an ultrafinitist by identifying as such, and Vopěnka's writings present convincing membership evidence, although he might have shied away from the label. In his own words, in designing the AST he attempted to put forward a theory of natural infinity (in which task a sizeable group of coworkers was indispensable; a seminar on the AST was run in Prague for several years in the late 1970s).
Nowadays, the AST might be viewed as a toy alternative attempt at foundations of mathematics, undertaken within classical logic. The usual shortcut to getting a first idea of AST points out a nonstandard model of V_ω, with the natural numbers in the model interpreting the natural numbers of the AST, while its finite natural numbers are interpreted with the standard numbers of the model. This interpretation is in keeping with Vopěnka quoting Robinson's nonstandard analysis as a source of inspiration. Even on an initial acquaintance, AST is seen consistent relative to ZFC.
On reading further, within Vopěnka's texts one will encounter passages that subscribe directly to ultrafinitism through denying (not the existence, but) a finite status to very large natural numbers (denoted by numerals), while admitting that such considerations are classically inconsistent. Such remarks are uttered with the explanation that a reasoner in AST is not necessarily convinced by exceedingly long proofs. These read as the hallmark of mistrust of the ease with which classical mathematics embraces various notational and proof-theoretic shortcuts that divert attention from the actual proof and representation size.
Ours is a work in progress, with several aims. The first one is a reconstruction, insofar as possible, of the ultrafinitist facet of Vopěnka's work in the context of other works with the same flavour (i.a., [3,4,6]). Another aim is to highlight the usefulness of the metaphor of the dichotomy of the standard vs. nonstandard natural numbers (which result from the usual interpretation of AST) to the dichotomy of the feasible vs. unfeasible ones (which can be viewed as a prime would-be applicaition thereof). Moreover, we shall discuss the point, articulated beautifully in [2], of it being the proper logical setting that ultrafinitism is still missing, e.g. when compared to intuitionism. Although one can only guess what the theory of witnessed universes might have been, were it developed, one can assume that, along with other such theories, the logic might depart from the usual, at least in rendering voluminous (i.e., in Vopěnka's terminology, "infinite") proofs irrelevant.
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PHILOSOPHIZING ON BIG DATA, DATA SCIENCE, AND AI

Session 27A

Congress section(s): C6

To whom do the concept of "Big Data" belongs? Is Big Data a scientific discipline, does it refer to datasets of a certain size? Or does Big Data best refer to a collection of information technologies, or is it a revolution within modern businesses? Certainly, "Big Data" is a buzzword used by many different people, businesses, and organizations to mean many different things.
Similar considerations can be made about the concepts of "Data Science" and "AI". Within academia, Data Science has, on several occasions, been used to refer to a "new" science that mix statistics and computer science. Another use of the term is as what "Data Scientists" (mainly in industry) are doing. (Which might differ.) Likewise, the term "AI" has been used to refer to the study of Artificial Intelligence as a scientific discipline. However, AI is also the new buzzword within industry. Though, here AI might better be translated to "Automated Intelligence". Within industry, AI is essentially the same as what "Big Data" used to refer to, however, the focus has moved towards how models can be embedded in applications that automatically makes decisions instead of just a focus on deriving insights from data.
Why are the different usages of these concepts relevant? On the one hand, if we want our science and philosophy to matter and have relevance beyond academia, it does matter how the concepts are used outside academia in the mainstream public and business world. On the other hand, there is, a much stronger sense in which the usage and different meanings of the concepts matter. It mattes to our philosophy. For instance, if we want "Philosophy of Big Data" to be about the ethics of automatic profiling and fraud detection used in welfare, health and insurance decisions, the dataset sizes and information technologies used do not really matter. Instead, it is how data about individuals is collected and shared, how biases in data

transfer to biases in machine learning model predictions, how predictive models are embedded in services and application, and how these technologies are implemented in private and public organizations. Furthermore, if by "Philosophy of Big Data" we are interested in the epistemological consequences of Big Data, it is again other aspects that are central.
In this talk I will therefore argue for the abandonment of usage of terms like "Philosophy of Big Data", "Philosophy of Data Science", "Philosophy of AI", etc. Instead I suggest that we, as philosophers paint a much more nuanced picture of a wide family of related concepts and technologies related to Big Data, Data Science, AI and their cousins such as "Cognitive Computing", "Robotics", "Digitalization", and "IoT".

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THE PHILOSOPHICAL ROOTS OF SCIENCE DENIALISM

Session 5B

Congress section(s): B1, B5

Philosophy is often perceived as an unworldly discipline with no other practical consequences than the positive consequences of clarity of thought. This does not seem to be quite true. Some of the most important thought patterns of science denialism are based on the methodology of radical doubt that was developed in philosophical scepticism. Furthermore, science denialism depends on science exceptionalism, i.e. the idea that the epistemological foundations of science are different from those of our other forms of knowledge. Science exceptionalism has been an implicit assumption in much of philosophy of science. Based on an analysis of the philosophical roots of science denialism, we will discuss what philosophers can and should do to defend science against the current onslaught of science denialism and other forms of pseudoscience.

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WHAT IS "BIOLOGICAL" ABOUT BIOLOGICALLY-INSPIRED COMPUTATIONAL MODELS IN COGNITIVE SCIENCE? IMPLICATIONS FOR THE MULTIPLE REALISATION DEBATE

Session 24F

Congress section(s): C5

In this talk, I investigate the use of biologically-inspired computational models in cognitive science and their implications for the multiple realisation debate in philosophy of mind. Multiple realisation is when the same state or process can be realised in different ways. For example, flight is a potential multiply realised process. Birds, planes and helicopters all fly relying on the same aerodynamic principles but their mechanisms for flying differ substantially: birds have two wings which they flap in order to achieve flight, planes also have two wings, but they are static rather than flapping and helicopters use rotors on the top to produce enough lift for flight. If these "ways" of flying are considered sufficiently different, then we can conclude that flight is a multiply realised process. Philosophers of mind (such as Putnam (1967) and Fodor (1974) but more recently Polger & Shapiro (2016)) have frequently taken multiple realisation to be significant for metaphysical debates about whether mental processes can be reduced to neural processes. The idea being that if mental processes such as pain are multiple realised, then pain does not reduce to a neural process since it can be instantiated in other ways.
The current literature on multiple realisation (for example, Polger and Shapiro (2016) and Aizawa (2018a; 2018b)) doesn't consider how artificial and engineered systems such as biologically-inspired computational models fit into this debate. I argue that the use of these models in cognitive science motivates the need for a new kind of multiple realisation, which I call 'engineered multiple realisation' (or EMR). By this, I mean that scientists aim to create multiple realisations of cognitive capacities (such as object recognition) through engineering systems. I describe various examples of this in cognitive science and explain how these models incorporate biological elements in different ways. Given this, I claim that EMR cannot bear on debates about the nature of mental processes. Instead, I argue that, when building computational models as EMRs, there are different payoffs for incorporating biology into the models. For example, researchers are often motivated to incorporate biological elements into their models in the hope that doing so will lead to better performance of their models. (Baldassarre et al (2017); George (2017); Laszlo & Armstrong, (2013)) Other researchers incorporate biological elements into models as a way to test hypotheses about the mechanisms underlying human vision. (Tarr & Aminoff, 2016) I emphasise that these payoffs depend on the goals of different modelling approaches and what the approaches take to be biologically relevant for these goals. By sketching out different approaches and their notions of biological relevance, I conclude that there are many important roles that EMR can play instead of informing traditional metaphysical debates about the reduction of mental to neural processes.
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CARNAP ON THE REALITY OF ATOMS

Session 19G

Congress section(s): B1, B4

In Empiricism, Semantics, and Ontology (1950), Carnap introduces a distinction between what he calls "internal" and "external" questions. The internal questions for Carnap are relatively straightforward since they arise within a language and are amenable to our ordinary methods of proof. In contrast, external questions are interpreted as practical questions that ask whether we should adopt a certain language based on its expected benefits. While Carnap had originally made this distinction to avoid metaphysical worries that the use of semantics posed to empiricists philosophers (1950), he later extended the application of the distinction to speak about theoretical entities as well (1966/1974). However, a straightforward application of the distinction to the Realism/Anti-realism controversy may be more problematic than what Carnap may think.
In recent scholarship, Penelope Maddy, made an objection to Carnap's extended use of the distinction using the example of the atomic hypothesis and argued that not only the internal/external distinction was unsuccessful for talking about atoms, but that it should be dismissed altogether (2008). According to her criticism, Carnap's distinction would make the reality of atoms a mere external question of adopting an "atom-language" for practical merits. This would undermine the remarkable significance of Perrin-Einstein experiments which decisively proved the existence of atoms. With the refinement of our ordinary methods of evidence based on Brownian motion, Perrin and Einstein settled the seemingly intractable debate between energeticists and atomists in favour of the latter. For Maddy, our acceptance of the atomic hypothesis gives us good reasons to dismiss Carnap's distinction as being confused and overly simplistic since it makes the reality of atoms a matter of convenience only and undermines the novel achievement by Einstein and Perrin.

According to William Demopoulos, however, we can develop an understanding of the distinction that does not reduce the atomic hypothesis to a mere linguistic proposal (2011). Moreover, the external debate between the realists and anti-realists could still be understood as a dispute about a preference of language. Both the realist and anti-realist would agree that atoms are real as a matter of fact, but differ in their understanding of the "atom-language" which they adopt to speak about atoms. In this way, the significance of Einstein and Perrin analysis would still be preserved while the difference between realists and anti-realists would be seen in how they understand the truth of theories.
In my talk, I will use Crispin Wright's pluralist account of truth (1992) to propose other semantic ways that realists and anti-realists could differ from each other beyond what Demopoulos has already suggested (2011). Both Wright and Carnap under my interpretation ought to share a common worry: "What is at stake between the realist and anti-realist if both agree the statements of the contested discourse are irreducibly apt for truth and falsity?" I will show that Wright's various criteria of objectivity can be used to allow for a more nuanced and stable understanding of Carnap's internal/external distinction.

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COMPUTERS AND THE KING'S NEW CLOTHES. REMARKS ON TWO OBJECTIONS AGAINST COMPUTER ASSISTED PROOFS IN MATHEMATICS

Session 4F

Congress section(s): C1

Computer-assisted proofs have produced outstanding results in mathematics. Surprisingly, these proofs are not well-received by the mathematical community, proofs ‚done by hand' are preferred over proofs where some deductive steps are executed by a computer. In my talk, I will analyse and question this behaviour. In the first part, building upon examples from the controversy on Appel and Haken's (1989) proof of the four-color conjecture, I will argue that two often made objections against the acceptance of computer-assisted proofs are (1) that computer-assisted proofs are messy and error-prone, and (2) that they do not give us insight into why a theorem is true. While both objections may be correct descriptions regarding certain proofs, proponents of computer-computer assisted proofs have to defend stronger versions by claiming that, based on objection (1) or (2), computer-computer-assisted proofs should not be accepted as valid in mathematics. In the second part I will argue that the first objection may hold with regard to Appel and Haken's initial proof, but not with regard to later improvements: Gonthier (2008) puts forward a fully formalized proof which is entirely executed by a standardized open-source theorem-proofer; this proof is neither messy nor error-prone. Dealing with the second objection, I will argue that it presupposes that a proof's ‚insight' could be judged in an (at least) intersubjectively stable way. This assumption can be questioned on the basis of recent empirical studies of mathematicians' proof appraisals by Inglis and Aberdein (2014, 2016) who were able to show that proof appraisals involve four different dimensions: aesthetic, intricacy, precision, and utility. If a quality Q can be represented as a linear combination of these four dimensions, mathematicians can be observed to disagree in their judgement of the same proof with regard to Q. Then, I will argue, based on data from the same survey, that the concept of ‚insight' presupposed by the second objection can be represented as a linear combination these factors, namely the aesthetic and the utility dimension and, thus, mathematicians judgements of a proof's insight are subjective. This constitutes a major problem for the second objection: Proponents have to explain these subjective judgements and, in addition to that, also have to provide a new concept of insight that is close to the mathematical practice but which does not give rise to such subjective judgements.
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INDUCTIVE METHOD, OR THE EXPERIMENTAL PHILOSOPHY OF THE ROYAL SOCIETY

Session 24G

Congress section(s): B1, B3, B4, B6

It is widely assumed by students of scientific method that a method of induction is logically impossible. The method of hypothesis seems therefore to be the only possible method open to science. Under the influence of Henri Poincaré, this makes certain theoretical truths conventional, and under the influence of Pierre Duhem physical theory seems always underdetermined by experimental facts, for logical reasons. Karl Popper and Willard Quine have also provided interesting variations on this influential analysis, extending it to all knowledge claims. Even those others who have supported induction in science have supported it in a supplementary role for evaluating hypotheses in a generally hypothetico-deductive scheme. I suggest that these opinions are mistaken.
There is a method. in a new 17th century form of induction. or of an experimental philosophy, that was proposed by Francis Bacon and adopted by the Royal Society, which is not undercut by these logical considerations. The key to this method of modern science was that it relied on recording erroneous judgments rather than on error-free judgement as its basis for performing inductions. It uses skeptical techniques to assemble erroneous judgments into "natural histories," under Francis Bacon's description, producing records of a "cross examination of nature." They yield, by a clever use of experiment, clusters of mutually inconsistent answers concerning what is apparently true. A collection of such experimental anomalies, when it is extensive enough, makes for a puzzle that admits of at most a unique solution. We may regard such unique solutions, when they are found by a result of "error analysis," as real knowledge to the extent that nothing else can fit the facts. The facts, of course, are the factual errors that are recorded in the Baconian or experimental natural history as apparently true. Because solutions are unique to each puzzle, they cannot count merely as hypotheses, precisely because nothing else can fit the facts. Unlike hypothetico-deductive analysis, inductive error analysis admits of no alternative to the solved puzzle. The new method of modern science consists in writing up the experimental record carefully. Each experimental phenomenon must be recorded with a detailed account of the conditions of its appearance. An inductive solution found by error analysis is a model of the reality underlying puzzling appearances. A carefully written experimental record of the conditions under which erroneous experimental appearances are to be found gives us a new kind of "power over nature," in the following way. When we have hit upon the unique model that works, the model indicates how experimental conditions yield apparent errors. The conditions for production may now be called "causal" conditions, and the errors, their "effects." This is a "pledge," as Bacon says, of truth. What we have is not merely a matter of words, as in the hypothetico-deductive method, but we know that we "have got hold of the thing itself" that we can now manipulate.

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MATHEMATICAL UNDERSTANDING BY THOUGHT EXPERIMENTS

Session 4E

Congress section(s): C1

In the experimental sciences, researchers distinguish between real experiments and thought experiments (TEs), although it is questionable whether any sharp border can be drawn between them in modern physics. The distinction between experiment and TE is also relevant, mutatis mutandis, for certain situations in mathematics. In this paper, I begin by unpacking this thesis in the context of a pragmatic approach to mathematics. I then argue that, from this point of view, the distinction between experience, experiments, TEs and exercises of thinking in mathematics makes it possible that TEs exist in mathematics as, for example, explicative proofs. TEs can therefore advance mathematical understanding without requiring any new mathematical experiments. The proposed solution avoids the classical alternative between Brown's Platonistic and Norton's empiristic solutions to the well-known dilemma of the interpretation of TEs. Finally, I support my thesis with some mathematical examples.

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STRUCTURALIST ABSTRACTION AND GROUP-THEORETIC PRACTICE

Session 3J

Congress section(s): B6, C1

Mathematical structuralism is a family of views holding that mathematics is primarily the study of structures. Besides genuine philosophical reasons to adopt a structuralist philosophy, the "structural turn" of the late 19th and early 20th centuries' mathematics itself can motivate the adoption of structuralist views. For example, the philosophical notion of (structuralist) "abstraction"—a transformation from the concrete/particular/intuitive to the abstract/universal/formal—is often said to be rooted in historical events, such as:
1) F. Klein's Erlangen Program of 1872.
2) The axiomatic definition of groups of 1882.
3) The invention and use of category theory, starting from 1942.
The focus on these particular demonstrations of historical abstraction processes does not exclude, of course, the possibility of alternative interpretations of structuralist abstraction, in philosophy or the history of mathematics. The questions are therefore the following:
What other structuralist abstraction process exist in the history of mathematics? What explanatory power on abstraction principles can they bear?
In its unrestricted form, the question will allow for an enormous amount of possible answers, but one does not have to depart from the original examples as much as it first seems: To each of the above examples, my suggestion is, there exist closely related historical evidences for rather different interpretations of the alleged "structuralist turn" n mathematics. Further, these new examples will all stem from the theory of groups, which will make comparison easier:
1*) F. Klein's "Hypergalois Program": While Klein's aim in the Erlangen Program was to organize several geometries under the unifying approach of algebra, the "Hypergalois Program" promotes means to tackle genuine algebraic problems with geometric means and thus constitutes a forerunner of the theory of representations. This meets Klein's own aspiration not to subsume geometry under algebra, but to combine the tools and methods of both theories.
2*) If the axiomatic definition of a group ignores the internal nature of its elements, why not climbing further on the "ladder of abstraction" and ignore their existence altogether—focusing solely on sub- and superrelations of algebraic structures like groups? Such ideas can be traced back to E. Noether (and arguably to Dedekind), but only in 1935/1936 O. Ore formalized such a "foundation of abstract algebra". His ideas marked the birth of the theory of lattices that brought new insights into abstract algebra, the foundational claim, however, failed and was soon abandoned.
3*) A more successful unification of algebraic theories was laid down in category theory; a theory that was nevertheless invented for much more modest and particular goals. Here, the focus is laid to the question how newly emerging category theory was thought to provide means for the most urging problems of group theory, namely those of homology and cohomology.
A tentative lesson drawn from these new examples could be not to regard structuralist abstraction as an imperative of modern mathematics that is applied for its own sake, but rather as an additional mathematical tool that (together its counterpart of "instantiation") modern mathematicians are free to use to serve their particular purposes.

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THE HISTORY OF SCIENCE AND THE METAPHYSICS OF CHEMISTRY

Session 5A

Congress section(s): B1

Any scientific discipline is shaped by its history, by the people within it and by the cultures within which they work. But it is also shaped by the world it investigates: the things and processes it studies, and the ways in which it studies them. The International Union of Pure and Applied Chemistry (IUPAC) has developed different systems of nomenclature for inorganic and organic substances, based systematically on their structure at the molecular scale. In different ways these systems reflect both chemistry's historical development, and also particular metaphysical views about the reality of chemical substances. Thus, for instance, IUPAC names many inorganic substances on the basis of a system which is the recognisable descendant of the scheme of binomial nomenclature proposed by Antoine Lavoisier and his associates in the 1780s as part of their anti-phlogistic campaign. IUPAC's nomenclature for organic substances is based on a theory of structure that was developed in the 1860s and 1870s to provide an account of various kinds of isomerism. Both of these were reforming developments: attempts to introduce order, clarity and precision into an otherwise chaotic and confused scene, based on a particular foundational conception of the field (or rather sub-field). But order, clarity and precision may come at a cost: by tidying things up in one way, laying bare one set of patterns and structures, chemists might have obscured or even buried other patterns and structures.
Suppose that one is primarily engaged in developing an account of what the world is like, according to chemistry, in the respects in which chemistry studies it. One might start with modern chemistry, studying its currently accepted theories and the implicit assumptions underlying its practices, and think about how the world would be, in the respects in which chemistry studies it, if those theories and assumptions were broadly true. In this kind of project the particular metaphysical views about the reality of chemical substances that underlie modern chemistry are of central interest, as is the story of how modern chemistry came to be the way it is. That story also includes the options not taken up: alternative systems of nomenclature based on different ways of thinking about chemical reality. That story is an indispensable part of understanding, in both historical and epistemic terms, why modern chemistry is the way it is.
How do we know that modern chemistry will present us with a coherent set of metaphysical views about the reality of chemical substances, something that can be regarded as, or can perhaps be shaped into, a metaphysics of chemistry? Of course, we don't. Chemistry and its history might present two kinds of difficulty: metaphysical disunity in modern chemistry, and historical options not taken up, but which demand to be taken seriously. There are different ways to respond to these difficulties, not all of them being pluralist (for disunity may reflect disagreement). And we don't know a priori that modern chemistry cannot be understood on the basis of a coherent metaphysical view of chemical reality.
Any philosopher who wishes to bring their study of science into contact with metaphysics must acknowledge the different forces that have shaped science as we find it, but acknowledging them brings with it the recognition that there are different ways to proceed. If the history of chemistry is a garden of forking paths, then so is the metaphysics of chemistry.

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COMBINING TEMPORAL AND EPISTEMIC LOGIC: A MATTER OF POINTS OF VIEW

Session 6L

Congress section(s): A1, A2, B2

In Temporal-Epistemic Logic (TEL) we need to evaluate sentences regarding both times and epistemic states (or possible worlds). For example, a sentence such as Mary believes/knows that Paul will come back tomorrow must be evaluated regarding a certain state (possible world) w and a certain time point t (now/today). According to the (modal) logical analysis of propositional attitudes, if the sentence above is true, then for any state v compatible with what Mary believes/knows at w, there is a time s ("tomorrow", later than t) such that, in v at s, Paul comes back.
This kind of analysis takes into account both epistemic states and times. So, we need to combine a Modal Epistemic Logic and a Temporal Logic into a more comprehensive logical system, which will precisely enable us to evaluate sentences that contain a temporal and an epistemic dimension, such as the examples above.
When we want to deal with a knowledge representation problem, it may be useful to consider the dynamic aspects of reasoning processes, that is, how knowledge changes over time as well as the different kinds of knowledge an agent can have about past and future. In TEL the first part of that challenge can be achieved in a relatively easy way by adding a temporal dimension to an epistemic system, as it happens in the sentence: Tomorrow Mary will believe that Paul came back (now/today/yesterday).
However, the second part of our goal is much more difficult to achieve, since it implies letting temporal operators occurring in the scope of epistemic ones, as we can see in the following example: Mary believes that Paul will come back tomorrow.

The most notable aspect of TEL systems is that we have to combine two different points of view in them, so that, on one hand, temporal points (instants) are determined from the point of view of an observer placed outside the world, and, on the other hand, the epistemic alternatives of each agent (in each instant) are relative to that agent.
The main goal of this talk is to show that the most important problems we have to face when we try to build a TEL system, can be overcome with the help of so-called Hybrid Logics, as hybrid formal languages include an interesting set of syntactic mechanisms (such as nominals, the satisfaction operator and binders) which enable us to refer to specific time instants, so that we´ll be able to formally express how knowledge (or beliefs) change over time and what kind of knowledge (or beliefs) an agent can hold about past and future.
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HYPERCOMPUTING MINDS: NEW NUMERICAL EVIDENCE

Session 24J

Congress section(s): A3

We report some progress [8] in generating numerical evidence for the hypothesis of hypercomputing minds. There is an ongoing discussion [3] on implications of Goedel's Incompleteness Theorems on Computationalism. Roughly speaking, the thesis of classical Computationalism states that Everyone is a Turing Machine of some finite complexity. In a more formal way, let p denote Persons, m Turing Machines and let cpl(m) measure the complexity of a Turing Machine in terms of states and transitions and let k be an Integer. According to [4] we might state the thesis of Computationalism as:
[C] $\forall p \exists m \exists k$ ($p=m \wedge cpl(m) \leq k$)
Now, recall that there are too many functions f: N→N for them all to be computable. Hence uncomputable functions must exist, [2]. In 1962, Rado, [1], presented the uncomputable function Σ, (aka the Busy Beaver function). Σ(n) is the largest number of 1's left on the tape by a halting binary n-state Turing machine when started on an all 0-tape. The Σ function is uncomputable, because otherwise it would solve the Halting problem, which is known to be undecidable. Hence, no single Turing Machine can compute Σ for all n.
Recently Bringsjord et al. brought forward [4] a New Goedelian Argument for Hypercomputing Minds based on the Σ function. It is their basic assumption that [A] If the human mind can compute Σ (n) it is able to eventually compute Σ (n +1). In [4] a formal argument is presented showing that if [A] holds then [C] cannot hold. We provide further evidence for [A] by applying novel AI based methods to finally compute the Σ function for one further, hitherto unknown argument, namely n = 5. Note that Σ(n) is currently known for n=1,2,3,4 but only lower bounds are in place for n > 4, see e.g. [5,6,7]. Our AI based methods enable us to move closer to prove Σ (5) = 4098, by classifying large sets of 5 state binary Turing Machines using data mining on their tape number sequences. For these sets of TMs, we can identify the recurrence relation R[tpn] of the tape number sequence tpn. Using then R[tpn] we can produce an automated induction proof for TM: R[tpn] -> R[tpn+1], showing that the Turing machine m associated with R[tpn] does NOT halt and hence does not contribute to Σ(5). Together with conventional methods like backtracking, we are coming very close to a complete classification of all 5-state, binary TMs and are thus zooming in on computing Σ(5) = 4098.
We thus provided one additional step in the ladder of computing Σ(n) for increasing values of n, providing numerical evidence for Bringsjord [4] new Goedelian Argument [A] which in turn is another piece of evidence that the Human Mind is operating above the Turing Limit.
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THE DEVELOPMENT OF EPISTEMIC OBJECTS IN MATHEMATICAL PRACTICE: SHAPING THE INFINITE REALM DRIVEN BY ANALOGIES FROM FINITE MATHEMATICS IN THE AREA OF COMBINATORICS.

Session 15F

Congress section(s): C1

We offer a case study of mathematical theory building via analogous reasoning. We analyse the conceptualisation of basic notions of (topological) infinite graph theory, mostly exemplified by the notion of infinite cycles. We show in how far different definitions of "infinite cycle" were evaluated against results from finite graph theory. There were (at least) three competing formalisations of "infinite cycles" focusing on different aspects of finite ones. For instance, we might observe that in a finite cycle every vertex has degree two. If we take this as the essential feature of cycles, we can get to a theory of infinite cycles. A key reason for the rejection of this approach is that some results from finite graph theory do not extend (when we syntactically change "finite graph", "finite cycle", etc. to "infinite graph", "infinite cycle" etc.
The activity to axiomatise a field is no purely mathematical one, which cannot be solved by proof but by philosophical reflection. This might sound trivial but is often neglected due to an over simplified aprioristic picture of mathematical research. We must craft our formal counterparts in mathematics by our intuitions of the abstract concepts/objects. While we normally think of a mathematical argument as the prototype of deductive reasoning, there are inductive elements in at least three senses:
1. In the heuristic of developing.
2. In the process of axiomatisation, while
2a. we test the adequacy of an axiomatisation.
2b. we are looking for new axioms to extend a current axiomatic system.
We want to focus on 2a and especially on the role of analogies. Nash-Williams (1992, p. 1) analysed that "the majority of combinatorialists seem to have concentrated on finite combinatorics, to the extent that it has almost seemed an eccentricity to think that graphs and other combinatorial structures can be either finite or infinite". This observation is still true, but more recently a growing group of combinatorialists work on infinite structures. We want to analyse the heuristics of theory development in this growing area.
There are vocabularies from finite graph theory for which it is not clear which infinite counterpart might be the best concept to work with. And for theorems making use of them it is also unclear whether they can or should have an extension to infinite graphs. This area is very suitable for the philosophical discourse, since the used concepts are quite intuitive and involve only a little background from topology and graph theory.
We argue that mathematical concepts are less fixed and eternal than it might seem. Shifting the focus from the sole discussion of theorems, which is overrepresented in the reflections of philosophers of mathematics, towards the interplay of definitions and theorems. While theorems are a very important (and probably even constitutive) part of the practice of mathematicians, we should not forget that mathematical concepts in the sense of concepts used in mathematical discourses develop over time. We can only proof / state / comprehend with fitting vocabulary, which we develop in a process of iterative refinement.

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TEXT-DRIVEN VARIATION AS A VEHICLE FOR GENERALISATION, ABSTRACTION, PROOFS AND REFUTATIONS: AN EXAMPLE ABOUT TILINGS AND ESCHER WITHIN MATHEMATICAL EDUCATION

Session 16A

Congress section(s): C1

In this talk we want to investigate in how far we can understand (or rationally reconstruct) how mathematical theory building can be analysed on a text-level approach. This is apparently only a first approximation concerning the heuristics actually used in mathematical practice but delivers already useful insights.
As a first model we show how simple syntactical variation of statements can yield to new propositions to study. We shall show in how far this mechanism can be used in mathematical education to develop a more open, i.e. research oriented experience for participating students.
Apparently not all such variations yield to fruitful fields of study and several of them are most likely not even meaningful. We develop a quasi-evolutionary account to explain why this variational approach can help to develop an understanding how new definitions replace older ones and how mathematicians choose axiomatisations and theories to study.
We shall give a case study within the subject of 'tilings'. There we begin with the basic question which regular (convex) polygon can be used to construct a tilling of the plane; a question in principle accessible with high school mathematics. Small variations of this problem quickly lead to new sensible fields of study. For example, allowing the combination of different regular (convex) polygons yields to Archimedean tilings of the plane, or introducing the notion of 'periodicity' paves the way for questions related to so-called Penrose tilings. It is easy to get from a high school problem to open mathematical research by only introducing a few notions and syntactical variations of proposed questions.
Additionally, we shall offer a toy model of the heuristics used in actual mathematical practice by a model of structuring a mathematical question together with variations of its parts on a syntactical level. This first step is accompanied by a semantic check to avoid category mistakes. By a quasi-evolutionary account, the most fruitful questions get studied, which leads to a development of new mathematical concepts.
Depending on whether there is time left, we show that this model can also be applied to newly emerging fields of mathematical research.
This talk is based on work used for enrichment programs for mathematically gifted children and on observations from working mathematicians.

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CAN WE APPLY THE SCIENCE/TECHNOLOGY DISTINCTION TO THE SOCIAL SCIENCES? A BRIEF ANALYSIS OF THE QUESTION

Session 6F

Congress section(s): C7

The interrelationship between society and technology has been a recurring topic of study not only for those commitment to the field of science and technology studies, but also to social scientists (from sociology to management studies). From the research in those fields arise an issue that has not been address properly yet; at least, and as far as I know, in an explicit nor in a deep way. Nor in the disciplines cited before neither in the philosophy of science and technology. That issue is the possibility of talking about "social technologies", in the sense that there are some fields in the realm of social sciences whose knowledge -although is referred to social phenomena- can be better understood if it is considered – epistemologically- as technical knowledge.

I this talk, I address the problem of applying the (philosophical) distinction of science and technology to the disciplines that study (or that deals with) social phenomena. Though this topic has not been studied in-depth yet, there have been a few scholars that have tried to address it, whose arguments I shall discuss.
First, I will expose the science/technology distinction based on Mario Bunge's approach (a framework of that he calls hylorealism). Also, I shall address the general discussion on this topic in the field of philosophy of science and technology.
Second, I will exhibit the arguments of those researchers who consider that it is possible to talk about technological disciplines in the fields that deal with the social world. I shall discuss then the "sociotechnology" (Mario Bunge) and the "social technology" (Olaf Helmer) approaches, besides the works of other scholars that, although they don't address this issue specifically, have considered the possibility of a technological discipline of the social.
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THE BUILDING BLOCKS OF MATTER: THE CHEMICAL ELEMENT IN 18TH AND 19TH -CENTURY VIEWS OF COMPOSITION

Session 6A

Congress section(s): B1

Currently, the IUPAC (1997) holds a double definition of the chemical element. These definitions loosely correspond to Lavoisier's and Mendeleev's respective definitions of the element: whereas Lavoisier (1743-1794) defined the element as a simple body, thus provisionally identifying all indecomposable substances as the chemical elements, Mendeleev (1834-1907) distinguished between elements and simple bodies. He reserved the term 'element' for the invisible material ingredient of matter, detectable only through its atomic weight, and not isolable in itself.
Today, philosophers of chemistry generally agree that two meanings of the term 'element' coexist, and that this leads to confusion. In order to study the nature of the chemical element, philosophers often refer to Lavoisier's and Mendeleev's views as illustrations of the two meanings. Thus, their definitions are analysed individually as well as compared to each other, independently of their historical context. This reinforces the idea that Mendeleev's definition marks a rupture in the history of the chemical element: it is presented as the return to a pre-existing metaphysical view (Scerri 2007, p. 114-116; Ghibaudi et al. 2013, p. 1627) or the establishment of a new concept of element (Bensaude-Vincent 1986, p.12). However, little is known about the evolution of the concept of chemical element during the early 19th century: where did the change in definition between Lavoisier and Mendeleev come from?
The aim of this paper is to historicise the notion of chemical element, and study its development in the context of 18 th and 19th-century chemistry. Based on the works of Chang (2011, 2012), Klein (1994, 2001, 2003), and Siegfried (2002), I will argue that the change in definition does not in itself constitute a rupture in the history of the chemical element; rather, it is part of a broader evolution of chemical practice which connects the two definitions through a continuous transfer of ideas. Indeed, a view very similar to Mendeleev's was already present in early 19th-century chemical atomism. The 'theory of chemical portions', identified by Klein (2001 p. 15-17, 2003 ch. 1), transformed the stoichiometric proportions in which

elements combined into an intrinsic quality of the elements: it “identified invisible portions of chemical elements [...] as carriers of the theoretical combining weights” (Klein 2001, p. 15). This theory in turn overlaps with Daltonian atomism, which constituted the height of ‘compositionism’ (Chang 2011). Compositionism was based on the assumption that chemical composition consisted of a rearrangement of stable building blocks of matter. This view was dominant in the 18th century and played a crucial role in Lavoisier’s chemical revolution (Chang 2011, 2012 p. 37-41, p.135; Siegfried 2002; Klein 1994). Thus, through a historical analysis this paper will identify the continuity between the views of Lavoisier and Mendeleev. In doing so, it will provide an example of how historical thinking can shed a new light on chemical ontology. Perhaps, a better understanding of the historical constitution of the chemical element will show the contingency of the current double definition, and thus help resolve the question of the nature of chemical element today.

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UNDERSTANDING THE CHEMICAL ELEMENT: A REFLECTION ON THE IMPORTANCE OF THE PERIODIC TABLE

Session 27G

Congress section(s): B6, C2

This year we celebrate the 150th anniversary of the periodic table. This classification plays an important part in my PhD research, which studies the evolution of the concept of element during the century leading up to its publication. Seeing as our current understanding of the chemical element is often traced back to Mendeleev’s work, my goal is to understand how his view of this concept could have been influenced by his peers and predecessors. The periodic table links my research to its present-day interests: the tremendous success of this classification accentuates the need for an understanding of the nature of that which is classified in it. When doing historical research, I use today’s periodic table as a tool to understand the chemists‘ observations and reasoning from a modern chemical point of view. Often, I am astonished by how much of the knowledge presented in the table was already available in early 19th-century chemistry, and this has sparked a personal reflection on the collective nature of scientific knowledge. Rather than as a break with his predecessors, Mendeleev’s table could be seen as the matured product of a period of collaborative research on the elements. In my opinion, the collective nature of this discovery only gives us more reason to celebrate it.

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TOWARDS A CHARACTERIZATION OF PSEUDO-FINITENESS

Session 17A

Congress section(s): A1

Methods with ultraproducts of finite structures have been used extensively by model theorists to prove theorems in extremal graph theory and additive combinatorics. In those arguments, they exploit ultralimits of the counting measures of finite structures, turning asymptotic analyses into questions about dimension and measure in an infinite structure.
Looked at in reverse, pseudo-finite structures always have meaningful notions of dimension and measure associated with them, so it seems valuable to characterize pseudo-finiteness itself. The best known existing theorem of this kind is Ax’s characterization of pseudo-finite fields. I will discuss an ongoing project to find a characterization of pseudo-finiteness for countably categorical theories in which algebraic closure is trivial. Our approach to proving such a characterization is, in a sense, the standard one for model theorists, but the details are novel. First, we’d like to identify certain primitive building blocks out of which models of pseudo-finite theories are made. Second, we’ll need to understand the program for actually putting those building blocks together. Our working hypothesis is that pseudo-finite theories are those that are approximable in a certain sense by almost-sure theories (those arising from 0,1-laws for classes of finite structures), which we also speculate are precisely the rank-1-super-simple theories. In a loose sense, randomness seems to take the place combinatorial geometry in the primitive building blocks of this discussion, and the process of assembling those building blocks into a model has a more analytic flavor than one usually sees in model theory. I will discuss the current state of this work and try to point out some of the interesting contrasts between this program and other classification programs we’ve seen.

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INTENSIONALITY, REFERENCE, AND STRATEGIC INFERENCE

Session 25M

Congress section(s): A2

In this paper I focus on the linguistic constructions in which intensional transitive verbs (ITVs) occur. In order to present the complex logical profile of the ITVs, I contrast them with the extensional transitive verbs and I inspect the semantic differences of their inferential behaviour with respect to the existential entailment, to the substitution salva veritate of the coreferential terms and to the semantic specificity. Insofar as the sentences containing ITVs are structurally ambiguous between an intensional, de dicto interpretation, and an extensional, de re interpretation, they threaten the coordination between agents. For this reason, if the speaker uses an ITV in one way and the hearer’s reading of it diverges from the speaker’s intended meaning, both agents have a coordination problem. In this regard, using the mathematical framework of games of partial information, I will show what conditions have to be satisfied in order for a rational speaker and a rational hearer to efficiently communicate with a sentence in which an ITV occurs, and to converge on the right interpretation when the sentence’s surface syntax is probabilistically silent about which of the de re or de dicto meaning the hearer has to choose in order to coordinate with the speaker. Furthermore, I will look at the semantic behaviour of the most emblematic verb of the semantic lexicon, „refers (to)”, through the prism of the semantic features considered definitory for the extensional idiom and I will show that there are strong theoretical reasons for considering that it belongs to the class of ITVs and that it generates structural ambiguity at each level of language in which it is involved. In this regard, I show how the conceptual framework of the games of partial information can successfully accommodate the semantic verb’s inherent ambiguity.
In order to accomplish this task, I present a scenario of strategic communication involving semantic verbs and I model it as a two-agent coordination game. The cognitive dynamics peculiar to the agents’ interaction will be presented on the background of some reasonable assumptions introduced to guarantee the finding of a solution concept corresponding to the game. In this regard, after I let the utility functions be sensitive to the agents’ preferences for more economical expressions, I compute the expected utilities of the strategic profiles, I determine a set of Nash equilibria and I show that the game’s unique solution can be equated with that set’s member which passes the test of Paretian efficiency. The present model highlights the mutually recursive way in which each agent reasons about the other agent’s probabilistic reasoning, and it manages to

integrate the uncertainty involved in the successful communication with the reasoning about reasoning process involved in the language production and interpretation. In the end, I present the picture of the interplay between the speaker's reference and the semantic reference which emerges from the game-theoretical framework adopted here and I will show some of its methodological consequences related to the way in which the key concept of reference has to be theoretically framed.

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EPISTEMOLOGY OF MODALITY WITHOUT METAPHYSICS

Session 12D

Congress section(s): B1, B4

The epistemological status of modalities is one of the central issues of contemporary philosophy of science: by observing the actual world, how can scientists obtain knowledge about what is possible, necessary, contingent, or impossible. It is often thought that a satisfactory answer to this puzzle requires making non-trivial metaphysical commitments, such as grounding modal knowledge on essences or being committed to forms of modal realism. But this seems to put the cart before the horse, for it assumes that in order to know such ordinary modal facts as "it is possible to break a teacup" or such scientific modal facts as "superluminal signaling is impossible", we should first have a clear metaphysical account of the relevant aspects of the world. It seems clear to us that we do have such everyday and scientific knowledge, but less clear that we have any kind of metaphysical knowledge. So, rather than starting with metaphysical questions, we offer a metaphysically neutral account of how modal knowledge is gained that nevertheless gives a satisfactory description of the way modal beliefs are formulated in science and everyday life.

We begin by explicating two metaphysically neutral means for achieving modal knowledge. The first, a priori way is founded on the idea of relative modality. In relative modality, modal claims are defined and evaluated relative to a system. Claims contradicting what is accepted, fixed or implied in a system are impossible within that system. Respectively, claims that can be accepted within the system without contradiction are possible. Necessary claims in a system are such that their negation would cause a contradiction, and so on. The second, a posteriori way is based on the virtually universally accepted Actuality-to-Possibility Principle. Here, what is observed to be or not to be the case in actuality or under manipulations gives us modal knowledge. Often this also requires making ampliative inferences. The knowledge thus gained is fallible, but the same holds for practically all empirical knowledge.

Based on prevalent scientific practice, we then show that there is an important bridge between these two routes to modal knowledge: Usually, what is kept fixed in a given system, especially in scientific investigation, is informed by what is discovered earlier through manipulations. Embedded in scientific modelling, relative modalities in turn suggest places for future manipulations in the world, leading to an iterative process of modal reasoning and the refinement of modal knowledge.

Finally, as a conclusion, we propose that everything there is to know about modalities in science and in everyday life can be accessed through these two ways (or their combination). No additional metaphysical story is needed for the epistemology of modalities – or if such a story is required, then the onus of proof lies on the metaphysician. Ultimately, relative modality can accommodate even metaphysical modal claims. However, they will be seen as claims simply about systems and thus not inevitably about reality. While some metaphysicians might bite the bullet, few have been ready to do so explicitly in the existing literature.

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MAPPING VS. REPRESENTATIONAL ACCOUNTS OF MODELS AND SIMULATIONS

Session 30I

Congress section(s): B1, B2, C3

Philosophers of science often analyse scientific models and simulations as representational tools (Frigg and Nguyen 2017, 2016; Weisberg 2013; Giere 2010; Suárez 2010). These accounts clarify how scientists use and think about models. This however does not mean that scientific models have to be defined in terms of representation. In this paper, I expose arguments against the representational accounts of models and illustrated them with case studies from the Blue Brain Project (BBP) (Markram et al. 2015).

1. Non-represented entities

The BBP simulations aimed at reconstruction of a small portion of connectome in the neocortical tissue of a rat and its dynamical properties. It was observed that the general behaviour of the biological circuit was dependent on the concentrations of extracellular calcium. The justification of the results of this project were based on the assumption that the tissues were simulated correctly. However, the representation of calcium concentrations was not necessary in order to conduct these simulations.

2. Directionality

Even if one accepts Suárez's (2010) directionality argument against isomorphisms in the analysis of scientific representation, it is not clear if it should be applied to models. Subsequently, it is not certain that models should be understood as representational entities.

There are two different reasons to reject the directionality argument based on scientific practice. HBP has several sub-projects in which brains are either modelled and simulated or brains serve as models to develop alternative computational methods and architectures. If these techniques should succeed, the model relation should support the switch in directionality.

The second observation comes from the building of computer systems that are intended to perform simulations of brains. In the construction phase, biological tissues serve as models for the construction of these systems. In the exploration phase, the relation is inverted.

3. Epistemic justification

There is a strong contrast between the levels of justification provided by the unrestricted notion of representation and the notion of isomorphism. Without qualification, anything can represent anything else. If model and simulation relations are analysed in terms of representation, the level of epistemic justification they provide is very low.

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ABŪ AL-BARAKĀT AND HIS 12TH CENTURY LOGIC DIAGRAMS

Session 5M

Congress section(s): A4

Abū al-Barakāt, a Jewish scholar in twelfth century Baghdad, described a radical new way of handling Aristotle's categorical syllogisms, using pictorial diagrams instead of Aristotle's proof theory. In fact his diagrams form a decision procedure for syllogisms. In the West essentially the same discovery was made by Gergonne in the first half of the nineteenth century—though Barakāt's diagrams are also loosely related to the logical diagrams suggested by Leibniz, Euler and Venn. Like Leibniz, though five hundred years earlier than him, Barakāt used horizontal lines instead of circles. But unlike Leibniz, Barakāt's use of diagrams was model-theoretic rather than proof-theoretic; this confirms Barakāt's reputation as one of the most original minds of his time. His diagrams were widely misunderstood and misreported, though fortunately at least one accurate manuscript survives.

Wilfrid Hodges, 'Two early Arabic applications of model-theoretic consequence', Logica Universalis 12 (1-2) (2018) 37-54.

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DR. WATSON: THE IMPENDING AUTOMATION OF DIAGNOSIS AND TREATMENT

Session 15G

Congress section(s): B3, C4, C8, C9

Last year may be remembered as the pivotal point for artificial "deep learning" and medicine. A large number of different labs have used Artificial intelligence (AI) to augment some portion of medical practice, most notably in diagnosis and prognosis. I will first review the recent accomplishments of deep-learning AI in the medical field, including: the landmark work of Esteva et al. (2017) which showed that AI could learn to diagnose skin cancer better than a dermatologist; extensions of similar projects into detecting breast cancer (Liu et al., 2017); Oakden-Rayner et al.'s (2017) work showing AI could create its own ontological categories for patient risk; and through analyzing tumor DNA identify more possible sites for intervention (Wrzeszczynski et al., 2017).

I will next argue that a forseeable progression of this technology is to begin automating treatment decisions. Whether this development is positive or negative depends on the specific details of who develops this technology and how it is used. I will not attempt to predict the future, but I will run out some emerging trends to their logical conclusions and identify some possible pitfalls of the gradual elimination of human judgment from medical practice.

In particular some problems could become significantly worse. It is the essence of deep learning AI that reasons for its outcomes are opaque. Many researchers have shown that industry has been adept at causing confusion by advancing alternative narratives (e.g. Oreskes and Conway, 2010), but at the very least with traditional research there were assumptions that could, in principle, be assessed. With this deep learning AI there are no such luxuries. On the other hand, I will argue that properly implemented deep learning solves a number of pernicious problems with both the technical and the social hindrances to reliable medical judgments (e.g. the end to a necessary reliance on industry data). Given the multiple possible routes that such technology I argue that consideration of how medical AI should develop is an issue that will not wait and thus demands immediate critical attention of philosophy of science in practice.

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FREE WILL AND THE ABILITY TO CHANGE LAWS OF NATURE

Session 10K

Congress section(s): B4

Peter van Inwagen argued that we are never able to change laws of nature, i.e., the laws of physics (van Inwagen, 1983, pp. 61–62). According to him, while we have the power to make some true propositions false, we do not have any power to falsify laws of nature. That is, in a case where it seems that we can change the truth-value of a law of nature, that proposition is not a real law of nature. The idea that we can never have the ability to change laws of nature plays an important role in the consequence argument, which aims to show that free will as the ability to do otherwise is incompatible with determinism. One premise of this argument is the sentence NL, which roughly means that the conjunction of all laws of nature holds and that no one has, or ever had, any choice about whether this conjunction holds (cf. van Inwagen, 1983, p. 93). Thus, whether or not we can change the laws of nature has been the important topic in the philosophical discussion about free will.

Some leeway compatibilists, who believe that free will as the ability to do otherwise is compatible with determinism, oppose this premise. Bernard Berofsky is one such compatibilist and claims that since psychological laws cannot be reduced to or supervene upon physical laws, the premise that we can't change the laws of nature is false (Berofsky, 2012). We have a power to disobey psychological laws relating to rational decision-making and to obey another rational strategy. We can choose laws of rationality in order to handle the imprecise information and the temporal limits of calculating skills (Berofsky, 2012, pp. 119–121). Therefore, we have free will not only as rational autonomy but also as the ability to do otherwise even in a deterministic world.

By focusing on supervenience relation between physical properties and psychological properties, I argue that Berofsky's argument is insufficient to defend the idea that physical determinism is not an enemy of free will as an ability to do otherwise. First, I explain van Inwagen's argument against the ability to change laws of nature and the basic formation of the consequence argument. Second, I examine Berofsky's expanded consequence argument and his argument showing that psychological laws do not supervene upon physical laws. Third, I argue for the thesis that psychological properties supervene on physical properties and show that this supervenience relation is still a threat to the compatibility of free will as the ability to do otherwise with determinism. And finally, I address Berofsky's and other possible counterarguments.

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THE INDESTRUCTIBILITY OF THE TREE PROPERTY

Session 24B

Congress section(s): A1

Abstract. Let λ be an uncountable regular cardinal. We say that the *tree property* holds at λ, and write (λ), if every λ-tree has a cofinal branch (equivalently, there are no λ-Aronszajn trees). Recently, there has been extensive research which studies the extent of the tree property at multiple cardinals, with the ultimate goal of checking whether it is consistent that the tree property can hold at every regular cardinal greater than $\aleph_1$. The method of proof is typically based on lifting a certain elementary embedding with critical point λ which witnesses the strength of λ and on application of criteria for a forcing notion adding or not adding new cofinal branches to *existing* λ-Aronszajn trees. With such criteria in place, an iteration can be defined which in effect kills all potential λ-Aronszajn trees, and thus forces the tree property. In this talk we study a related question and search for criteria for forcing notions adding or not adding *new* λ-Aronszjan trees (we will always consider forcings which preserve λ as a regular cardinal to avoid trivialities). To give our study more specific focus, we will focus on cardinals of the form $\lambda = \kappa^{++}$ where $\omega \leq \kappa$ is a regular cardinal. We aim to identify a class ("I" for "indestructible") of forcing notions which is as inclusive as possible and which consists of forcing notions which – at least consistently over some model V^* – do not add new κ^{++}-Aronszajn trees. Since typically (κ^{++}) will hold in V^*, we will say that (κ^{++}) is *indestructible in V^* with respect to forcing notions in* . We will consider "Mitchell-style" models V^* which seem to provide the greatest level of generality for a description of a typical "Mitchell-style" model). After discussing the extent of , reviewing the construction of the model and comparing to existing models, we will suggest applications for manipulating the generalized cardinal invariants using the presented method.

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REAL CLIMATE POSSIBILITIES: PROXIMATE VS. REMOTE

Session 8B

Congress section(s): C2

Although many risks associated with climate change can be predicted with high levels of confidence, some predictions are made in conditions of 'deep uncertainty' – i.e. in the absence of precise probabilities. For instance, while many scientists anticipate that global warming might have runaway effects, there tends to be uncertainty about its pace and about tipping points in the climate system. Additionally, economic models which calculate the long-term costs and benefits of climate action face major uncertainty about variables such as future human welfare, our future ability to adapt to changing climates, and our future potential to geo-engineer the climate. As a result, to invoke a distinction common in the literature, in anticipating climate futures we often do not find ourselves in a context of 'risk' (with known probabilities), but in a context of 'uncertainty' (absent precise probabilities; sometimes referred to as 'deep' or 'Knightian' uncertainty).
In response to this epistemic predicament, some scholars have urged to refine our theoretical tools for assessing future possibilities, even when evidential probabilities are vague, unreliable or unknown (e.g. Betz 2010; Fox Keller 2015; Hansson 2016). One strategy for doing so – common in climate science – is to outline so-called scenarios about the future (Challiner at al. 2018). A drawback of possibilistic approaches, however, is that they tend to overreach, addressing many 'mere possibilities' that will never be actualized (Betz 2015). To serve as a useful theoretical tool, a minimal condition for what if-scenarios about climate futures is that they depict 'real possibilities' (Spangenberg 2018).
A similar call for focusing on 'real possibilities' has recently been advanced in discussions of the precautionary principle (e.g. Gardiner 2006). To serve as a plausible guide for decision-making in uncertainty, this principle should be restricted to what appear to be 'real possibilities': precautions should only be taken to avert bad outcomes that, to the best of our knowledge, might otherwise be actualized. Hence, a better epistemic grasp of 'real possibilities' is also relevant for ethical decisionmaking about climate change.

In this presentation I discuss early work which explores whether further advances can be made by differentiating between 'real possibilities' of different kinds. For instance, Katvaz et al. (2012) and Katvaz (2014) have proposed that we can rank different possibilities in terms of how 'remote' they are. But how can we assess the remoteness of different possibilities, absent appeal to evidential probabilities? Can such a ranking be made on purely possibilistic grounds? Or do all epistemic appeals to 'real possibilities' ultimately come down to probabilistic claims, as Roser (2017) suggests?

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DO ABSTRACT ECONOMIC MODELS EXPLAIN?

Session 18F

Congress section(s): C7

At least in the last two decades, the problem of the putative explanatory force of abstract economic models has been framed as follows. How can it be that these highly idealized, even contrafactual, thus literally false models can be explanatory of the real world? The explanatory bottleneck was thus taken to consist in the gap between models and real world (Sudgen 2000). Reiss 2012 even postulated an "explanation paradox" due to the literal falseness of the models.
In this paper, I shall claim that the really fundamental explanatory bottleneck of explanations by abstract models has been misidentified. Even if the transfer from abstract models to the real world were completely unproblematic, a model would not explain by itself some real world state.
Let us assume that we can model the state to be explained in the model world. For instance, if the explanandum is some kind of segregation, in a checkerboard model like Schelling's the explanandum would be represented by some clustering of the white and the black chips, a "model-segregation state". A suitable model like Schelling's consists of a model dynamics that somehow represents a possible real dynamics, for instance by "model-weak-racial-preferences". The model is then capable of generating the model-segregation state out of an initial "model-non-segregation state", i.e., a random distribution of white and black chips. Is this generation of the final model-segregation-state out of the initial model-non-segregation-state an explanation of the former in the model world? This is clearly not the case because we can easily build models with different dynamics also leading to model-segregation as final states, e.g., models with model-strong-racial-preferences, or model-apartheid-laws, or model-race-related-income-differences, etc. Thus, if in the model world a final state with model-segregation is given, every particular model producing that final state only delivers a potential explanation for the emergence of model-segregation out of model-non-segregation. This result is quite general: models never produce actual explanations in the respective model world but only potential explanations. This is because alternative models may lead to the same final state.
To see how a potential model world explanation transforms into the real world, we continue to assume that the correspondences between model states and model dynamics on the one hand and real world states and real world dynamics on the other is unproblematic. Does the Schelling model with model-weak-racial-preferences then explain real world segregation states? Of course not, because already in the model world it produces only a potential explanation, so in the real world it also produces (at best) a potential explanation. In order to transform a potential explanation into an actual one, one has to exclude all alternative potential explanations. It is fundamental to note that the actual explanation is distinguished from other potential explanations primarily not by an intrinsic property such as high credibility (as Sugden assumes), but by its comparative advantage against competitors. This may be accomplished by showing that the empirical conditions necessary for alternative models to work do not obtain.

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GOOD SINGULARITIES, BAD SINGULARITIES

Session 19I

Congress section(s): C2

Classical electrodynamics, which introduces the existence of particles and fields, has singularities. But not all singularities are created equal: some are good, some are bad, and some are really bad. The singularities in the Maxwell–Lorentz theory of classical electrodynamics are either bad or really bad. Both kinds of singularities manifest themselves in the self-interaction problem: one is coming from the near field and the other from the radiation field. Either singularity needs to be treated differently in order to yield equations of motion for charges. Dirac showed how to do that: while the far field singularity (a bad singularity) can be dealt with by Taylor approximations, the near field singularity is more severe and needs a mass renormalization procedure, which introduces an infinite electrodynamic mass.
In order to have a well-defined theory that doesn't rely on approximations, Born and Infeld developed in the 1930s a classical electromagnetic theory with a dual ontology of particles and fields that doesn't suffer from the self-interaction problem. But still this theory keeps a singularity for the electromagnetic field at the location of the charges. This singularity is a good one, though, because it (surprisingly) leads to well-defined equations of motion and to a finite self-energy. Since the Born-Infeld field theories are non-linear, it is very difficult to rigorously solve the equations of motion. Therefore, very little is actually known about this theory.
In the 1940s, Bopp, Podolsky, Landé, and Thomas, proposed an alternative theory of charges and electromagnetic fields that is linear, although of higher-order than the standard Maxwell equations. This theory also has singularities, but they are of a mild kind that lead again to well-defined self-interactions and to finite self-energies. Although the interaction between charges and the electromagnetic field poses mathematical and physical challenges, the Born–Infeld and the Bopp–Podolsky theory are promising, unfortunately rather unknown, proposals on how to construct a consistent classical theory.

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TOOL-DRIVEN SCIENCE

Session 26K

Congress section(s): B1, B3, C3, C4, C9

Research on the human microbiome has generated a staggering amount of sequence data, revealing variation in microbial diversity at the community, species, and genomic levels. In order to make this complexity more manageable and easier to interpret, new units—the metagenome, core microbiome, and enterotype—have been introduced in the scientific literature (Arumugam et al. 2011). Here, I argue that analytical tools and exploratory statistical methods, coupled with a translational imperative, are the primary drivers of this new ontology. By reducing the dimensionality of variation in the human microbiome, these new units render it more tractable and easier to interpret, and hence serve an important heuristic role. Nonetheless, there are several reasons to be cautious about these new categories prematurely "hardening" into natural units: a lack of constraints on what can be sequenced metagenomically, freedom of choice in taxonomic level in defining a "core microbiome," typological framing of some of the concepts, and possible reification of statistical constructs (Huss 2014). Slight differences in seemingly innocuous methodological decisions, such as which distance metric or which taxonomic rank to use, can result in radically different outcomes (Koren et al. 2013). In addition, the existence of tools to study microbiota at the community level through metagenomic and other "holistic" approaches has led to a presumption that causal explanation is also to be found at the community level (O'Malley and Skillings 2018). The general phenomenon is that within microbiome research, and perhaps more generally, the tools of investigation leave an imprint on the resulting knowledge products that can be mistaken as stemming from the features of the natural system under study (Juengst and Huss 2009). As a partial corrective to this, I argue for a return to the robustness analysis of William Wimsatt (1981) in which failures of robustness are used to localize the factors that natural invariances depend upon. A further methodological improvement would be a return to hypothesis-driven research.

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DISPUTING UNCONSCIOUS PHENOMENALITY

Session 5F

Congress section(s): C5

Several influential accounts of unconscious mental states portray them as direct replicas of their conscious counterparts, minus the consciousness. Rosenthal (1999) famously argues that unconscious states possess "the very same" properties as conscious ones. Coleman (2015), in his protopanpsychism, uses the similar argumentative strategy: the qualitative character of a mental state is already fully present at the unconscious level and it only differs from the conscious level in its (un) availability to the subject. These claims follow directly from an assumption about consciousness, shared by both authors. On their account, consciousness results from a single operation that makes content available to the subject, either by operation of a higher-order thought or an 'awareness' procedure. These operations serve a sole purpose of making the content subjectively accessible, so it has to be fully there, prior to its uptake into consciousness.
Recently, Marvan and Polák (2017) came with few novel arguments that support what they call a 'dual' view, i.e. the claim that phenomenal character of mental states is present independently of its conscious status.
I argue that arguments for unconscious phenomenality perfectly matching conscious phenomenality, are unpersuasive. I start from an observation that most of the examples used are perceptual and they tend to come from a single modality. While it might be the case that a perception of a vividly red patch possesses all of its phenomenal qualities prior to its appearance in consciousness, the likelihood decreases with increasing complexity of phenomenal states. Examples of multimodal and category perception present an especially challenging area for the defenders of the dual view. Well documented cases of multisensory integration, such as McGurk effect, parchment skin illusion or spatial ventriloquism, illustrate my point. In all of them, the resulting subjectively perceived state is not a sum of components from different modalities, but a newly emergent quality that does not correspond to a sum of qualities that were processed along various single modality pathways. Similarly, category perception, especially of the kind that crosses various modalities (such as seeing something as edible or hearing something as dangerous) indicate complex processing that is unlikely to appear complete already at the unconscious level.
While the argument is not conclusive, it places the burden of proof on any defender of the 'dual' theory to show that instead of assuming a phenomenal identity in both conscious and unconscious cases, one needs to establish it for all the diverse cases. If it turns out that the 'dual' view cannot meet this challenge, alternative theories of unconscious states are needed.

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METAPHYSICS AND PHYSICS OF CONSCIOUSNESS AS A PROBLEM OF MODERN SCIENCE

Session 18H

Congress section(s): B4

The synthesis of physics and metaphysics is possible at the approach to consciousness as to a certain cut (to level, a layer) of an information reality.
A special question — the ontological status of the information. The majority of researchers consider it in the spirit of K. Shennon as a system of distinctions and relations in abstract logic space. In this sense the information is everywhere where «there is a casual and remembered choice of one variant from several possible and equal in rights» (G. Kastler). N. Winer asserted that «the information is information, not a matter and not energy».
It is necessary to recognize that nobody has yet found out the information in the pure state, without the material carrier, though all agree that the sense and value of the information do not depend on the character of the carrier. But it is real now as science knows only physical fields and matter-energy interactions, instead of «information threads», «information codes», «information fields», etc.
In numerous interpretations of quantum mechanics scientists also involve the concept of consciousness for an explanation of corpuscular-wave dualism and a reduction («collapse») of wave function (so-called «many-worlds» interpretation of quantum mechanics» with G. Everett, J. Willer and De Vitt). Can we indicate that consciousness is an absolutely fundamental property of physical reality, one that needs to be brought in at the very most basic level? They have appealed especially to the role of the observer in the collapse of the wave function, i.e., the collapse of quantum reality from a superposition of possible states to a single definite state when a measurement is made. Such models embrace a form of quasi-idealism, in which the very existence of physical reality depends upon its being consciously observed.
The informative cognitive events are traditionally represented in the form of modules of consciousness — thinking, sensuality, memory and will. These events have correlation communications with physical fields, cellules of brain and all cellular structures of an organism - society - and if to consider a physical principle of the Mach, with the Universe as a whole. Each mental event as a unique subjective experience is information-synergetic unity which is reflected «condensed» in speech. Using physics language it is possible to assume that through «speaking» and «writing» there occurs a reduction («collapse») mental event to its concrete material fixing (analogy to measuring procedures in the quantum mechanics).
For deepening of the philosophical analysis of a problem of consciousness and to bridge the psycho-physical gap, it is necessary to comprehend the specific essence of the information as the primary, initial reality, uniting the matter-energy (substance-power) carrier and the ideal-semantic reality, and to try to express it, using a mathematical apparatus of theoretical physics.
It is important to underline that metaphysical principles for such trains of thought of all the above considered theories of physicists are: 1. Representation about an initial fundamental reality as an information field, existing and developing under certain programs (by analogy to the already well-known Internet); 2. Representation of consciousness as the higher and necessary stage of evolution of life realizing thus through mathematical structures the possibility of direct communication with an information field of the Universe.

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EVEN LOGICAL TRUTHS ARE FALSIFIABLE.

Session 14M

Congress section(s): A2

A special group of sentences, namely logical true sentences like the Law of Excluded Middle or the Law of non-Contradiction, are interesting for most philosophers because of – among other things – their infallibility. Moreover, it seems that their truth value is so obvious that it is not necessary to justify them. These properties lead some philosophers to use them as trustworthy sources to construct philosophical theories or even as direct justifications of philosophical theories. But are they really infallible or are they really self-evident?
In this paper, I want to answer both of these questions with no. For the infallibility-part, I will argue that just in the case that a sentence is analytic, necessary or a priori, it makes sense to speak about its infallibility. In other words, if a sentence is neither analytic, nor necessary, nor a priori, then it is not infallible. With some examples, I will show that a logical true sentence like the Law of Excluded Middle -- as we use it in philosophy -- has none of these properties and therefore is not infallible.
In the second part – the justifiability-part – I will argue that there is a direct connection between sentences in need of justification and falsifiable sentences. Since logical truths are neither analytic, nor necessary, nor a priori sentences and therefore not falsifiable, they are not exempt from justifications either. In other words, their truth value is not always assessable, is context dependent, and often cannot be determined by rational and/or transcendental methods alone. Thus, logical truths need justification.

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KUHN’S WIDE-RANGING INFLUENCE ON THE SOCIAL SCIENCES, LITERARY THEORY, AND THE POLITICS OF INTERPRETATION

Session 9E

Congress section(s): B6

The influence of Thomas Kuhn’s views about science outside HPS and philosophy in general is enormous. It ranges from natural and social sciences to legal studies, the teaching of writing and literary theory.
In this paper we will address only a few aspects of this astonishingly wide influence, focusing on the reception of Kuhn’s views in the social sciences and literary theory. To our knowledge, the latter has not been discussed at all, and the former has been confined for the most part to social scientists’ attempts to make sense of the history and the scientific status of their disciplines in the light of Kuhn’s developmental model of the natural sciences. We argue that Kuhn’s deeper impact was the support he provided for “the interpretive turn” in the social sciences in the 1970s when his views were received by a number of influential social scientists as undermining “the Geistes- und Naturwissenschaften” divide and thus narrowing the gap between “hard” and “soft” sciences.
This “interpretive turn” made literary theory both more relevant to and more interested in the history and philosophy of science. While a number of its practitioners embraced and /or appropriated the key insights of Kuhn’s views to propose that

literary meanings are determined by interpretive paradigms and communities, others used the same insights to characterize both social phenomena and knowledge about them as textual and attempted to bring issues of literary interpretation to bear upon the epistemological issues in the social sciences. Through this double movement of importing Kuhn into literary studies while exporting the problems of textual interpretation to the social sciences, literary theory might be said to have played a particularly significant role in the erosion of strict disciplinary boundaries, which is an important aspect of Kuhn's legacy.

One major consequence of the interaction between the two fields was the politicization of epistemological positions about knowledge, truth and validity in interpretation. Those who embraced the view that there is no one correct interpretation, no ahistorical, paradigm-independent knowledge and truth argued that universalism is repressive while relativism is liberating. We end by considering the tenability and usefulness of this mode of politicization and its relevance today.

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BROAD-SPECTRUM CONCEPTUAL ENGINEERING

Session 17C

Congress section(s): B1, C5

I. TOPIC INTRODUCTION — Conceptual engineering is commonly characterized as the method for assessing and improving our representational apparatuses. The basic assumption is that, in so doing, conceptual engineering will enable the amelioration of the quality of one's performance when executing a cognitive task—"from the most sophisticated scientific research, to the most mundane household task" (Burgess and Plunkett 2013: 1097). And accordingly, the expectations on the program of conceptual engineering are very high. Yet, to date, no proper account for how to devise the methodological framework of conceptual engineering is available in the literature on conceptual engineering. The purpose of this talk is to make a first step towards overcoming this theoretical gap by providing a way to ensure the broadest scope and impact for the method of conceptual engineering.

II. BASIC ASSUMPTION — After having introduced the topic of my talk [Part I], I will next turn to spelling out its basic assumption. This assumption concerns what the subject matter of conceptual engineering should be, that is, how to construe the representational apparatuses that conceptual engineering is meant to assess and improve. And, building on a taxonomy that distinguishes several different types of 'representational engineering' (e.g. 'lexical,' 'terminological,' 'semantic', etc.), I will argue that conceptual engineering should be about concepts, on pan of being a misnomer otherwise—which would obviously turn the label itself into a very bad case of conceptual engineering (call it "the self-discrediting predicament").

III. MAIN ISSUE — Against the background previously set out [Part I, II], I will then focus on the main issue of the MET4CE Symposium, namely, the lack of any detailed methodological framework for conceptual engineering, at least as the program stands now. I will give two reasons why the Carnapian method of explication very well serves the purposes of the program of conceptual engineering—namely, its normativity and the fact that it may well have identified criteria that "govern by default (and thus defeasibly) conceptual [engineering]" (Machery 2017: 215). Then, I will identify three shortcomings of Carnapian explication—namely, its focus on individual concepts, the linearity of its structure, and its restriction to a theoretically-driven agenda (cf. Brun 2016, 2017). And in the remaining of my talk, I will focus on the last of these three shortcomings with a view to broadening the scope and impact of the method of conceptual engineering.

IV. POSITIVE PROPOSAL — In the last part of my talk, I will eventually outline a way to make conceptual engineering a widely applicable and highly adaptable method for the cognitive optimization of our conceptual apparatuses. To this end, building on a basic psychological characterization of concepts as default bodies of information about some referent (Machery 2009), I will argue that, for the purposes of conceptual engineering, concepts should be construed as as multiply realizable functional kinds (Machery 2010), that is: (i) as performing some specific causal/explanatory functions in our higher cognitive processes (e.g. abstraction, categorization, induction, etc.), and (ii) as realizable by several different context-dependent and task-appropriate basic kinds (viz. exemplars, prototypes, theories). And I will conclude, at last, by presenting a prototype framework for implementing this variant of hybrid theories of concepts (cf. Machery and Seppälä 2011) as a methodological tool for assessing and improving ANY of our conceptual apparatuses. Thereby, I will claim, conceptual engineering will be ensured the broadest scope and impact—namely, in the form of a broad-spectrum method.

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WHO DISCOVERED IMAGINARIES? ON THE HISTORICAL NATURE OF MATHEMATICAL DISCOVERY

Session 8C

Congress section(s): C1

In the aftermath of Thomas Kuhn's "The Structure of Scientific Revolutions", which established a new image of science, philosophers of mathematics also turned their attention to the role of paradigms and the possibility of revolutions in mathematics (e.g. Donald Gillies (ed.) Revolutions in Mathematics (1992)) What seemed to be needed was a new historiography of mathematics analogous to the new historiography of science proposed by Butterfield, Lovejoy and especially Alexandre Koyre. Yet the case of mathematics proved to be more difficult and less fruitful than that of modern physics. Mathematics, after all, presents us with a conceptual stability that grounds all other objective (empirical) sciences (this is the core of the argument that structural realists still use to counter Kuhn's position). Mathematical entities seem to have an existence outside of time that makes them immune to historical change as well as to the mathematician's mortal touch! In this paper, I argue that the history of mathematics, if seen as a "repository for more than anecdote or chronology" can present us with a fascinating and dynamic image of mathematics different from what appears to us from textbooks and what the traditional schools in philosophy of mathematics have proposed.

My methodology is based on the analysis of the case of imaginaries (square root of negatives) from the 16th century on. While any number of examples can be used to demonstrate the point about the dynamic nature of mathematics, imaginaries present a particularly interesting case given their origins, the longstanding dispute on their status, and their indispensable applicability in mathematics and other sciences. The goal of this study is to show that the distinction that philosophers of science make between seeing and seeing as is also of crucial importance in mathematics. Through examining the problem of "identification", I hope to pursuade philosophers of mathematics about the dynamic nature of mathematics, similar to that of modern physics and other empirical sciences.

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SENDING KNOWNS INTO THE UNKNOWN: TOWARDS AN ACCOUNT OF POSITIVE CONTROLS IN EXPERIMENTATION

Session 10C

Congress section(s): B1, C3, C4, C7

Although experimental inquiry has received due attention in the last 30 years or so, surprisingly little has been written specifically about the history and philosophy of experimental controls. This stands in spite of the obvious methodological importance of controls to scientists themselves, both within their own work and their role as peer reviewers.[1] Positive

controls and negative controls are used in a variety of different ways across scientific disciplines, despite there being no consensus on just what a “positive” or “negative” control is, nor what epistemic function these controls serve for the experimenter.

This paper offers a preliminary analysis of how positive controls function in experimental practice, using historical and contemporary examples from medical and life sciences. I approach this topic from a philosophy of measurement perspective. My aim is to generate a typology according to the epistemic role played by controls in the measuring process within the experiment or assay. This perspective provides a meaningful distinction between positive and negative controls, from which I further construct a finer-grained typology of two types of positive controls, “extrinsic controls” and “intrinsic controls.”

In the first part of the paper, I show that extrinsic positive controls are used in systems of comparison; a trial with a known intervention is conducted in order to compare results with that of an unknown intervention -- often, in tandem with a negative control (absence of intervention). This creates a scale of comparison for meaningfully interpreting results from the unknown intervention, which is performed in a separate experimental environment. Extrinsic controls are used for making sense out of the experimental data we get at the end of the day.

In the second part of the paper, I show that intrinsic positive controls play a very different role: ensuring that the experiment itself is operating as expected. They can be used for calibration of the experimental conditions, validation of instruments or indicators, and debugging during the design of the experiment or assay. As such, intrinsic controls share an experimental environment (or some aspects of the experimental environment) with the independent variable under study. Thus, intrinsic controls offer information within the long, tedious process of experimental design and offer assurance that the final experiment is operating under safe assumptions, particularly to ensure against false negatives.

While extrinsic and intrinsic positive controls are both used to aid our interpretation of experimental results, intrinsic controls play a role in experimental design, whereas the epistemic role of extrinsic controls begins after the experiment ends. In my analysis of intrinsic controls, I employ examples from microbiology, as a field whose subject matter forces special attention to careful experimental design. In the concluding section, I argue that the typology developed in this paper is general enough to apply to cases outside of life sciences, and has the potential to inform discussions on how to improve both internal and external validity for studies in social sciences.

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FINDING CAUSATION IN TIME: BACKGROUND ASSUMPTIONS FOR DYNAMICAL SYSTEMS

Session 4B

Congress section(s): B1

Much of scientific practice is concerned with the identification, prediction, and control of dynamical systems – systems whose states change through time. Though typically modeled with differential equations, most are presumed to involve causal relations, with the changing states of some variables driving change in others over time, often reciprocally. A variety of new tools have been introduced outside of the causal Bayes net framework to learn aspects of this structure from noisy, real-world time series data.

One collection of such tools is built around the notion of a dynamical symmetry [1]. This is a transformation of the state of a system that commutes with its time evolution. The set of dynamical symmetries of a system, along with the algebraic structure of those symmetries under composition, picks out an equivalence class of causal structures in the interventionist sense. This means that a test for sameness of dynamical kind can be used as a tool for validating causal models or determining whether observed systems fail to share a common causal structure. Algorithms have been published for implementing such tests, but they apply only to deterministic systems [2,3].

Here we present a generalization of the theory of dynamical kinds to the case of stochastic systems where the target question is not whether two systems differ in their causal structure, but to what degree. This requires addressing a variety of interrelated problems. First, though distinct from the causal graph approach, any inference from data concerning dynamical symmetries must deploy similar assumptions about the relation between the statistics of sampled variables and causal relations on the one hand, and about the causal relations themselves on the other. Chief among these is the assumption of Stochastic Dynamical Sufficiency (SDS). We clarify this condition, and draw out the ways in which it differs from the Causal Sufficiency condition in the Bayes net context.

Second, there is the question of how this sufficiency assumption interacts with the choice of variables. In typical real-world settings one is forced to use variables to describe a system that may aggregate over another set that obeys the SDS. Given that some set of variables meets the SDS condition, under what conditions can lossy transformations of these variables do so as well? How can the presence or absence of these conditions be verified? If a set of variables does not satisfy SDS, is it possible to determine whether a finer-grained description exists that does? How do the answers to these questions depend upon the kinds of interventions possible? For all of these questions, we present some preliminary answers, illustrated with ongoing applications to the study of collective behavior in animal swarms.

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TRUTH AND TRUTHFULNESS, PART I: WHAT FAKE NEWS IS AND WHAT IT’S NOT

Session 6B

Congress section(s): B1, B5

Recently, the term “fake news” has become ubiquitous in political and public discourse. Despite its omnipresence, however, it is anything but clear what fake news is. An adequate and comprehensive definition of fake news is called for. We provide a systematic account of fake news that makes the phenomenon tangible and rehabilitates the use of the term. Against the background of this account, we set fake news apart from related phenomena, such as journalistic errors, selective and grossly negligent reporting, satire, propaganda, and conspiracy theories.

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TRUTH AND TRUTHFULNESS, PART II: WHY THEY MATTER

Session 6B

Congress section(s): B1, B5“

On our view, fake news are news reports lacking on two dimensions: they are false or misleading (thus lacking truth) and they are circulated by people with an intention to deceive or a bullshit attitude (thus lacking truthfulness). Our definition is not only extensionally adequate, but can also contribute to improve the public debate. A special merit is that the definition lays open how fake news cause epistemic problems for societies, since truth and truthfulness are the foundations of knowledge, trust, and deliberation. Finally, we argue that our definition is superior to others that have recently been proposed by Matthew Dentith, Axel Gelfert, Nikil Mukerji, and Regina Rini.

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HILBERT AND THE QUANTUM LEAP FROM MODERN LOGIC TO MATHEMATICAL LOGIC

Session 3K

Congress section(s): A4

The leap to Mathematical logic is demonstrated by tracking a paradox posed in the Prior Analytics. Aristotle poses three problematic valid syllogisms which are not in any valid forms:

I.	II.	III.
W = K.	S is of Q.	S = N.
W is of G.	G = Q.	S is of G .
∴ K is of G.	∴S is of G.	∴ Some N is of G. (PA, 48a40–48b24)

Syllogistic logic does not capture either '=' or 'of'.
Leibniz comes close to the solution by formulating these arguments as hypothetical syllogisms but cannot produce valid syllogisms as he cannot invert the major and minor premises. Lenzen reformulates Leibniz's theorem 15 set theoretically so we get:

I.	II.	III.	
K⊆W ∧W⊆G → K⊆G	S⊆Q ∧ Q⊆G → S⊆G	N⊆S ∧ S⊆G→ N⊆G	(Lenzen, p. 15)

Boole suggests the shift from the logic of classes of syllogisms to the conditional logic of propositions and we can come close to seeing these as valid hypothetical syllogisms, yet Boole had not axiomatized propositional logic. Frege depicts these syllogisms as valid instances of theorems involving conditionals as well as quantifiers.
In Hilbert and Ackerman, first Aristotle's three syllogisms are translated into mathematical logic by a schema for translating categorical propositions into mathematical logic. Then, the arguments are put in Skolem Normal Form (SNF):

I. $(E\,g)(Ej)(Ek)(Ew)(Ex)(\forall h)(\forall u)\{[w(x)\wedge k(x)]\vee[w(x)\wedge\overline{k(x)}]\vee\{[u(g)]\}\vee[j(h)]$.

II. $(Ef)(Eg)(Eq)(Er)(Et)(\forall h)(\forall s)\{[s(q)]\vee[f(g)\wedge\overline{f(r)}]\vee[\overline{f(g)}\wedge f(r)]\vee[t(h)]$.

III. $(Eh)(Em)(En)(Es)(Ex)(\forall j)(\forall t)\{[s(x)\wedge\overline{n(x)}]\vee[s(x)\wedge n(x)]\vee[t(h)]\vee[m(j)]\}$.

Their validity can be demonstrated within the extended predicate calculus axiomatic system and the independence, consistency and completeness of the calculus is also proven. Hence, we see how the evolution of logic from Aristotle to Leibniz to Boole to Frege to Hilbert provides a complete and comprehensive solution to the problem of problematic syllogisms.

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THE HISTORY OF SCIENCE-RELATED MUSEUMS: A COMPARATIVE AND CULTURAL STUDY

Session 11D

Congress section(s): B7

Science-related museums are special kinds of museums that are concerned with science, technology, the natural world, and other related issues. Today, there are many science-realted museums worldwide operating in different styles, and playing different social roles such as collecting, conserving and exhibiting objects, researching relevant issues and educating the public. Through the different development process of science-related museums in the Western world and in China, we can say that science-related museums are outcomes of the influence of social and cultural conditions such as economy, local culture, policy, humans' views on science, and so on. The Western world is considered to be the birthplace of science-related museums, where the museums experienced different developments that includes natural history museum, traditional science and technology museum, and science centre. However, museums are imported goods for China. Foreigners and western culture affected the emergence of museums in China, while they are developing rapidly today.

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ENTERING THE VALLEY OF FORMALISM: RESULTS FROM A LARGE-SCALE QUANTITATIVE INVESTIGATION OF MATHEMATICAL PUBLICATIONS

Session 14A

Congress section(s): B3, C1

As pointed out by Reuben Hersh (1991) there is a huge difference between the way mathematicians work and the way they present their results. In a previous qualitative study on mathematical practice we confirmed this result by showing that although mathematicians frequently use diagrams and figures in their work process, they tend to downplay these representations their published manuscripts, in part because they feel subjected to genre norms and values when they prepare their work for publication (AUTHOR and ANONYMIZED 2016; Accepted). This result calls for a better understanding of these genre norms and for the development the norms may undergo over time.
From a casual point of view, it may seem that the norms are currently in a process of change. The formalistic claim that figures and diagrams are superfluous has been contested by philosophers of mathematics (e.g. Brown 1999, Giaquiont 2007), and looking at mathematics journals and textbooks, one gets the impression that diagrams and figures are being used more frequently. That however is merely an impression, as we do not have solid empirical data tracking the representational style used in mathematics texts.
In order to fill this gab ANONYMIZED, ANONYMIZED and AUTHOR developed a classification scheme that makes it possible to distinguish between the different types of diagrams used in mathematics based on the cognitive support they offer (AUTHOR et al 2018). The classification scheme is designed to facilitate large-scale quantitative investigations of the norms and values expressed in the publication style of mathematics, as well as trends in the kinds of cognitive support used in mathematics. We presented the classification scheme at conferences and workshops during the summer 2018 to get feedback from other researchers in the field. After minor adjustments we applied the scheme to track the changes in publication style in the period 1885 to 2015 in the three mathematics journals Annals of Mathematics, Acta Mathematica and Bulletin of the AMS.
In this talk I will present the main results of our investigation, and I will discuss the advantages and disadvantages of our method as well as the possible philosophical implications of our main results.

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THE DIRECTION OF TIME

Session 28K

Congress section(s): C2

A common belief is that the direction of time is based on a global directedness, a universal time with direction, and this is often thought of as based upon the second law of thermodynamics + the assumption that the entropy immediately after Big Bang had a very low value. There are several problems with this account, the most severe is perhaps Price' observation that it presupposes a distinction between earlier and later events, thus in fact presupposing time directedness.
I will instead explain the directedness of time by considering how we measure time with clocks.
A clock is a system consisting of two parts, one that oscillates (a certain number of oscillations define the time unit) and one that counts the oscillations. Any counter which keeps track of the number of oscillations will do. This counter must by necessity undergo irreversible state changes when counting.
Before we introduce the notion of time we can determine which of two states is the earlier one when observing two states of a counter which counts the number of a clock's oscillations, even if we don't know which observation is done first. Hence irreversibility of state changes of partially isolated physical systems can be used to define the direction of time for limited periods. Then a global directed time can be constructed out this shorter time periods, since different clocks' working periods in many cases partially overlap.
A clock is by necessity a macroscopic object. This can be inferred from the result of Misra et.al. (1979), viz., that no operator with monotonously increasing expectation value can be defined on Hilbert spaces. In other words, pure quantum systems cannot function as counters.
This tells us that the directedness of time is not based on properties of individual quantum systems. Hence, a clock must be a macroscopic system in order to be able to register and record a number of oscillations.
Sooner or later any particular clock mechanism will stop functioning. But we can use many such clock mechanisms, partially overlapping in time, when constructing a universal time, thus giving us a universal direction of time.
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THE EVOLUTIONARY EPISTEMOLOGY OF RUPERT RIEDL – A CONSEQUENT REALIZATION OF THE PROGRAM OF NATURALIZING EPISTEMOLOGY?

Session 31I

Congress section(s): B1, C3

The program of evolutionary epistemology, of which Rupert Riedl is one of its main representatives besides Konrad Lorenz and Gerhard Vollmer, is to describe the development of consciousness and human reason as the natural consequence of the principles of evolution. As Konrad Lorenz pointed out, all humans including the natural scientists are also living beings, which owe their properties, including the ability to recognize, to evolution, in the course of which all organisms have dealt with reality and adapted to it.

According to Rupert Riedl, evolutionary epistemology is understood as a theory derived from biology in order to gain understanding of cognitive processes. Evolutionary epistemology seeks to study how knowledge is acquired in organic life, how living beings attain knowledge about the world, and how living beings form a picture of the world.
Riedl is concerned with a study of the human cognitive apparatus. He regards human intuition as innate and prior with respect to our individual reason. At the same time, these forms of intuition would also be posterior and inherited products of adaptation and learning of a biological tribe. Thus, they are products of phylogenetic learning, which determine our intuition. They evoke in us a system of fundamental hypotheses, which functions as a ratiomorphic teacher and predetermines our consciousness.
For Riedl and Lorenz, life in itself is a process of knowledge acquisition, through which biological structures reflect and manage their surrounding environment. Riedl's idea is that the patterns of the order of human thought processes derive from the systematic order of organic nature. The patterns of thinking are thus to be seen as a consequence of a mapping of the patterns of nature and ultimately are also a product of natural selection. After all, human culture as a whole could only be understood as a consequence of evolution. From this point of view, all basic patterns of human thought have emerged only on the basis of patterns of order on the more fundamental material and organic levels.
The evolutionary perspective of Rupert Riedl and others like for instance Konrad Lorenz or Gerhard Vollmer leads to a naturalization of epistemology. This naturalistic point of view implies that we can objectively grasp nature and that an objective understanding of nature is a prerequisite for our understanding of human culture as well. According to this view, study of the "culturomorphic" superstructure is insufficient for gaining understanding of human reason; it is also necessary to consider the "theriomorphic", viz., animal-organic dispositions which form the basis for the formation of the culturomorphic superstructure and for the formation of human reason as well.
Evolutionary epistemology, in the sense of Riedl, seeks to elucidate the genetic makeup of man's psychic equipment as well as his cognitive and social capabilities. The causes of the product of the hereditary fixations are to be found in the conditions for adaptation to extrasubjective reality and in the natural history of the human species. Riedl emphasizes the role of hereditary instructions in our human equipment which determine our vision of the world. Thus, our worldview apparatus has developed in real life, so to speak, it initially has been adapted for the purpose of mere survival, rather than world knowledge.
This paper reexamines the views of Riedl and Lorenz, in connection with related views to be found, for example, in Gerhard Vollmer, Erhard Oeser or Franz Wuketits. On this basis, I will try to develop preliminaries toward a comparison with more well known varieties of evolutionary or naturalized epistemology such as to be found in the philosophies of Karl Popper or W. V. O. Quine.

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COALITIONAL LOGIC ON NON-INTERFERING ACTIONS

Session 13B

Congress section(s): A2

Suppose that there is a group of agents, and they perform actions and the world will change. Assume that they change different parts of the world and these parts do not overlap. Under this assumption, their actions do not interfere with each other. Then the class of possible outcomes of a joint action is the intersection of the classes of possible outcomes of those individual actions in that joint action. This property can be called the intersection property. A special case of the previous assumption is that every agent controls a set of atomic propositions and these sets are disjoint. This is a basic setting in [HoekWooldridge05].
In Coalition Logic (CL), proposed by [Pauly02], the class of possible outcomes of an individual action consists of the possible outcomes of the joint actions that are extensions of that individual action and possible outcomes of joint actions are arbitrary. As a result, the intersection property is not met.
The STIT logic proposed by [Horty01] has the intersection property. However, it requires that the classes of possible outcomes of the individual actions of the same agent are disjoint. This constraint is too strong.
This work presents a complete coalitional logic NiCL on non-interfering actions.

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UPDATING SCIENTIFIC ADVISER MODELS FROM POLICY-MAKER PERSPECTIVES: A LIMIT OF DEBATE OF ‚SCIENCE AND VALUE' AND NORMS OF PUBLIC POLICY

Session 4G

Congress section(s): B5

The importance of use of scientific knowledge in policy making has been widely recognised. In philosophy of science, the argument from inductive risk, which involve the ethical value judgment by scientists has become increasingly sophisticated and help us to consider the ways of normative judgments by scientific advisers in policy making. Indeed, values play a role in all scientific processes. Each scientific organisation, group, and individual has to consider ethical consequences at different levels (Steel 2016). However, most of the philosophical approaches for improving scientific adviser models in policy making have overlooked a significance of advocacy-embedded political situations. Although the process of production and use of scientific advice should be understood as a problem of boundary work, in which continuous process between demarcation and coordination by scientist and policy maker at the science/policy interface (Hoppe 2005), those studies were unwilling to reduce the problem to only science-side, that is, the issues of value-ladeness of science. The purpose of this paper is to analyse the problem which cannot be handled in the debate of 'science and value', to improve the scientific adviser model from the viewpoint of the policy-side. In complex and high-stakes environmental problems, it is possible to accumulate legitimate facts based on different kind of values. As a result, policy makers cannot yield a consistent view about mutually incommensurable but legitimate scientific expertise. This problem is known as "excess of objectivity" (Sarewitz 2004). How should policymakers make a judgment on adopting or rejecting conflicted scientific advice? By examining public policy norms, I find appropriate strategies to resolve value conflicts in the face of the excess of objectivity. Considering from the policy-side make enables to understand scientific adviser model in the science-policy interaction. Recent developments of scientific adviser model- pragmatic model (e.g., Brown and Havstad 2017) - also emphasised this interaction. However, there is potential for improvement in those studies. This paper confirms that the significant components of the pragmatic model are unlikely to function, at least insufficient, in practice and present a roadmap for improvement.

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MACHINE LEARNING: A NEW TECHNOSCIENCE .

Session 11F

Congress section(s): C9

There has been a tremendous development of computerized systems for artificial intelligence in the last thirty years. .

Now in some domains the machines get better results than men:
– playing chess or even Go, winning over the best champions,
– medical diagnosis (for example in cancerology)
– automatic translation,
– vision : recognizing faces in one second from millions of photos...
The successes rely on :
– progress in hardware technology, of computational speed and capacity of Big Data ..
– New ideas in the structure of the computers with the neural networks ,inspired originally from the structure of vision treatment in the brain ,and progress in mathematical algorithms for exploiting statistical data, extensions of Markovian methods.
These developments have led the main actors to talk about a new science, or rather a new techno-science: Machine learning, defined by the fact that it is able to improve its own capacities by itself (see [L]).
Some opponents give various reasons for their scepticism , some following an old tradition of identification of 'Numbers ' with the modern industrial civilization [W],some with theoretical arguments ,coming from the foundations of information and complexity theory (Ma]), some doubting of the bayesian inferential approach to Science – refusing prediction without understanding [Th] which might lead to a radical attack on classical science ([H]). In particular the technique of neural networks created a new type of knowledge with a very particular mystery of 'Black Box'. We will describe the new kind of '' truth without verificability '' that is issued from this practice.
We will discuss carefully these various topics,in particular:
Is it a new science or a new techno-science ?
Where is the limit between the science of Machine Learning and various conjectural visions leading to the science-fiction's ideas of transhumanism ?
What are the possible consequences of recent success of AI on our approach of language, of intelligence of man's cognitive functioning in general. And finally what are the limits of this numerical invasion of the world ?

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CONNEXIVITY AND CONDITIONAL LOGIC

Session 26F

Congress section(s): A2

In this talk, I will comment on the relationship between connexive logics and conditional logics in the Lewis/Stalnaker theory. In particular, I will be interested in the philosophical underpinnings of these two large projects and in how much these underpinnings intersect. Though it has always been clear that there seems to be some connection here, it has, I believe, not yet been established what that connection is, precisely. I will propose a view of connexivity (drawing on earlier work) that not only fits well to the philosophical discussion about conditonal logics, but is also able to shed new light on topics in that discussion, such as the dispute about the Law of Conditonal Non-Contradiction.

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WHY COGNITIVE KINDS CAN‘T BE THE KIND OF KINDS THAT ARE NATURAL KINDS? A NEW HYPOTHESIS FOR NATURAL KINDS

Session 31L

Congress section(s): B4, C5

It is generally assumed by philosophers that natural kinds mirror the causal structure of the world or corresponds to its real division. Ultimately natural kinds are to be found in the fixed order of nature that we humans endeavour to pick them up through different operations.
Boyd and Tobin (2012) offer a very clear-cut characterization of natural kinds put in the following terms: “To say that a kind is natural is to say that it corresponds to a grouping or ordering that does not depend on humans”. They are quite right in underlining that natural kinds are independent of any human whims and desires and that what makes natural a natural kind is its naturalness, in other words, the main criterion to warranty ‚naturalness‘ is human mind-independence; nonetheless, such notions as ‚natural‘ or ‚mind‘ require a thorough characterization.
Four main views of natural kinds have been put forward from the late 1970s: essentialists, conventionalists, homeostatic property cluster (Boyd, 1991) and natural binding practices. Each of these views is unsatisfactory relative to natural kinds and cognitive kinds, for example Sullivan (2016) contends that “scientific practice in the neurosciences of cognition is not conducive to the discovery of natural kinds of cognitive capacities” and Irvine’s (2012) eliminative statement “The lack of well-demarcated and suitably targeted mechanisms of consciousness means that there are no scientific kinds that ‘consciousness’ refers to”.
The main purpose of this presentation is to hypothesize a new view on natural kinds embedded in common sense realism and active realism (Chang, 2012; Sankey, 2014). To that end, I will first lay down a critique of the assumption that natural kinds are also those events, phenomena, mechanisms, processes, relationships, properties, functions, capacities, laws and concepts that play the role of a demarcating tool in the structure of nature by claiming that their relative complexity and enmeshed layers do not support for their candidacy as natural kinds. Then, I will proceed toward setting reasons why cognitive kinds can‘t be natural kinds by maintaining that at best they can be no more than brain functions. Finally, from the previous delineated evidence, I will devise arguments for a new hypothesize on what counts as a natural kind.

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ABSTRACT AND CONCRETE CONCEPTS: AN APPROACH TO CLASSIFICATION

Session 13M

Congress section(s): A2

1. Term logic, called also traditional or Aristotelian logic, deals with terms as the main components of propositions. Terms are considered to represent ideas or concepts. Concepts are usually divided into concrete and abstract ones. Concrete concepts (such as Socrates, tree, table, etc.) are taken to refer to things or objects, and abstract concepts (such as wisdom, truth, etc.) refer to properties of objects. Objects, in contrast to properties, have independent being.
V. Bocharov and V. Markin in their textbook «Elements of Logic» (Moscow, 1994) propose a refinement of this classification by distinguishing between various kinds of objects. Namely, they consider individuals, n-tuples of individuals and sets of individuals to be different types of objects, which can have properties and enter into relations with each other. Then a concept is said to be concrete if and only if its extension consists of individuals, n-tuples of individuals or sets of individuals, and it is abstract otherwise.
2. This classification is problematic, since it does not fit a common-sense idea of abstract/concrete distinction, and moreover, it finds no support in observations of modern psychology. Specifically, according to Bocharov-Markin’s definition, such abstract objects as numbers, geometrical figures, truth values, etc. are considered to be individuals, and thus, the concepts about them should be recognized as concrete side by side with concepts about tables, chairs and trees. Moreover, a concept about concept will then also be concrete, what is counterintuitive. Besides, one and the same concept can appear both concrete and abstract, depending on different treatment of the corresponding objects. It seems also difficult to differentiate between concepts having different types of abstractness, such as friendship and symmetrical relation.
3. I propose an approach to the concept classification based on a metaphysical division between particulars and universals. Accordingly, the concepts can be divided into logically concrete and logically abstract. As usual, particulars can be defined as concrete, spatiotemporal entities accessible to sensory perception, as opposed to abstract entities, such as properties or numbers. Then a concept is logically concrete if and only if its extension consists of particulars, and it is logically abstract otherwise. Thus, logically abstract concepts concern various kinds of universals, such as sets of particulars, properties, relations, abstract objects, theirs n-tuples, sets, etc. This approach makes it possible to differentiate between levels of abstractness. Thus, concepts about properties, relations and functional characteristics will be less abstract then, for example, concepts about properties of properties, etc.
4. The proposed idea of the concept classification based on the types of generalized objects opens further opportunities for a natural language analysis and determining a degree (level) of abstractness of the given discourses and domains.

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THE SCIENTIST’S DILEMMA: AFTER WEBER

Session 28B

Congress section(s): B1, B5

Max Weber’s lecture “Science as vocation” [Wissenschaft als Beruf], which will be celebrating the 100th anniversary of its publication in 2019, raises a number of ethical issues in science, which topicality only grows during the last century. Among them there is a problem confronting the underdetermination of the disciplinary status of the ethics of science in terms of philosophical and specially-scientific subject matters, methods, on the one hand, and the goal to determine methods and approaches for a normative management of science. Along this way, one has to compare the philosophical and other ethical and regulatory programs as it relates to science; to respond to questions about resolving Hume’s guillotine and the paradox of Merton-Popper in the ethics of science; to demonstrate the ethics of science ability to justify science as a form of public good. The ethics of science has been partially formed by transferring the methods of its justification from philosophical ethics: as descriptive, normative and applied ethics, as metaethics. And here, Aristotle and Kant dispute with Bentham and Quine on the autonomy of or naturalizing the ethics. It turns out that a clear choice among these programs is hardly possible for the ethics of science. It cannot, on the one hand, do without borrowing empirical evidence from the cognitive sciences and the

cultural studies, and on the other, cannot be limited to the actual state of affairs. Rather, specially scientific and philosophical aspects of the ethics of science (descriptive, applied, professional, normative ones) focus on the various types of formal and informal regulation of science. The ethical impetus generated by Max Weber's lecture "Wissenschaft als Beruf" helps bridge a gap between these two dimensions: a "profession" as a feature of science's social institute and "vocation" as an existential propensity of a person. The Merton-Popper paradox is also an attempt to elucidate and to reconcile the factual autonomy of profession and vocation pointed out by Weber. I propose to project the ethos of science assimilating some approaches in virtue epistemology (J. Kvanvig, J. Turri) in order to resolve the paradox. In the ethos, particular norms and values operate at the definite structural levels of epistemic community and at the stages of its development. Also, they occasionally transcend the core of scientific community and influence the social change. Namely, epistemic virtues and vices can be viewed as distributed among three dimensions of scientific community viewed in terms of the revised grid-group analysis (M. Douglas): inner structure, group boundary and the pass transmitting the content of inner structure over the group boundary to the external surrounding of science, society as a whole. Let inductive inference hardly bridge a reliable gap from moral facts and moral norms as well as from epistemic facts and epistemic norms. Still, one is able to use the gap positively, falsifying norms with facts and criticizing facts in the face of norms. Thus the communication aimed at resolving Hume's guillotine and Merton-Popper paradox creates a rational discourse transcending science toward society and establishing an external "trading zone". Yet it is hardly a trade that shapes the game; it is rather a gift (an analogy borrowed from M. Mauss)/ As a result, special epistemological status of science is ethically justified as providing integrity, solidarity and common good.

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UNDERDETERMINATION OF THEORIES, THEORIES OF GRAVITY, AND THE GRAVITY OF UNDERDETERMINATION

Session 4I

Congress section(s): B1, B2, B6, C2

Arguments for underdetermination seek to establish that evidence fails to determine a scientific theory over its rivals uniquely. Given the various ways in which underdetermination has been argued for, it is best to see those arguments as constituting a class whose members purport to list conditions under which evidential worth of rival theories become inoperative. One common requirement for underdetermination has been the acceptance of holism in theory-testing as it allows for theoretical terms to form part of scientific terminology without ever being directly confirmed by empirical tests (W.V.O Quine: 1975, 1992). Another requirement is for the rival theories to be empirically equivalent, although we argue that a suitably defined notion of 'empirical success' can replace the more stringent requirement of empirical equivalence. Further requirements come from acknowledging the domain-specific reliability of inductive strategies. While the unsuitability of eliminative inference in fundamental theorising has been noted (Kyle Stanford, 2006), inductive generalisation is similarly unreliable if it involves extending a theory to worlds radically different from those where the theory has proven to be empirically successful. We propose an account of underdetermination that exists between those empirically successful theories which are central to those scientific domains where otherwise reliable inductive strategies can be expected to fail. We identify theorising about gravity as particularly vulnerable to our version of underdetermination and present a brief overview of theories of gravity past and present – from Aristotelian cosmology to General Relativity and its possible modification by MOND theories – to support our assertion.

Underdetermination has erroneously been seen as a consequence of the supposed intractability of the Duhem-Quine problem (the question of how blame is to be partitioned between conjuncts in the event of falsification), which in turn is supposed to follow from holism. However, mere acceptance of holism need not entail the irresolvability of the Duhem-Quine problem, as Bayesian reconstructions of various historical episodes show. The Bayesian solution to the Duhem-Quine problem, on the other hand, should not be seen as the resolution of underdetermination. To establish this, we select two debates over the nature of gravity – the first surrounding the precessing perihelion of Mercury and its explanation by General Relativity, and the second over the correct explanation for the mass discrepancy suggested by galaxy rotation curves and cluster dispersion data. We offer a Bayesian reconstruction of these two debates to show that although Bayesian confirmation theory can model how falsification affects individual conjuncts even in the absence of any differentiating role played by the prior values of conjuncts, it offers little to assuage fears of underdetermination. For although the Bayesian machinery successfully models how evidence should affect belief in a theory, it can offer little help if the evidential worth of theories were to become inoperative, as it happens under conditions that lead to underdetermination.

Underdetermination is generally understood as engendering scepticism about the possibility of knowing certain features of the world. We stand with the scientific realists in holding that most such arguments, including ours, do not threaten sophisticated accounts of selective realism. Apart from scepticism about knowability, underdetermination also motivates scepticism about decidability. Weaker versions of underdetermination, like the one we present, suffice to establish the scepticism about non-uniqueness in theory choice. It is in relation to the problem of theory choice that we see the significance of underdetermination. Specifically, we argue that because underdetermination renders the evidential worth of theories inoperative, it leads to a particularly intransigent disagreement over theoretical descriptions of the world. We also show that underdetermination induced disagreements are epistemically desirable as they promote inter-disciplinarity, consilience, and theoretical unification. We support this conclusion by examining strategies that have been employed in the current debate over dark matter and modified gravity as the explanation for the 'missing mass problem'.

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A LEARNING THEORETIC ARGUMENT FOR SCIENTIFIC REALSM

Session 18K

Congress section(s): B1, B2, B3

Scientific realism has long been tied to no-miracle arguments. Two different measurement technologies would not miraculously generate the same spurious result (Hacking 1981). If there were absolute motion, the moving magnet would not miraculously produce the same current as the moving coil (Einstein 1905). Independent mechanisms in the complete causal story would not miraculously cancel---else there is a meta-cause of the cancelation missing from the story (Spirtes et al. 1993). Other celebrated examples include Copernicus' argument against epicycles, Newton's argument against celestial magnetism, Darwin's argument against special creation, Lavoisier's argument against phlogiston, and Fresnel's argument against Newton's particle theory of light.

In each of those examples, there are two competing theories, a simple one and a complex one, and a miraculous tuning of the complex causes is capable of generating simple observations for eternity. Anti-realists (van Fraassen 1980) ask how a bias in favor of the simple theory could be truth-conducive, since one would receive exactly the same data for eternity regardless whether the truth is simple or miraculous.

A pertinent response is that favoring the simple theory until it is refuted fails to converge to the truth only over the set of all miraculous worlds, which is negligible given both the simple theory and given the complex theory, whereas favoring the complex theory no matter what fails in all simple possibilities, which is non-negligible given the simple theory.

But a gap remains in that argument, because one can still favor the complex theory over a refutable set of miraculous worlds, for which the empirically equivalent simple worlds are negligible given the simple theory. The failure set for that method is still negligible given both theories, so the ball is still in the realist's court.

But not all convergence to the truth is equal. Plato held that one finds the truth better if one finds it indefeasibly. We show, by a novel, topological argument, that any method that (1) favors even a single miraculous world, and whose failure set is

negligible given both the simple and the complex theory, drops the truth in some complex world. Thus, the Pyrrhic reward for favoring complex worlds indistinguishable from simple worlds is either to fail non-negligibly given some theory or to drop the truth in a complex world. In contrast, the realist's favored Ockham method that always favors simplicity fails only negligibly given either theory, and never drops the truth. The argument is extendible to statistical inference. It provides a new foundational perspective on theoretical science in general, and on recent causal discovery algorithms in particular.

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MESSY METAPHYSICS: THE INDIVIDUATION OF PARTS IN LICHENOLOGY

Session 4A

Congress section(s): B1

Lichens are defined as symbiotic systems that include a fungus (mycobiont) and a photosynthetic partner (photobiont), such as algae or cyanobacteria. The standard view has been that lichens are systems that have one fungus—typically an Ascomycete or Basidiomycete. Although other fungi are known to be parts of the lichen (in a less functional or evolutionarily impactful role), the classical view of lichen composition of mycobiont-photobiont has been widely accepted. This bipartite view suggests that the criteria for lichen stability is the presence of the same mycobiont in the lichen system and underpins classificatory practices that rely on the fungus to name lichens.

But this one-lichen, one-fungus metaphysics ignores relevant alternatives. Recent discoveries show that some lichens are composed of three rather than two symbiotic parts (Chagnon et al 2016). The metaphysical concept of the lichen and what are considered to be its parts determines how lichens are individuated and how they are named and tracked over time. Naming the lichen symbiont relies on capturing its parts but also on the means by which we attribute parthood. If we say that something is a part of something else, reference to its parthood is typically thought to be metaphysically grounded (e.g. its parthood is due to a particular relationship of composition, kind membership, or inheritance), or, saying that something is a part might be indicative of our understanding of its role in a process (e.g. which entities are involved in a pathway's functioning over time). Brett Calcott (2009) suggests that parts may play different epistemic roles depending on how they are used in lineage explanations and for what purpose parthood is attributed to them. A part may be identified by the functional role it plays as a component of a biological process. Or, parts-talk may serve to indicate continuity of a phenomenon over time, despite changes between stages, such that one can identify it as the same at time T1 as at time T2. I employ Calcott's account of the dual role of parts to shed light on the messy individuation activities, partitioning of the lichen symbiont, and criteria of identity used in lichenology. I use this case to explore what grounds we have for relying on different ontologies, what commitments we rely upon for our classifying practices, and how reliable taxonomic information can be gleaned from these messy individuation practices. I show how ontological messiness may be both (i) problematic in making it difficult to count biological individuals or track lineages, and (ii) useful in capturing the divergent modes of persistence and routes of inheritance in symbionts.

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SYMPOSIUM ON JOHN BALDWIN'S MODEL THEORY AND THE PHILOSOPHY OF MATHEMATICAL PRACTICE

Session 30A

Congress section(s): A1, B1, C1

John Baldwin's monograph Model Theory and the Philosophy of Mathematical Practice (Cambridge) ([Bal18]) provides a novel setting within which various philosophical questions can be raised.

A metaphilosophical question comes immediately to mind: As a work in the emerging area of the philosophy of model theory, how does Baldwin's approach define the central philosophical concerns of this new field? Also, what light does the Baldwin approach to the philosophy of mathematical practice shed on that practice, that is not available in other approaches?

The general philosophical point of view on which Model Theory and the Philosophy of Mathematical Practice relies is built on the idea of localization; of seeking targeted, local foundations for mathematics, as opposed to a global foundation. What is the nature of this seemingly anti-foundationalist view, and how deep does this anti-foundationalist stance reach? Does localizing in this case mean rejecting any kind of global framework? If so, how does [Bal18] shape this seeming pluralism into a coherent philosophical view? Or are questions of this kind bracketed in favor of the focus on methodology?

Shelah's dividing lines, established by the Main Gap Theorem, play a central role in the book, the author expounding Shelah's dividing line strategy as a general methodology in [Bal18, Chapter 13]. How do we know that this or indeed any classificatory scheme in mathematics tracks the actual contours of the subject? For example, theories on the structure side of Shelah's Main Gap Theorem admit dimension-like geometric invariants. Do these theories track our geometric or spatial intuition more closely than theories on the non-structure side, if they can be said to track these intuitions at all?

How does [Bal18] weigh in on the question whether the Main Gap Theorem is a foundational theorem in the sense that Hilbert imagined, demarcating tractable vs the untractable in mathematics? Or are the theories on the non-structure side tractable from some other point of view?

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SIMILARITIES AND DIFFERENCES IN THE LOGIC OF ARISTOTLE AND AVICENNA

Session 28H

Congress section(s): A4

The article analyzes some similarities and differences in the logic of Aristotle and Avicenna. A brief analysis of the logical teachings of Aristotle and Avicenna shows that the main differences in the views of Aristotle and Avicenna are found only in determining the logic and range of problems that constitute the subject of this science.

In his solution of this question, Avicenna was at the side of the Neo-Platonists and showed irrefutable evidence in support of their view that logic is both a part of philosophy and an instrument of science. Whereas, according to Avicenna, the meaning of logic as part of philosophy is to study the forms of thinking, its value as an organon is that, permeating all sciences as an organon, it connects and unites them into one single system.

Avicenna also developed, more thoroughly than Aristotle, a theory of proposition, which includes the doctrines of both categorical and conditional propositions and conditional syllogisms.

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SEMIOTIC ANALYSIS OF DEDEKIND'S ARITHMETICAL STRATEGIES

Session 16A

Congress section(s): C1

In this talk, I will present a case study, which uses close reading of [Dedekind 1871] to study semiotic processes of mathematical text. I focus on analyzing from a semiotic perspective what Haffner [2017] describes as 'strategical uses of arithmetic' employed by Richard Dedekind (1831-1916) and Heinrich Weber (1842-1913) in their joint work on function theory [Dedekind and Weber 1882]. My analysis of Dedekind's representations shows that neither word-to-word correspondences with other texts (e.g. texts on number theory), nor correspondences between words and stable referents fully capture Dedekind's "strategic use of arithmetic" in [Dedekind 1871]. This use is thus the product of a textual practice, not a structural correspondence to which the text simply refers.

An important line of argument in [Haffner 2017] is that a mathematical theory (be it function theory as in [Dedekind and Weber 1882] or ideal theory as in [Dedekind 1871]) becomes arithmetical by introducing concepts that are 'similar' to number theoretical ones, and by transferring formulations from number theory. Haffner's claim only emphasizes why we need a better understanding of the production of analogy as a semiotic process. Since the definitions and theorems of [Dedekind 1871] do not correspond word-for-word to number theoretical definitions and theorems, simply saying that two concepts or formulations are 'similar' neglects to describe the signs that make us see the similarities. Thus, appealing to similarity cannot account for the semiotic processes of the practice that produces analogy of ideal theory to number theory. The case study aims to unfold Haffner's appeals to similarity through detailed descriptions of representations that Dedekind constructs and uses in [1871].

Dedekind is often pointed to as a key influence in shaping present mathematical textual practices and a considerable part of this influence stems from his development of ideal theory, of which [Dedekind 1871] is the first published version. Therefore, apart from being interesting in their own right, a better understanding of the semiotic processes of this text could contribute to our views on both present mathematical textual practices and late-modern history of mathematics.

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DETERMINISTIC AND INDETERMINISTIC SITUATIONS

Session 17K

Congress section(s): B2

The arguments for establishing the compatibility of determinism and chance usually turn on different levels of description of reality. Different levels of description delineate different sets of possible worlds with their associated state spaces and chance functions. I try a different route to argue for the compatibility of some form of determinism with indeterminism by explicating these ideas in terms of truth-maker semantics, as developed by Kit Fine. I believe the appeal of this approach is that the point can be made without having recourse to a linguistic framework to delineate different levels of description. Instead we can model different levels of reality in a more direct manner. To that end, I start with a situation space (S, ⊑), where S (the set of situations) is non-empty and ⊑ (part) is a partial order on S. Informally, the situation of my tasting a piece of chocolate is part of my tasting and finishing the chocolate bar. The situation space is also endowed with a fusion operator, which enables us to talk about extensions of a situation. The maximal extensions of a situation make up possible worlds

(called world-states), but we need not define determinism at that global level. Situations can be actual or possible, and we discern possible situations, a subset of S, by S◊, so that we can define compatible or incompatible situations. My tasting the chocolate is compatible with the situation of my finishing it and also compatible with the situation of my not finishing it. We call a situation s in S deterministic iff whenever some s' and t in S are such that s'⊑s, and s'⊑t, then either s⊑t or t⊑s. That is, s is deterministic iff it is part of the unique extension of its sub-situations.

Suppose we aim to model a micro-level reality grounding the situations of S. Let us assume we have another situation space (Sm, ⊑m), with its set of possible situations
denoted by Sm◊, and fusion operator ⊔m, satisfying the mereological conditions
sketched above. Assume for each s in S◊, there exists a subset Sm(s) of Sm such that the fusions of the elements of Sm(s) make up any part of s. More precisely, if s'⊑s, then s' is equivalent to some ⊔msm,i, where {sm,i} is a subset of Sm(s). What I have described so far does not preclude the possibility that the micro-level is identical with the macro-level. That is not a short-coming, as it can very well be the case in some possible worlds. The interesting possibilities, however, are when the levels differ, and allow us to find examples when the situations are deterministic at the macro-level and indeterministic at the micro-level or vice versa. That possibility is what I illustrate in my talk. I also dwell on the advantages of this approach for its avoidance of a chance function to represent indeterminism, invoking Norton's indeterministic dome example.

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TOWARDS THE RECONCILIATION OF CONFIRMATION ASSESSMENTS

Session 31H

Congress section(s): B2, B3, C2

The philosophy of scientific confirmation has as its task the formal and informal representations of scientific reasoning, in particular, how evidence relates to the confidence that scientists have in theories. This paper examines the recent discovery of the Higgs boson at the LHC, and in particular the reasoning that has led to the high confidence in a particular hypothesis about the Higgs boson, the Standard Model (SM) Higgs hypothesis. While the confidence in the SM Higgs is very high, a logical model of confirmation, Bayesian or otherwise, falls short of establishing this. What has been confirmed by the LHC data, as Dawid (2015) and others correctly point out, is the Higgs mechanism of spontaneous electroweak symmetry breaking, but not any particular implementation of this mechanism. The paper aims to address the question of how it is that a particular formulation of the Higgs hypothesis is taken to be so highly confirmed. That is, how can one account for, and possibly bridge, the gap between the informal and the logical assessments of confirmation. This paper consists of two broad parts.

First, to show how far the SM Higgs hypothesis is logically confirmed and to elaborate on the limitations of this kind of confirmation, which is done in Sections 2. Here, I review what I call the "direct confirmation" of the SM Higgs hypothesis in isolation, which captures the precision, novelty, and riskiness of the predictions with respect to the data.

In Section 3, I examine this hypothesis in the context of other competing hypotheses and show that the lack of unique predictions that can currently be determined undermines claims about the high degree of confirmation of the SM Higgs hypothesis. The second part explores two avenues that might enrich a simple model of confirmation to reconcile it with the judgements of physicists. The first is to consider what I call the "indirect confirmation" of the hypothesis, which stems from the degree to which alternative hypotheses are being ruled out by data. This approach is shown to be problematic given that the competing models are in very close agreement in achievable energy levels. The second is to factor is that not all predictions are equally valuable for confirmation. To capture this I introduce a hierarchical Bayesian framework for confirmation to make use of a structured hypothesis space and hyperparameters to capture the varying levels of importance of a model's hypotheses. This avenue is also problematic given that the probabilities must be determined by products and a single unconfirmed hypothesis gives the entire model a zero degree of confirmation.

Thus, finally an approach is taken where the individual contributions are summed and suitably normalised. This is not a strictly Bayesian approach, but its machinery can bring us towards the reconciliation of confirmation assessments.

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REMARKS ON ABSTRACT LOGICAL TOPOLOGIES: AN INSTITUTIONAL APPROACH.

Session 29J

Congress section(s): A1, A3

In “Logical Topologies and Semantic Completeness” [10] V. Goranko established a master note about the connection between logic and topology. He proposed a topological approach to prove semantic completeness of a logical system with respect to a class of “standard models”, provided a weaker completeness result with respect to a larger class of “general models”. The author has pointed that there is no general method for solving the problem described above, but usually some specific model-theoretic constructions are applied which transform general into standard models while preserving satisfiability and that topological methods and results have so far been under-utilized for solving purely logical problems.
Our goal is to establish an appropriate framework for all above within an axiomatic setting, for this reason, we appeal to Institution Theory.
Institution theory [2] is an important field in the so-called Universal Logic. It is a categorical abstract model theory, which formalizes the notion of logical system, including syntax, semantics and the satisfaction relation between them. One of the many achievements of Institution theory has been to provide a conceptually elegant and unifying definition of the nature of logical systems. It provides a complete form of abstract model theory, including signature morphisms, model reducts, mappings between logics noted as Institution - independent model theory.
We propose the concept of topological semantics at the level of abstract model theory provided by an institution-independent framework. Our abstract topological logic framework provides a method for systematic topological semantics extensions of logical systems from computer science and logic. Also, provides us with several appropriate model theoretical tools for proving semantic completeness on arbitrary logics. Furthermore, we can show how to develop an extension of the institution-independent method of ultraproducts [5] to topological semantics, to topological satisfaction and develop an ultraproduct fundamental theorem for the modal satisfaction. Furthermore, is easy to show that this scheme consists of fundamentals notions of logics in topological framework, like Craig’s interpolation theorem, Completeness, Bisimulation, Filtration etc.
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INTUITION, INTELLIGENCE, DATA COMPRESSION

Session 16F

Congress section(s): C6

The main goal of this paper is to argue that data compression is a necessary condition for intelligence. One key motivation for this proposal stems from a paradox about intuition and intelligence. For the purposes of this paper, it will be useful to consider playing board games—such as chess and Go—as a paradigm of problem solving and cognition, and computer programs as a model of human cognition. I first describe the basic components of computer programs that play board games, namely value functions and search functions. A search function performs a lookahead search by calculating possible game continuations from a given board position. Since many games are too complex to be exhausted by search, game-playing programs also need value functions, which provide a static evaluation of a given position based on certain criteria (for example, space, mobility, or material). As I argue, value functions both play the same role as intuition in humans (which roughly corresponds to what is often called ‚System 1 processing‘) and work in essentially the same way. Increasingly sophisticated value functions take more features of a given position into account, which allows them to provide more accurate estimates about game outcomes, because they can take into account more relevant information. The limit of such increasingly sophisticated value functions is a function that takes all of the features of a given position into account and determines as many equivalence classes of positions as there are possible positions, thereby achieving perfect accuracy. Such a function is just a complete database that stores the game-theoretic values of all possible positions. Following Ned Block (1981), there is widespread consensus that a system that solves a problem by consulting a database—or, as in Block’s example, a ‚lookup table‘—does not exhibit intelligence.
This raises our paradox, since reliance on intuition—both inside and outside the domain of board games—is usually considered as manifesting intelligence, whereas usage of a lookup table is not. I therefore introduce another condition for intelligence that is related to data compression. According to my account, for a judgment or action to be intelligent, it has to be based on a process that surpasses both a certain threshold of accuracy and a certain threshold of compression. In developing this account, I draw on complexity measures from algorithmic complexity theory (e.g. Kolmogorov 1963). My proposal allows that reliance on a lookup table—even if it is perfectly accurate—can be nonintelligent, while retaining that reliance on intuition can be highly intelligent. As I explain, and as has been pointed out by researchers in computer science (e.g. Hernández-Orallo 2017), there are strong theoretical reasons to assume that cognition and intelligence involve data compression. Moreover, my account also captures a crucial empirical constraint. This is because all agents with limited resources that are able to solve complex problems—and hence, all cognitive systems—need to compress data.
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TOWARDS A NEW PHILOSOPHICAL PERSPECTIVE ON HERMANN WEYL’S TURN TO INTUITIONISM

Session 5K

Congress section(s): A4

Hermann Weyl’s engagement with Intuitionistic-based ideas can be ascribed to as early as 1910, when he started to consider constructive methods as a viable opponent to classical mathematics (Feferman 1998, 2000). The publication of “Das Kontinuum” (Weyl 1918) marked the high point of his intensive commitment to constructivism, which then matured into an intuitionistic approach in his renown paper “On the New Foundational Crisis in Mathematics” (Weyl 1921), up until the mid 1920’s when Weyl has retracted from intuitionism with a rapprochement to Hilbert’s axiomatic program (Beisswanger 1965).

From an historical perspective, Weyl is habitually described as a “wanderer” (Scholz 2004), traveling amid mathematical approaches as well as through philosophical fields, differing with the change in his scientific views. Most of the literature written about Weyl’s ‘conversion’ to intuitionism and vice-versa has offered causal explanations for his change of heart, such as philosophical stances adopted by Weyl from which he drew motivation to pose new scientific questions or to redirect his research orientation (van Dalen 1995). Such historical accounts, more often than not, assign to Weyl’s frequent change of mind an aspect of confusion deriving from his indecisiveness (Scholz 2000, Feferman 1988). However, these explanations have yet been able to account for the reasons behind Weyl’s indecisive acts, as well as his inability to undoubtfully convince other members from the scientific community. In order to comprehend the whole picture, we need to consider the way scientific transitions occur in general, and more specifically, how practitioners exchange ideas within the scientific trading zone (Galison 1997, Collins, Evans and Gorman 2007).

In the following paper I aim to introduce a different perspective, suggesting that Weyl’s feeling of ambivalence is the product of a rational process of self-criticism. Building on and away from Menachem Fisch’s model describing the intrapersonal process of becoming ambivalent towards a theory or a concept (Fisch 2010, 2011, 2017), I intend to bring forth the incentive Weyl had for considering new transformative ideas (both philosophical and mathematical) in the first place, arguing that he wasn’t a “fallen victim to a “blindfolding” feeling of evidence during his Fichtean and Brouwerian phase” (Scholz 2000, pp. 16) but a reasonable and active participant in an intrasubjective process of self-critical deliberation.

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A LOGICAL APPROACH TO NASH EQUILIBRIA

Session 15B

Congress section(s): A2

Maximizing expected utility is a central concept within classic game theory. It combines a variety of core aspects of game theory including the agent’s beliefs, preferences and their space of available strategies, pure or mixed. These frameworks presuppose quantitative notions in various ways, both on the input and output side. On the input side, utility maximizing requires a quantitative, probabilistic representation on the agent’s beliefs. Moreover, agents‘ preferences over the various pure outcomes need to be given quantitatively. Finally, the scope of maximizing is sometimes taken to include mixed strategies, requiring a quantitative account of mixed actions.

Also on the output side, expected utilities are interpreted quantitatively, providing interval scaled preference or evaluations of the available actions, again pure or mixed. In this contribution, we want to pursue qualitative, logical counterparts of maximal utility reasoning. These will build on two core components, logical approaches to games and to probabilities.

On the game side, the approach builds on a standard modal logic for n-player matrix games with modalities $[]_i$ and $\geq_i$ for all players, denoting their uncertainty over opponent’s choices and their preferences over outcomes respectively. This language is expanded with a mild form of conditional belief operators. Given that players are still to decide on their actions, agents cannot reasonably have beliefs about the likelihood of outcome strategy profiles. Rather, agents have conditional beliefs $p_i(\varphi|a)$, denoting i’s beliefs about which outcome states (or formulas) obtain(ed) if she is or was to perform action a. To see how such languages can express maximal utility reasoning, assume agent i‘s goals in some game to be completely determined by a formula φ: she receives high utility if the game’s outcome satisfies φ and low utility else. In this case, the expected utility of her various moves is directly related to their propensity of making φ true. More concretely, the expected utility of performing some action a is as least as high as that of b iff $p(\varphi|a) \geq p(\varphi|b)$.

Notably, the current approach is not restricted to cases where utility is determined by a single formula. We will show the formalism expressive enough to represent all utility assignments on finite games with values in the rationals. Moreover, we will show that the framework can be used to represent pure and mixed strategy Nash equilibria, again over finite games: For all combinations of rational valued utility assignments there is a formula expressing that an underlying game form equipped with the relevant probability assignments is in equilibrium with respect to these utilities. Lastly, we show the logical framework developed to be well-behaved in that it allows for a finite axiomatization and is logically compact.

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DEFINITIONAL IDENTITY IN ARITHMETIC

Session 28A

Congress section(s): A2, A3, C6, C8

Definitional identity is the relation that holds between two expressions that are identical by definition. It is natural to expect that the principles governing this relation will depend on the underlying language. In this talk I wish to consider the formalization of definitional identity in arithmetic and draw attention to a striking contrast between the language admitting only first-order terms and the language admitting terms also of higher order.

In the first-order case we may rely on Kleene (Introduction to Metamathematics, §54) and Curry (e.g. Combinatory Logic, ch. 2E). They formalize definitional identity as the equivalence relation inductively generated by all axioms of the form

definiens = definiendum

and the two rules of inference:

1. from a=b infer a[t/x]=b[t/x]
2. from a=b and c=d infer a=b[d/c]*

Here b[t/x] is ordinary substitution, and b[d/c]* is substitution of d for any number of occurrences of c in b.

There are two admissible forms of definition: explicit definition of an individual constant or a function constant and recursive definition of a function constant. A definitional equation for a function constant takes the form

f(x)=t[x]
where x is a sequence of variables. An explicit definition consists of one such equation, a recursive definition of two.
This relation can be shown to be the reflexive, symmetric, and transitive closure of a reduction relation that formalizes the process of unfolding defined terms in an expression. This relation may, in turn, be thought of as an evaluation relation. Indeed, one can show that it is confluent and strongly normalizing. The definitional identity a=b can in this case therefore be interpreted as “a and b have the same value”.
Now let us admit variables x and constants f of arbitrary type. Definitional equations for such f’s take the same form as before. In particular, the definiens is f(x) of type N. However, f may now occur isolated, for instance as argument to another function. Various considerations leads one here to postulate a further rule:
3. from f(x)=g(x) infer f=g, and vice versa.
One consideration in support of this rule is the following. Suppose I have defined f and g by
f(x)=t
g(x)=t‘
and it turns out that
t=t‘
namely that t and t‘ are definitionally identical. It is then natural to say that also f and g are definitionally identical,
f=g
But this example also shows that when rule 3 is admitted, then definitional identity can no longer be interpreted as “a and b have the same value”. Namely, in the example we have f=g, but both f and g are irreducible (only when they are supplied with arguments can a reduction take place).
One conclusion to draw from this is that although a reduction relation can be defined for higher-order expressions f, this relation cannot be thought of as evaluation. Evaluation makes good sense only after f has been supplied with arguments so that a ground-level expression results.

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MODELING BIOLOGICAL POSSIBILITIES IN MULTIPLE MODALITIES

Session 16E

Congress section(s): B4

A noticeable feature of contemporary modeling practice is the employment of multiple models in the study of same phenomena. Following Levins’ (1966) original insight, philosophers of science have studied multiple modeling through the notions of triangulation and robustness. Among the different philosophical notions of robustness one can discern, first, those that focus on robust results achieved by triangulating independent epistemic means, and, second, those that target the variation of the assumptions of a group of related mathematical models (Knuuttila and Loettgers 2011). The discussion of modeling has concentrated on the latter kind of robustness as the models being triangulated are typically not independent (c.f. Orzak and Sober 1993). Yet, the problem of robust results derived from related mathematical models sharing a common “causal” core is that they may all be prone to the same systematic error (Wimsatt 2007). One compelling strategy to safeguard against systematic errors is the construction of related models in different material modalities. This paper considers such multiple modeling practices through the cases of synthetic genetic circuits and minimal cells. While the various incarnations of these model systems are not independent by their design, they utilize independent media – mathematical, digital and material – thus mitigating the errors prone to using only one kind of modeling framework. Moreover, the combination of a related model design and independent representational media, also tackles the worries concerning inconsistent and discordant evidence (e.g. Stegenga 2009). The cases studied highlight also another important dimension of multiple modeling: the study of what is possible. While much of scientific modeling can be understood as an inquiry into various kinds of possibilities, the research practice of synthetic biology takes modeling beyond mere theoretical conceivability toward material actualizability.
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PREREQUISITE FOR EMPLOYING INTELLIGENT MACHINES AS HUMAN SURROGATE

Session 15G

Congress section(s): C5, C6, C8

This paper discusses qualifying conditions for employing and utilizing intelligent machines as human surrogate. It thereby induces us to philosophical reflection on the rationality of our own act of designing, manufacturing and utilizing such artifacts. So long as the conditions discussed here are not realized, recruitment and use of such system shall be advised as unreasonable, thus unacceptable.
Machines equipped with higher level of AI will surrogate the human role in ever-increasing range. Decision of the extent and appropriate mode of such surrogacy is an important societal task about which we should make decisions. For this purpose we should first analyze the question, what the prerequisite condition for acceptable robotic surrogacy is.
This paper discusses the primary condition for intelligent machines in order to be justified to surrogate human agents. Seen from the viewpoint of the analysis of this paper, it will be hard for a machine to satisfy the requirement. It suggests that our societies proceed more carefully with such intelligent artifacts than they do now.
This paper discusses the case of autonomous vehicles that are coming incorporated into our transportation system as primary example. An essential condition that a driver-AI should fulfill in order to legitimately take the driver role in our traffic system is that it assume and apply a perceptual taxonomy that is sufficiently near to that of the competent human drivers, so that it tends to make eventually the same classification of all the objects encountered while driving as the human drivers do.
How can we make the intelligent artifacts to share human systematics? We may consider two paths. One is the method of inputting the classification system we are adopting and applying into the program (top-down inscription), and the other is to let them interact with human beings in the real world and thereby learn it bit by bit (bottom-up learning). But neither way seems to be effective for realization of the goal. I will show why.
I suggested an ontological stance that interprets AI as a form of “externalized mind”(Ko 2012). Externalized mind is a boundary type of extended mind. The former differs from the latter in that 1) it does not belong to a specific cognitive agent, even though it might owe its origin to certain agent, and 2) it does not necessarily have a center for which the extended resources of cognition serve and supplement.
The AI that operates a networked system of unmanned vehicles, for instance, can be interpreted as the externalized collective mind of decision-makers about the system, of those engineers who perform design in details, engineers of manufacturing and implementing, and administrators of the system. This way of ontological interpretation stands against the philosophical stance that grants the status of independent agents to smart robots (e.g., in terms of the “electronic personhood”). I argue that it is a philosophical scheme adequate for addressing the issue of responsibility and contribution allocation associated with the application of the intelligent machines. From this perspective, I will stand with J. Bryson in the debate on the socio-ethical status of robots (Gunkel 2018 & Bryson 2018).
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TOP-DOWN INHIBITORY EXPERIMENTS: THE NEGLECTED OPTION

Session 10C

Congress section(s): C3, C5

In the new mechanistic literature, three types of inter-level experiments for determining constitutive relevance are frequently discussed. They differ from one-another with respect to the direction of influence (top-down versus bottom-up) and the type of intervention (excitatory or inhibitory). Both excitatory and inhibitory bottom-up experiments (stimulation and interference respectively) are recognised, but only excitatory top-down experiments (activation) are usually discussed. While the possibility of top-down inhibitory experiments is recognised by Craver (Explaining the Brain, Oxford: Clarendon, 2007) in a footnote, no full-scale treatment exists. This contribution rectifies the absence.
I argue that there are two distinct types of top-down inhibitory experiments: deprivation and cessation experiments. Deprivation experiments are the type of experiments mentioned in Craver's footnote. They involve large-scale lesions or disruptions to the system exhibiting the explanandum phenomenon, such that the phenomenon cannot manifest anymore. In Craver's example, researchers lesioned test animals' eyes and observed altered development of brain cortex in absence of visual stimulation. I argue that these experiments are of limited utility, since such changes to the system will inevitably affect the occurrence of many phenomena exhibited by the system. When changes in the operation of the system's components are observed, it is then impossible to match the observed changes to the disruption of the phenomenon of interest as opposed to any of the other phenomena disrupted. For instance, the experiment above disrupts the phenomena of object recognition, as well as the phenomenon of pupil dilation, and many others. Craver claims that these experiments rarely occur. I conclude that even when they do occur they are not used as inter-level experiments to infer constitutive relevance relations, but for merely exploratory purposes.
A type of top-down inhibitory experiments not discussed in the literature are what I call cessation experiments. They involve manipulating the conditions in which the system operates so that the phenomenon ceases. I argue that this is significantly different from those activation experiments, in which a group of systems exhibiting the phenomenon are compared to a group of subjects which do not exhibit the phenomenon. The difference arises because cessation experiments provide information about what would happen if a phenomenon were made to stop. This can differ from what happens if a phenomenon is not exhibited at all, for example in pathological cases. Furthermore, cessation experiments can be useful as an alternative to activation experiments for phenomena with unknown, or highly disjunctive set-up conditions. For instance, cessation experiments can be used to investigate the mechanisms behind involuntary cognitions in healthy subjects. I discuss the case of involuntary musical imagery, which can be triggered in a variety of circumstances, but not reliably, and with significant intersubjective differences. This makes activation experiments on involuntary musical imagery impractical, since the phenomenon cannot be reliably induced. However, once the phenomenon manifests, it tends to persist for some time, and a number of intervention techniques are being developed for stopping the involuntary imagery. If these are combined with an imaging paradigm, a cessation experiment can be performed.

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ON TWO KINDS OF CONCEPTUAL ENGINEERING AND THEIR METHODOLOGICAL COUNTERPARTS

Session 17C

Congress section(s): B1

'Conceptual Engineering' is the name of a method which aims to revise rather than describe our representational devices. Current metaphilosophical debates about conceptual engineering (CE) have uncovered a variety of important foundational issues about this method, such as its compatibility with semantic externalism or how the original topic can be preserved through the process of engineering. Surprisingly, however, recent debates have rarely touched upon a question that seems central for the project of CE, namely on what kind of representation – e.g. concepts, lexical items, or conceptions – this method operates on. I will argue that answering this question is not only relevant for developing an adequate metatheory of CE, but also has dramatic consequences for its actual implementation.
In my talk, I will begin with a critical discussion of two extant attempts at answering it. According to Cappelen (2018), CE is about words instead of concepts. The engineer's goal is thus to change the meaning of lexical items. I will argue that this proposal is wanting, for once we take into account that word meanings are determined by facts external to the minds of their users, this picture of CE does not sit well with the idea that CE aims to bring about a change in how people mentally categorize the world – an idea I take to be central to almost any actual CE project. According to Machery (2017), CE is about psychological concepts. I will argue that this proposal helps to remedy the problems of the first, as psychological concepts arguably play an important role in how we mentally categorize our environment. Ultimately, however, this proposal is wanting in that it does not show how such concepts relate to language. This is worrisome because changing lexical meanings is crucial to many actual CE projects.
In the second part of this talk, I will sketch how a more comprehensive account of CE could look like. The basic idea of my proposal is that concepts have a dual character, i.e., that they are constituted by both semantic and non-semantic features. Whereas the semantic features determine meaning and reference, the non-semantic features play a role particularly in one's mental categorization. I will substantiate this view by linking it to various extant discussions in the literature about concepts. The upshot for CE is that due to the dual character of concepts, there are also two fundamentally different kinds of conceptual amelioration: psychological and referential. Whereas referential engineering is achieved by establishing new causal relations between a concept and an object, psychological engineering requires changing one's dispositional profile. These two projects have different success conditions and suggest different strategies for their actual implementation. Nonetheless, my proposal allows to see them as two aspects of a unified and broadly applicable methodological framework.
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HOW VIRTUE SIGNALLING MAKES US BETTER: MORAL PREFERENCE OF SELECTION OF TYPES OF AUTONOMOUS VEHICLES.

Session 14G

Congress section(s): C8

In this paper, we present a study on moral judgement on autonomous vehicles (AV). We employ a hypothetical choice of three types of "moral" software in a collision situation ("selfish", "altruistic", and "aversive to harm") in order to investigate moral judgement beyond this social dilemma in the Czech population we aim to answer two research questions: Whether the public circumstances (i.e. if the software choice is visible at the first glance) make the personal choice "altruistic" and what type of situation is most problematic for the "altruistic" choice (namely if it is the public one, the personal one, or the one for a person's offspring).
We devised a web-based study running between May and December of 2017 and gathered 2769 respondents (1799 women, 970 men; age IQR: 25-32). This study was a part of research preregistered at OSF before start of data gathering.
The AV-focused block of the questionnaire was opened by a brief information on AV and three proposed program solutions for previously introduced "trolley problem like" collisions: "selfish" (with preference for passengers in the car), "altruistic" (with preference for the highest number of saved lives), and "aversion to harm" (which will not actively change direction leading to killing a pedestrian or a passenger, even though it would save more lives in total). Participants were asked the following four questions: 1. What type of software would you choose for your own car if nobody was able to find out about your choice ("secret/self"). 2. What type of software would you choose for your own car if your choice was visible at the first glance ("public/self"). 3. What type of software would you choose for the car of your beloved child if nobody was able to find out ("child"). 4. What type of software would you vote for in secret in the parliament if it was to become the only legal type of AV ("parliament").

The results are as follows, test of independence was performed by a chi square: “Secret/self”: “selfish” (45.2 %), “altruistic” (45.2 %), “aversion to harm” (9.6 %). “public/self: “selfish” (30 %), “altruistic” (58.1 %), “aversion to harm” (11.8 %). In public choice, people were less likely to choose selfish software for their own car.
“Child”: “selfish” (66.6 %), “altruistic” (27.9 %), “aversion to harm” (5.6 %). A vote in parliament for legalizing single type: “selfish” (20.6 %), “altruistic” (66.9 %), “aversion to harm” (12.5 %) In choice of car for one’s own child people were more likely to choose selfish software than in the choice for themselves.
Based on the results, we can conclude that the public choice is more likely to pressure consumers to accept the altruistic solution making it a reasonable and relatively cheap way to shift them towards higher morality. In less favourable news, the general public tends to heightened sensibility and selfishness in case of one’s own offspring, and a careful approach is needed to prevent moral panic.

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FRAMES – A NEW MODEL FOR ANALYZING THEORIES

Session 15I

Congress section(s): B1

The frame model was developed in cognitive psychology (Barsalou 1992) and imported into the philosophy of science in order to provide representations of scientific concepts and conceptual change (Andersen and Nersessian 2000; Andersen et al. 2006; Chen and Barker 2000; Chen 2003; Barker et al. 2003; Votsis and Schurz 2012; Votsis and Schurz 2014). The aim of my talk is to show that beside the representation of scientific concepts the frame model is an efficient instrument to represent and analyze scientific theories. That is, I aim to establish the frame model as a representation tool for the structure of theories within the philosophy of science.
In order to do so, in the first section of my talk, I will briefly introduce the frame model and develop the notion of theory frames as an extension of it. Further, I will distinguish between theory frames for qualitative theories, in which scientific measurement is based on nominal scales, and theory frames for quantitative theories, in which measurement is based on ratio scales. In two case studies, I will apply the notion of theory frames to a linguistic and a physical theory.
Section 2 contains a diachronic analysis of a qualitative theory by applying the notion of a theory frame to the pro drop theory of generative linguistics. In section 3, I will provide a frame-based representation of electrostatics, the laws of which contain quantitative theoretical concepts.
Based on the two case studies, I will argue that the frame model is a powerful instrument to analyze the laws of scientific theories, the determination of theoretical concepts, the explanatory role of theoretical concepts, the abductive introduction of a new theoretical concept, the diachronic development of a theory, and the distinction between qualitative and quantitative scientific concepts. I will show that due to its graphical character the frame model provides a clear and intuitive representation of the structure of a theory as opposed to other models of theory representation like, for instance, the structuralist view of theories.
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SIEBEL’S ARGUMENT AGAINST FITELSON’S MEASURE OF COHERENCE RECONSIDERED

Session 11L

Congress section(s): B2

This talk aims at showing that Mark Siebel’s (2004) counterexample to Branden Fitelson’s (2003) probabilistic measure of coherence can be strengthened and thereby extended to an argument against a large number of other proposals including the measures by Shogenji (1999), Douven and Meijs (2007), Schupbach (2011), Schippers (2014), Koscholke (2016) and also William Roche’s (2013) average mutual firmness account which has not been challenged up to now. The example runs as follows: There are 10 equally likely suspects for a murder and the murderer is certainly among them. 6 have committed a robbery and a pickpocketing, 2 have committed a robbery but no pickpocketing and 2 have committed no robbery but a pickpocketing.
Intuitively speaking, the proposition that the murderer is a robber and the proposition that the murderer is a pickpocket are quite coherent in this example. After all, there is a large overlap of pickpocketing robbers. However, as Siebel has pointed out, Fitelson’s measure indicates that they are not. Siebel’s example is compelling. But it shows us much more. First, for any two propositions φ and ψ under a probability function P such that $P(\neg\varphi\wedge\neg\psi) = 0$ which is the case in Siebel’s example, any measure satisfying Fitelson’s (2003) dependency desiderata is unable judge the set $\{\varphi,\psi\}$ coherent, even in cases where it should. As already mentioned, this includes the measures proposed by Fitelson, Shogenji, Douven and Meijs, Schupbach, Schippers, Koscholke and many potential ones. Second, it can be shown that under a slightly stronger constraint, i.e. for any two propositions φ and ψ under a probability function P such that $P(\neg\varphi\wedge\neg\psi) = P(\varphi\wedge\neg\psi) = 0$, Roche’s average mutual firmness account is unable to judge the set $\{\varphi,\psi\}$ incoherent, even in cases where it should—this can be motivated by slightly modifying Siebel’s example. These two results suggest that the aforementioned proposals do not generally capture coherence adequately.
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BIG DATA IN LIFE SCIENCES

Session 27A

Congress section(s): C6

In this talk, I am going to discuss differences between ways in which data for Big Data Analysis is gathered in the context of business and Life Sciences context especially in the medical biology projects. Since both size and complexity of experimental projects in life sciences is varied, I would like to focus on big interdisciplinary projects that usually combine different testing methods.
In business the process usually starts with collecting as much information as possible. Only then people try to determine what can be inferred from the data, forming assumptions upon which the subsequent analysis is carried out. In Life Sciences operating model is different: it starts with planning what information a scientist needs to collect in order to get the answer for the scientific question. Moreover, scientists usually have limited budget for their broad experimental projects and collecting of each and every information cost. For that reason the scope of collected information, as well as type and size of the study group, should be carefully planned and described. Furthermore, in medical sciences the cooperation between a number of medical and scientific units is crucial. Because of that one often has to deal with data collected by different teams, using various methods and different storage formats (not all of them being digital). Thus, data in life sciences is not only big, varied and valuable, but also tends to occupy large space in laboratories and archives.
Only recently scientists got at their disposal high-throughput genomic technologies that enable the analysis of whole genomes or transcriptomes originating from multiple samples. Now they are able to correlate these data with phenotypic data such as biochemical marks, imaging, medical histories etc.
Some of the challenges in that endeavor are: choosing the best measurement methods that can be used by different people or teams and collecting the most reliable data. Later there comes a problem of digitizing the results of measurements and combining them with the other data. Furthermore genomic experiment tend to result in creating huge files of raw data that need to be analyzed using specific algorithms. It is not obvious what should be done with those raw data after the analysis. Should they be saved, because there is a chance for a better analyzing algorithm in the future? Should they be deleted, to make room for future data? Should they be shared in some commonly accessible databases?
Life Science is developing rapidly, bringing about spectacular discoveries. Yet scientists are often afraid of Big Data, even though they deal with it very often. In my opinion there is a need for discussion resulting in development of guidelines and standards for collecting diverse types of scientific data, combining and analyzing them in a way that maximizes the reliability of results.

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FACTICITY OF UNDERSTANDING IN NON-CAUSAL EXPLANATIONS

Session 12K

Congress section(s): B1

In the literature on scientific explanation, understanding has been seen either as a kind of knowledge (Strevens 2008, 2013; Khalifa 20117; De Regt 2015) or as a mental state that is epistemically superfluous (Trout 2008). If understanding is a species of knowledge an important question arises, namely, what makes the knowledge from understanding true?
In causal explanations, the facticity of understanding is conceived in terms of knowing the true causal relations (Strevens 2008, 20013).
However, the issue about facticity is even more conspicuous in non-causal explanations. How to conceive of it in non-causal explanations if they don't appeal to causal, microphysical or in general ontic details of the target system?
I argue that there are two ways to conceive the facticity of understanding in a particular type of non-causal explanation, i.e. the topological one. It is through understanding "vertical" and "horizontal" counterfactual dependency relations that these explanations describe.

By "vertical", I mean counterfactual dependency relation which describes dependency between variables at different levels or orders in the mathematical hierarchy.
These are explanatory in virtue of constraining a range of variables in a counter-possible sense, i.e. had the constraining theorem been false it wouldn't have had constrained the range of object level variables. In this sense, the fact that a meta-variable or a higher order mathematical property holds entails that a mathematical property P obtains in the same class of variables or operations (Huneman 2017: 24).
An example of this approach would be an explanation stability of an ecological community. Species and predation relations between them can be modeled as a graph which can have a global general network property of being a "small-world". The fact that the small-world property holds for that system constrains various kinds of general properties, e.g. the stability or robustness (Huneman 2017: 29).
On the other hand, by "horizontal" I mean the counterfactual dependency relations that are at the same level or order in the mathematical hierarchy. An example of "horizontal" counterfactual dependencies relations are the ones that hold between the topological variables such as the node's weighted degree or the network communicability measure and the variables that describe system's dynamics as a state space.
Factivity in vertical cases is easy to understand, it's basically a proof or an argument. It can be laxed or stricter. A laxed version is in terms of soundness and validity of the argument, a stricter sense is in terms of grounding (Poggiolesi 2013).
Factivity in horizontal cases is a bit more difficult to pin down. One way would be through possible world analysis of counterfactual dependency relations that the explanations describe. In this sense, the facticity has a stronger form which is germane to the notion of necessity.
The remaining question is whether this account can be generalized beyond topological explanation. I certainly think that at the very least it should be generalizable to all horizontal varieties of non-causal explanations.

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THE NASCENCY OF LUDWIK FLECK'S POLEMICS WITH TADEUSZ BILIKIEWICZ

Session 17L

Congress section(s): B6

The debate between Fleck and Bilikiewicz—a historian and philosopher of medicine—took place shortly before the outbreak of WWII and remained virtually unnoticed until 1978; some slightly wider recognition of their exchange was, however, possible only when English (Löwy 1990) and German (Fleck 2011) translations appeared.
Basically, the polemics concerns understanding of the concept of style and the influence of the environment on scientific activity as well as its products and it starts as a review of Bilikiewicz's book (1932) where the historical account of the development of embryology in early and late Baroque was interwoven with (at times) bold sociological remarks. The commentators of the debate were quick to notice that the claims made by Fleck at that time are crucial for understanding of his position—esp. because they support its non-relativist reading. While the importance of the controversy was univocally acknowledged, its assessments so far have been defective for two reasons. First, for decades the views of Bilikiewicz were known only from the short and rather critical presentation given by Fleck and this put their discussion into an inadequate perspective. Second, for over 40 years it remained a complete puzzle how this symposium originated. This paper aims to close these gaps.
Thus, on the one hand, it is to indicate the gist of the disputation between Fleck and Bilikiewicz within the context of Bilikiewicz's views; on the other one—and more importantly—it is to show its genesis basing on recently discovered and unpublished archival materials. For the preserved correspondence of theirs gives an opportunity to advance some hypotheses about the aims and hopes tied to the project but also about its failure.
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ANTHROPOCENTRISM IN SCIENCE

Session 11J

Congress section(s): C2

According to the Encyclopedia on the Rights and Welfare of Animals, Anthropocentrism relates to any idea which suggests central importance, superiority and supremacy of man in relation to the rest of the world. Anthropocentrism denotes also that the purpose of Nature is to serve human needs and desires, based on the idea that man has the highest value in the world. (Fox, 2010) Even if anthropocentrism can be seen as a concept fitting mainly in the field of Environmental Ethics, we could say that it can be considered as a concept connected also with Science, as being a part of the scientific outlook to the world. Even if we claim that the scientific outlook is objective and not subjective, provided that this parameter is controllable, are we at the same time in the position to assert that our view of the world is free of anthropocentrism?

The branches of science which are more vulnerable to such a viewpoint, as their name may indicate, are the Humanities as they focus on man and the achievements of human culture. Such an approach is not expected by the so-called positive sciences. Nevertheless, the anthropocentric outlook is not avoided entirely. An example of this in Cosmology is the noted Anthropic Principle.

The main idea of the Anthropic principle, as we know it, is that the Universe seems to be “fine-tuned” in such a way, in order to allow the existence of intelligent life that can observe it. How a philosophical idea of “old cutting” such as the “intelligent design of the Universe” can intrude into the modern scientific outlook and why it is so resilient?

In my presentation, I will attempt to present briefly the anthropic principle and to answer the questions mentioned above. In addition, I will try to show how anthropocentrism contradicts with the human effort to discover the world. Also I will refer to the consequences of Anthropocentrism for Ethics and Science itself.

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CRITICAL THINKING AND DOXASTIC COMMITMENTS

Session 5L

Congress section(s): A2

Following Krister Segerberg (“Belief revision and doxastic commitment”. Bulletin of the Section of Logic, 27(1–2), 43–45.) we consider the doxastic commitments of an agent as a subset the initial belief set. Segerberg considers a “complex” to be a pair (V, T) where V and T are theories in some given language and with respect to some given logic L such that $V \subseteq T$. The only primitive operation on complexes is the operation of revision $*$. Informally, T represent a belief set and V – a set of doxastic commitments. In Segerberg’s presentation doxastic commitments are treated as irrevocable under a revision operator. Thus, they remain stable and secure an epistemic fallback — the set by which one can always return the initial consistent belief set if needed. By using fallbacks one can define a specific operation of “irrevocable revision” over pairs (V, T). A sentence p being added to a belief set by the irrevocable revision cannot be removed and gets a knowledge status. Since the elements of a fallback have a knowledge status, they are subject to the “persistence property”: $Kp \rightarrow [*q]Kp$. In this sense the doxastic commitments always remain in the fallback. In particular, the sets of logical tautologies, and of the fundamental axioms for T always belong to V.

Thus, every pair (V, T) is ordered under irrevocability and their components are not equivalent by default. Moreover, changes in belief set T are accomplished by the rules of set V, and thus, V can be considered a set of pure epistemic commitments which holds for a strictly organized theory and idealized rational agent.

By considering the agent’s ability to critical thinking one can relate it to the taken doxastic commitments. Taking into account that critical thinking normally (1) relies upon criteria, (2) is self-correcting, and (3) sensitive to context (Lipman, M. “Critical thinking – What can it be?”. Educational Leadership, 1988, 46(1), 38-43.), one can observe that the set V as a set of the pure epistemic commitments is subject only to (1). To ensure compliance with (2) and (3) one should be able to model a dynamic character of the revision procedures. To this effect one should provide a certain feedback between T and V. This can be achieved by explicitly introducing doxastic component (as a context sensitivity) in the commitment set and specify certain interrelations between this component and the belief set.

In this way one can discriminate between “universal commitments” and “contextual commitments” by extending Segerberg’s construction by an additional set C, and considering a complex to be a structure (V, C, T), where C is the set of contextual commitments. The complex can be then ordered by the revision operation. If some belief set $C \subseteq T$ turns out to be successful in the sense that any execution of a revision operation under T saves the set C, then C can be added to the commitment set V. Thus, as soon as V is subject to the irrevocable revision it can be considered to be a set of doxastic commitments, although it may give rise to several independent fallbacks.

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THE PROBLEM OF FIGURATIVENESS IN SCIENCE: FROM COMMUNICATION TO THE ARTICULATION OF SCIENTIFIC KNOWLEDGE

Session 11H

Congress section(s): B1, B2

In understanding the essentially rhetorical character of science, special attention should be paid to the place of figurativeness in research discourse. The central question of my presentation is whether it is possible in the absence of figurativeness to produce radically different meanings that will transform the conceptual space of science.

In most cases, the role of figurativeness is reduced to the optimisation of knowledge transmission. The expressive power of language is meant to make the transmitted knowledge more accessible for another and, in the best case, to help to ‘transplant it [knowledge] into another, as it grew in [one’s] own mind’ (Bacon). One of the rhetoric elements most widely used in research discourse, the metaphor often becomes an irreplaceable vehicle, for it makes it possible to create an idea of the object, i.e. to generate a certain way to think about it. The use of figurative elements in scientific language translates both in communicative optimisation and, owing to the uniqueness of the interpretation process, in the discovery of new ways to understand the object.

However, the role of figurativeness in research discourse is not limited to knowledge transmission. Despite the significance of communication (i.e. either explicit or implicit inclusion of another into the creative process of the self) for the development of a scientific ontology, the major source of intention is the cognising subject. Thus, in considering the role of figurativeness in scientific discourse, the focus should be shifted from the concept of communication to that of articulation, in other words, from another to the self.

The function of figurativeness as a tool for the ‘articulation’ of knowledge is determined by the features of the scientific ‘articulation’ per se. The central question of the presentation can be supplemented with that whether it is possible to ‘capture’, to register the meaning-assigning synthesis of the elusive ‘actualities of consciousness’ (Husserl) beyond figurativeness. This concerns the ‘act of thought’ that unlocks the boundaries of meaning conventions and thus transforms scientific ontology. One can assume that the answer to this question is ‘no’. In this presentation, I will put forward arguments in support of this answer.

To build and develop a theoretical model, the mere abstraction of the object is not sufficient. There is a need for a constant ‘live’ interest in the object, i.e. the persistent imparting of significance to it, accompanied by the segregation of the object

from the existing ontology. Although not realized by the author, the mechanisms of figurativeness may assume this function and make it possible to isolate and 'alienate' the object, thus granting it the 'intellectualised' status and making it 'inconvenient'. Always beyond the realm of convenience, figurativeness by default transcends the existing conceptual terrain. Sometimes, it refutes any objective, rationalised convenience. It even seems to be aimed against conveniences. It means an upsurge in subjectivity, which, in the best case, destroys the common sense of things that is embedded in the forms of communicative rationality. From this point of view, figurativeness is an essential feature of the 'articulation' of scientific knowledge.

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POPPER AND MODERN COSMOLOGY: HIS VIEWS AND HIS INFLUENCE

Session 13C

Congress section(s): B6

Karl Popper only commented on modern cosmology at a few occasions and then in general terms. His only paper on the subject dates from 1940. Nonetheless, his philosophy of science played a most important role in the epic cosmological controversy that raged from 1948 to about 1965 and in which the new steady-state theory confronted the evolutionary cosmology based on Einstein's general theory of relativity. The impact of Popper's philosophical views and of his demarcation criterion in particular is still highly visible in the current debate concerning the so-called multiverse hypothesis. In astronomy and cosmology, as in the physical sciences generally, Popper's views of science – or what scientists take to be his views – have had much greater impact than the ideas of other philosophers. The paper analyses the interaction between Popper's philosophy of science and developments in physical cosmology in the post-World War II era. There are two separate aspects of the analysis. One is to elucidate how Popper's philosophical ideas have influenced scientists' conceptions of the universe as a whole. The other aspect is to investigate Popper's own views about scientific cosmology, a subject he never dealt with at any length in his publications. These views, as pieced together from published as well as unpublished sources, changed somewhat over time. While he had some sympathy for the now defunct steady-state theory, he never endorsed it in public and there were elements in it which he criticized. He much disliked the big bang theory, which since the mid-1960s has been the generally accepted framework for cosmology. According to Popper, the concept of a big bang as the beginning of the universe did not belong to science proper. Generally he seems to have considered cosmology a somewhat immature science.

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WHAT IS PROOF COMPLEXITY?

Session 6E

Congress section(s): A3

A mathematical proof may be complex and hard to follow. Which aspect of the proof causes the hardness is subjective. It may use unfamiliar notions or esoteric logic, or it may just assume too much background knowledge. It may also be quite long and even if it is parsed into many simple steps, or many simple cases to consider separately, one may get lost.
Proof Complexity is a mathematical area which links the intuitive notion of complexity of proofs with mathematical notions from computational complexity theory. A proof is complex if it requires a lot of time to verify using some predefined formal criteria. It is one of the richest areas connecting mathematical logic with computational complexity theory. This connection has many facets and I will try to explain some of them. In particular, I shall consider the fundamental question of whether the complexity of computations and the complexity of proofs are essentially different phenomena or whether one can be in some sense reduced to the other.

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HOW EARLY HUMANS MADE THE SCIENCES POSSIBLE

Session 7H

Congress section(s): B6

The common view is that the sciences emerged largely since the 17th century in Europe (e.g. Cromer 1995; Logan 1986). That standard view of science has been maintained because, in studying the question of the origin and foundations of the sciences, researchers have focused commonly on proximate factors (or causes) as explanations: including the printing press, the work of Bacon, Galileo and Newton, and the like. Some scientists and philosophers have looked deeper than the proximate factors since the 17th century by attempting to identify the underlying foundational factors that have allowed us to develop the sciences in the first place. Leading cognitive scientists such as Daniel Dennett (1991) argues that a massive reprogramming of the mind made the sciences possible. Leading evolutionary psychologists Pinker (2010) and Cosmides and Tooby (2013) have stressed features of the mind such as our ability to use our folk physics to deal with the world around us. I however provide a methodological account of the fundamental factors that have helped make the sciences possible, focusing in particular on our biological and cognitive capacities for systematic observation, problem-solving, basic experimentation etc. which we largely developed before the Paleolithic.

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POSITIVISATION OF POLITICAL PHILOSOPHY AND ITS IMPACT ON THE WHOLE DISCIPLINE

Session 8G

Congress section(s): B1, B4, B5

For the past decade, we have witnessed the outburst of discussions concerning the methods of political philosophy (mostly its prevailing analytical branch) resulting in the emergence of the first propaedeutic literature systematizing and

summarizing existing methodological approaches and frameworks (see Blau 2018, List and Valentini 2016, Leopold and Stears 2008). To understand the cause and the possible impact of these discussions, we need to perceive them in the broader context given by the problematic position of political philosophy within the positivist-oriented political science. The paper presented aims to outline such context and explain how similar tendencies – such as positivisation of political philosophy - might lead to the limitation of its epistemological scope and critical capacity.

One of the features that political philosophy incorporated from the positivist-oriented science is the urge for validation of its normative outcomes. This fact has led to the development of the frameworks and methods aiming at the assessment of external validity of the normative theories that is linked to the positivist presumption of epistemological objectivism. One of the methods developed for these purposes was the reflective equilibrium that is nowadays being considered as the most widely used method in the moral and political philosophy (Varner 2012: 11 in Knight 2017: 46, Rawls 1999, 15–18, 40–46; Daniels 2013 in List, Valentini 2016: 17). However, the reflective equilibrium has been widely criticized for its effects, such as the avoidance of controversial questions that are mostly linked to the metaphysics, meta-ethics, and religion (Norman 1998: 284), as well as its interconnection to the liberal paradigm (especially the Rawlsian strand). The paper presented will outline how reflective equilibrium produces such normative outcomes and how (unintentionally) limits the critical capacity of the political philosophy since it serves as an affirmative framework that presupposes its own conclusions rather than a framework that would provide an objective assessment of various normative theories and principles. Finally, the paper will critically discuss the possibility of the objective assessment of normative theories regarding the fact that the subject of the biggest reasonable disagreement in the society and political philosophy is the question of objectivity itself (see e. g. Nussbaum 2001: 890).

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INDUCTIVELY APPROACHING A PROBABILISTIC TRUTH AND A DETERMINISTIC TRUTH, THE LATTER IN COMPARISON WITH APPROACHING IT IN A QUALITATIVE SENSE.

Session 6D

Congress section(s): B2

Theories of truth approximation almost always deal with approaching deterministic truths, either actual or nomic, and have a Popperian background. E.g. Nomic truth approximation revisited (Kuipers, 2019), henceforth NTAR, deals primarily with qualitatively approaching deterministic nomic truths, based on the hypothetico-deductive method.

In contrast, approaching probabilistic truths can naturally be based on inductive logic or inductive probability theory (Kuipers, 1978). Assume e.g. random sampling with replacement in a universe conceptualized in a finite partition. The primary problem of verisimilitude is the logical problem of finding an optimal definition. In the present context this amounts to an optimal definition of the distance between any probability distribution and the true distribution. There are some plausible standard measures. However, the epistemic problem of verisimilitude is at least as interesting: what is a plausible distribution to start with, and how to update it in the light of empirical evidence and non-empirical characteristics such that convergence to the true distribution takes place.

Carnapian systems converge to the corresponding true probabilities, the probabilistic truth, in an inductive probabilistic way, starting from equal probabilities or, in generalized form, starting from some other well-argued prior distribution. Hintikka systems add to this the inductive probabilistic convergence to the true constituent, i.e. the deterministic truth about which conceptual possibilities actually exist (or are nomically possible), starting from some well-argued prior distribution over the constituents. Hence, if applied in the random sampling context, both types of systems can be reconstructed as inductively approaching a probabilistic truth and, in the Hintikka-case, in addition inductively approaching a deterministic truth.

The plausible connecting question is whether Hintikka systems can in addition be conceived as extensions or concretizations of qualitatively approaching the deterministic truth? Let U be the set of conceptual possibilities and T the true subset of instantiated conceptual possibilities and CT the corresponding true constituent. Let T⊂W⊂V⊆U with corresponding constituents CT, CW, and CV. Everybody will agree that CW is closer to the truth CT than CV. According to NTAR the (qualitative) ‘success theorem’ says that, CW will be at least as successful as CV if ‘at least as successful’ is defined in a certain qualitative way. By analogy, in the sketched probabilistic context it holds, under some plausible parameter conditions, that CW is ‘probabilistically at least as successful’ as CV in the sense that $p(en|CW) \geq p(en|CV)$, where en is any possible sequence of ‘outcomes’ of n random drawings, i.e. any en belonging to the Cartesian product Tn. The remaining question is whether ‘probabilistically at least as successful’ can be seen as a concretization of ‘qualitatively at least as successful’.

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ABILITY AND KNOWLEDGE

Session 12B

Congress section(s): A2

Imagine that I place all the cards from a deck face down on a table and ask you to turn over the Queen of Hearts. Are you able to do that? In a certain sense, yes – this is referred to as causal ability. Since you are able to pick any of the face-down cards, there are 52 actions available to you, and one of these guarantees that you turn over the Queen of Hearts. However, you do not know which of those 52 actions actually guarantees the result. Therefore, you are not able to turn over the Queen of Hearts in the epistemic sense. I explore this epistemic qualification of ability and three ways of modelling it.

I show that both the analyses of knowing how in epistemic transition systems (Naumov and Tao, 2018) and of epistemic ability in labelled STIT models (Horty and Pacuit, 2017) can be simulated using a combination of impersonal possibility, knowledge and agency in standard epistemic STIT models. Moreover, the standard analysis of the epistemic qualification of ability relies on action types – as opposed to action tokens – and states that an agent has the epistemic ability to do something if and only if there is an action type available to her that she knows guarantees it. I argue, however, that these action types are dispensable. This is supported by the fact that both epistemic transition systems and labelled STIT models rely on action types, yet their associated standard epistemic STIT models do not. Thus, no action types, no labels, and no new modalities are needed.

Epistemic transition systems as well as labelled STIT models have been noticeably influenced by the semantics of ATL. In line with the ATL tradition, they model imperfect information using an epistemic indistinguishability relation on static states or moments, respectively. In the STIT framework this implies that agents cannot know more about the current moment/history pair than what is historically settled. In particular, they cannot know anything about the action they perform at that moment/history pair. This is at odds with the standard epistemic extension of STIT theory which models epistemic indistinguishability on moment/history pairs instead.

The main benefit of using the standard epistemic STIT models instead of epistemic transition systems or labelled STIT models is that they are more general and therefore provide a more general analysis of knowing how and of epistemic ability in terms of the notion of knowingly doing.

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ON TAKEUTI'S VIEW OF THE CONCEPT OF SET

Session 6K

Congress section(s): A4

Gaisi Takeuti occasionally talked about the concept of set. Based on some remarks from him on it, we can see that Takeuti had a highly original view of it. However, Takeuti gave only scattered remarks on it and no sufficiently systematic development of his own view of it in its entirety. In this talk, we try to put together Takeuti's remarks on the concept of set, most of which are currently available only in Japanese ([1], [2], [3], [4], etc.), and to reconstruct Takeuti's view of the concept in a manner as systematic as possible.

Takeuti's view of the concept of set can be summarized by fundamental points. First, one of such points is concerned with the relationship between logic and the concept. Takeuti seems to have had a somewhat uncommon view of how logic is related to it. Although most set-theorists seem to consider separately (first-order) logic and the concept, Takeuti thought that a proof-theoretic analysis of higher-order predicate calculus, following the Hilbert-Gentzen formalist method, can provide an illumination of the nature of the concept of set.

Secondly, Takeuti suggested that the concept of set to be analyzed in this way is a sort of reification of the concept of proposition and claimed that such a concept of set is different from the one given in an axiomatic set theory, e.g., ZF, but it is closer to the concept of set introduced by the comprehension principle in higher- order predicate calculus. Although Takeuti had a view that ZF is very important from a technical viewpoint and did a lot of work related to ZF, apparently Takeuti thought that ZF is of limited foundational or philosophical interest, since it does not provide a desired intuition of the concept of set.

Thirdly, in some of his last works (e.g., [6]), Takeuti revisited the issue of set-theoretic paradoxes and expressed the view that set-theoretic paradoxes show that the universe of set is a growing universe, which looks similar to Michael Dummett's concept of the indefinitely extensible.

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WHAT MATURE LAKATOS LEARNT FROM YOUNG LAKATOS

Session 5G

Congress section(s): B6

Prior to his career in England, Imre Lakatos spent his post-university years in Hungary. Because of his well known involvement in the political turmoil of the Hungarian post-war era, his dubious political activity has been a focus of several studies and discussions. Much less is said about the philosophical content of his Hungarian writings. Apart from his often cited commitment to Hegelian roots in his philosophy of mathematics, as well as the usually vague references to the Marxist framework of his intellectual background, only a few details are known of his early adventures in philosophy.

After his graduation from the university of Debrecen (1945), and before his studies in Moscow (1949) and the following imprisonment, Lakatos published a relatively large number of papers on science and scientific education (for a partial list and summaries, see Kutrovátz 2008). The main purpose of these works was to develop a Marxist interpretation of scientific progress and to criticize improper (idealist, bourgeois) forms of scientific thought. Perhaps most notable of these are the two papers representing his (lost) doctoral dissertation of 1947 (for an English translation, see Lakatos 2002).

During his final years in Hungary Lakatos turned his interest to mathematics, and this line of research continued through his Cambridge years. He returned to philosophy of science in general only in the 1960s, developing his theory of the MSRP.

The fundamental question of this paper is whether there is anything at all that Lakatos in England shares with Lakatos in Hungary a decade earlier, or he started everything anew. Based on his wiritings and on unpublished material from the LSE Lakatos Archives, I propose that his intellectual framework in the post-war period was shaped by the following principles:

1. Science is a never ending dialectical process of conceptualizing nature.
2. Science and society are profoundly interconnected.
3. Science is not an ideology or world view.
4. In science, theory and practice are inseparable.
5. Science must be taken from capitalists and given to the proletariat.

It seems that items 1-3 remain unchanged among the commitments characterizing his post-immigration period. Ad 1, Proofs and refutations identifies this dialectical process in mathematics, while Popperian philosophy lends a new sense to the never ending nature of science. Ad 2, the Marxist background is replaced with sensitivity to the external/internal distnction and its contingency on rational reconstruction. Ad 3, the science/ideology dichotomy remains prevalent, with an additional interest in the demarcation problem. On the other hand, 4) weighs relatively less in his later writings, as he abandons explanations by 'modes of production', and 5) naturally disappears with his rejection of official communist ideology.

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A LOGIC FOR AN AGENTIVE NAÏVE PROTO-PHYSICS

Session 30J

Congress section(s): A3

We discuss steps towards a formalisation of the principles of an agentive naïve proto-physics, designed to match a level of abstraction that reflects the pre-linguistic conceptualisations and elementary notions of agency, as they develop during early human cognitive development. To this end, we present an agentive extension of the multi-dimensional image schema logic ISL based on variants of STIT theory, which is defined over the combined languages of the Region Connection Calculus (RCC8), the Qualitative Trajectory Calculus (QTC), Ligozat's cardinal directions (CD), and linear temporal logic over the reals (RTL), with 3D Euclidean space assumed for the spatial domain. To begin to formally capture the notion of "animate agent", we apply the newly defined logic to model the image schematic notion of "self movement" as a means to distinguish the agentive capabilities of moving objects, e.g. study how to identify the agentive differences between a mouse (an animate animal) and a ball (an inanimate yet causally relevant object). Finally, we outline the prospects for employing the theory in cognitive robotics and more generally in cognitive artificial intelligence and questions related to explainable AI.

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TRUTH, INCOHERENCE AND THE EVOLUTION OF SCIENCE

Session 8A

Congress section(s): B1

Thomas Kuhn argued that scientific development should be understood as an ever-continuing evolutionary process of speciation and specialization of scientific disciplines. This view was first time expressed explicitly in The Structure of Scientific Revolutions. Kuhn kept on returning to it until the end of his life. In his last published interview, Kuhn laments that "I would now argue very strongly that the Darwinian metaphor at the end of the book [SSR] is right and should have been taken more seriously than it was" (2000, 307).
However, in my paper I do not focus on the evolution of Kuhn's notion of evolutionary development of science as such, but study two of its significant consequences regarding scientific progress. The one is the resulting incoherence of science as a global cognitive venture. The other is the relation of incoherence with truth as an aim of science.
Kuhn remarked that "[S]pecialization and the narrowing of the range of expertise now look to me like the necessary price of increasingly powerful cognitive tools ... "[T]o anyone who values the unity of knowledge, this aspect of specialization ... is a condition to be deplored" (2000, 98). These words imply that the evolution of science gradually decreases the unity of science. Further, the more disunified science is, the more incoherent in total it is.
Kuhn rejected the idea that science converges on the truth of the world, or the teleological view of scientific development, in part because he saw it as a historically unsubstantiated claim. But is truth as an aim of science also conceptually incoherent in Kuhn? It seems evident that the evolutionary view of scientific development makes the goal of progressing towards the singular Truth with the capital T impossible. As Nicholas Rescher, for example, has argued, the true description of the world should form a maximally coherent whole or a manifestation of ideal coherence (Rescher 1973, 1985). But what if truth is seen as local, applicable in the specialized disciplines, so that they produce truths of the matters they are specialized in describing? Could science aim at producing a large set of truths about the world without the requirement of their systematic coherence? Science would be a collective of true beliefs without directionality or unity.
In brief, I study the relations between truth, incoherence and the evolution of science within the Kuhnian philosophical framework.
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INTERACTIVE TURING-COMPLETE LOGIC VIA GAME-THEORETIC SEMANTICS

Session 16B

Congress section(s): A2

We define a simple extension of first-order logic via introducing self-referentiality operators and domain extension quantifiers. The new extension quantifiers allow us to insert new points to model domains and also to modify relations by adding individual tuples. The self-referentiality operators are variables ranging over subformulas of the same formula where they are used, and they can be given a simple interpretation via game-theoretic semantics.
We analyse the conceptual properties of this logic, especially the way it links games and computation in a one-to-one fashion. We prove that this simple extension of first-order logic is Turing-complete in the sense that it exactly captures the expressive power of Turing-machines in the sense of descriptive complexity: for every Turing-machine, there exists an equivalent formula, and vice versa.
We also discuss how this logic can describe classical compass and straightedge constructions of geometry in a natural way. In classical geometry, the mechanisms of modifying constructions are analogous to the model modification steps realizable in the Turing-complete logic. Also the self-referentiality operators lead to recursive processes omnipresent in everyday mathematics.
The logic has a very simple translation to natural language which we also discuss.

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COMMUTATIVE TRANSFORMATIONS OF THEORY STRUCTURES

Session 4L

Congress section(s): B1

Reconstruction of scientific theories as complex [1], variable dynamic and coordinated polysystems [2] has been supported by the case studies of various actual theories [3].
In a scientific theory we distinguish subsystems with specific constitutive generative elements: names, languages, propositions, axioms, models, problems, operations, procedures, values, heuristics, approximations, applications etc. As subsystems of the same theory, they are intimately interrelated and a change of any element induces many changes both in its own and other subsystems. Note, that propositional and model-theoretic conceptions of a scientific theory consider its propositions and models as respective constitutive elements. The usage of the informal language of commutative diagrams [4] allows one to separate and classify various types of interconnected amendments of theory structures.
Let α: X -> Y symbolize a transformation (in many cases it represents a relation) of one component X of a theory T into another component Y of T. For instance, if Y is a model M from T, then X can be a certain language L used to construct M. Let μ be a certain homogeneous transformation X -> X*, such that there is the inverse transformation μ-1. Since all elements in question belong to T, μ induces the homogeneous transformation π: Y -> Y*. The set α, μ, π and ρ is commutative if the transformation ρ: X* -> Y* is such that $\rho = \mu\text{-}1\#\alpha\#\pi$, where # is a composition of transformations.
Factually, many real non-trivial transformations of theory elements are commutative. Let us, for example, reformulate some problem (P -> P*) in terms of a new model (M -> M*). We have here the set of four transformations 1) α: P -> M; 2) μ: P -> P*, 3) π: M -> M*; and 4) ρ: P -> P*, which will be commutative if $\rho = \mu\text{-}1\#\alpha\#\pi$.
Commutative transformation is T-internal, if all its constituents belong to the same theory (e.g., Le Verrier's resolution of problem of systematic discrepancies between Uranus's observed orbit and the one calculated from Newton classical mechanics through its reformulating in terms of the new model of Solar system that includes Neptune), and T-external, if some constituents belong to different theories (Bohr's resolution of the problem of atom instability in terms of his model of atom stationary orbits).
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ON THE RELATIONS BETWEEN VISUAL THINKING AND INSTRUMENTAL PRACTICE IN MATHEMATICS

Session 8C

Congress section(s): C1

Following the development of cognitive sciences over the past twenty years, several attempts have been made in the framework of the philosophy of mathematical practice to use the results of research in cognitive science in the interpretation of visual thinking in mathematics (see the papers in Mancosu, Jorgensen and Pedersen, eds. 2005). The most detailed among these attempts is the book of Marcus Giaquinto Visual thinking in mathematics: an epistemological study. Despite its many merits the book received a critical review (see Avigad 2009), which can be seen as a criticism of any attempts to use empirical results of cognitive science in the normative philosophy of mathematics. Even if criticisms of this sort are not rare (see Balaguer 1999, Lavine 1992, or Riskin 1994), it seems that they miss an important point—the use of representational tools in mathematics.
The aim of the paper will be to complement the cognitive interpretation of visual thinking in mathematics with its instrumental aspect. Pictures in mathematical texts are used as tools of visual representation. As such they have a semiotic dimension: in interpreting these pictures, conventions play a crucial role. These conventions often force us to interpret the picture in a way that is contrary to what is actually drawn on the paper. Thus the conventions require interpreting the intersection of two lines as a point even though we are looking at a small region. Such conventions establish the instrumental practice in which representations are subjected to manipulations and form the basis of logical inferences. In the framework of the instrumental practice the manipulations of representations and inferences made on their basis acquire a normative character. In the practice we distinguish a legitimate manipulation from an illegitimate one and a justified judgment from an unjustified one. The normative character of the instrumental practice is often ignored by the proponents of the cognitive approach to visual thinking and so comments of the critics are often justified.
We are convinced that the normative character of the visual representations is an integrative part of their use in mathematics practice. Recourse to this normative aspect of mathematical practice makes it possible to address the mentioned criticism.
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THEORY OF FORMALIZATION: THE TRACTARIAN VIEW

Session 12M

Congress section(s): A3

Logical formalization is an established practice in philosophy as well as in mathematics. However, the rules of this practice are far from clear.
Sainsbury(1991) and Epstein(1994) were among the first to specify criteria of formalization. Since Brun's detailed monograph (Brun(2004)) criteria and theories of formalization have been discussed intensively (cf., e.g., most recently Peregrin(2017)).

No single theory of formalization has emerged from this discussion. Instead, it more and more becomes clear that different theories of formalization involve different traditions, background theories, aims, basic conceptions and foundations of logic. Brun(2004) envisages a systematic and, ideally, automated procedure of formalizing ordinary language as the ultimate aim of logical formalization. Peregrin(2017) ground their theory on inferentialism. Like Brun, they try to combine logical expressivism with a modest normative account of logic by their theory of reflective equilibrium. In contrast, Epstein(1994) bases his theory of formalization on semantic and ontological foundations that are rather close to mathematical model theory. Sainsbury(1991) grounds the project of formalization within the philosophical tradition of identifying logical forms in terms of representing truth conditions. He identifies Davidson as the most elaborated advocate of this tradition. Davidson refers to Tarskian semantics and distinguishes logical formalization from semantic analysis. Sainsbury also assigns the Tractarian View of the early Wittgenstein to the project of identifying truth conditions of ordinary propositions by means of logical formalizations. In contrast to Davidson, however, Wittgenstein does not distinguish the project of formalization from a semantic analysis and he does not rely on Tarski semantics. Instead, Wittgenstein presumes semantics according to which instances of first-order formulas represent the existence and non-existence of logically independent facts.
In my talk, I will show that the Tractarian view can be spelled out in terms of a theory of formalization that provides an alternative to Davidson's account of what it means to identify logical forms and truth conditions of ordinary propositions. In particular, I will argue that Wittgenstein with his early abnotation envisaged an account of first-order logic that makes it possible to identify logical forms by ideal symbols that serve as identity criteria for single non-redundant conditions of truth and falsehood of formalized propositions. Instead of enumerating an infinite number of possible infinitely complex models and counter-models in first-order logic, ideal symbols provide a finite description of the structure of possibly infinitely complex conditions of truth and falsehood. I will define logical forms and criteria of adequate formalization within this framework. Furthermore, I will show how to solve (i) termination problems of the application of criteria of adequate formalization, (ii) the trivialization problem of adequate formalization (iii) the problem of the uniqueness of logical form, and (iv) the problem of a mechanical and comprehensible verbalization of truth conditions. All in all, I will argue that a theory of formalization based on the Tractarian view provides a consistent and ambitious alternative that can be utilized for a systematic and partly algorithmic explanation of conditions of truth and falsehood of ordinary propositions expressible within first-order logic.
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AS THURSTON SAYS

Session 6C

Congress section(s): C1

It is commonplace in the educational literature on mathematical practice to argue for a general conclusion from isolated quotations from famous mathematicians. We will critique this mode of inference.
The issue can be illustrated by, for example, the way philosophers have written about properties of proofs such as elegance and explanatory power. Much of this literature assumes that there is a consensus among mathematicians about which proofs are elegant or explanatory. Recently, Matthew Inglis and Andrew Aberdein subjected this assumption to an empirical test. They used a proof of the Sylvester-Gallai theorem taken from Aigner & Ziegler (2000) and asked mathematicians to judge the accuracy of twenty adjectives that might describe the proof. The mathematicians in this sample differed significantly among themselves on the qualities of this particular proof, for reasons that are not revealed by the data.

In another study with a similar design (Inglis, Mejia-Ramos, Weber & Alcock 2013), it was found that the mathematicians who responded did not agree when asked whether a given proof was valid, and those who judged it to be invalid gave three different reasons why it was not valid. In the same vein, a study by Weber and Mejia-Ramos (2019) shows that mathematicians disagree about exactly which inferences in a given proof in real analysis are rigorous.
These results from cognitive psychology concern the private reasoning of individual mathematicians in isolation. Mathematical practices are shared, however, and can be viewed as social patterns of behaviour that arise from interaction among mathematicians. That is not to say that interaction always leads to a single shared view of best mathematical practices; it often leads to starkly opposed camps. Therefore, to investigate the differences of opinion among mathematicians on the aspects of mathematical practice that interest philosophers and educators, we will also consider public disagreements on the relevant issues among mathematicians, whether they are expressing personal opinions or those of a defined like-minded group.
We will examine the career of one much cited and anthologised paper, WP Thurston's 'On Proof and Progress in Mathematics' (1994). This paper has been multiply anthologised and cited hundreds of times in educational and philosophical argument. Examination of this case will illuminate the use to which testimony from mathematicians has been put and the conditions that have been, or should have been, placed on making inferences from it.
The interesting question is not whether mathematicians disagree—they are human, so of course they do. The question is whether there is a researchable object, even as an ideal type, answering to the expression 'mathematical practice', to which testimony from mathematicians might give us access. We will argue that debate about the nature and purpose of mathematics (such as the Jaffe-Quinn debate that Thurston contributed to) is itself part of the practice. This conception allows us to retain mathematical testimony as a resource, while indicating how to use it critically. The paper will end with reflections on the usefulness of quotations from research mathematicians for mathematical education.

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WHY FIELD THEORIES ARE NOT THEORIES OF FIELDS

Session 20I

Congress section(s): C2

The story of field theory is often told as a tale of triumph. The field concept (it seems) is one of the most successful in all of science. It has survived, if not driven, all major physical revolutions of the past century and is now a central part of our most thoroughly tested theories.
The aim of this talk is to put forward a different narrative: while fields have proven to be extremely successful effective devices, physics has time and time again run into an impasse by reifying them and buying into a dualistic ontology of particles and fields.
On closer examination, the concept of fields as mediators of particle interactions turns out to be philosophically unsatisfying and physically problematic, as it leads in particular to problematic self-interactions. Contrary to common claims, this issue was not solved but inherited by quantum field theory, where it appears in form of the notorious ultraviolet divergence.
Against this background, I will argue that the true significance of fields is that of "book-keeping variables" (Feynman), summarizing the effects of relativistic interactions to formulate dynamics as initial value problems. The field concept, in other words, allows us to trade a diachronic, spatiotemporal description in terms of particle histories for a synchronic description in terms of an infinite number of field degrees of freedom that encode the particle histories in their spatial dependencies. And while this move is very successful (maybe even indispensable) for practical applications, it is ultimately at odds with the principles of relativity.
I will first spell out the case against fields in the context of Maxwell-Lorentz electrodynamics, the locus classicus of field theory, which is actually inconsistent as a theory of fields and point particles. In the classical regime, the Wheeler-Feynman theory provides a viable alternative, explaining electromagnetic phenomena in terms of direct particle interactions, without introducing fields as independent degrees of freedom.
Turning to more modern physics, I will point out that the ontology of quantum field theory is a largely open question and explain why attempts to construct QFT as a quantum theory of fields in 3-dimensional space have failed. I will end with some indications how quantum field theory can be construed as a theory of point particles, recalling the concept of the Dirac Sea that has recently received renewed attention in the philosophical literature. In the upshot, quantum field theories do not provide convincing arguments for a field ontology; for physical and philosophical reasons, point particles are still our best candidate for a fundamental ontology of physics.

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COMMENT ON "POPPER AND MODERN COSMOLOGY"

Session 13C

Congress section(s): B6

As Helge Kragh notes, Karl Popper never engaged scientific cosmology to the degree that he did with other physical sciences, but his dislike of the big bang theory can be shown to be consistent with his own philosophical views. We are, therefore, in a position to examine contemporary cosmological theories through a Popperian methodology, and a criticism of the current cosmological paradigm can be based on his falsificationist ideas.
One of Kragh's insights is that Popper's demarcation criterion has had a lasting impact on the development of scientific cosmology. In this paper I will defend the view that a methodological analysis of contemporary cosmological models that is in line with Popper's demarcation criterion between scientific and non-scientific cosmology can greatly benefit from the use of formal methods. For example, formal methodological criteria can help us answer the question of whether physical cosmology should be considered, as Popper did, to be an immature science. The application of these formal criteria will reveal that there are two contrasting approaches in cosmology, one of which is is compatible, and the other one incompatible with Popper's methodological views.
In practical terms, the difference between these approaches is that in the former the focus is on studying small scale phenomena (e.g. galaxies, clusters), and trying to build models that are successful at making novel predictions at these scales. In the latter approach the primary attempt is to form a model of the universe as a whole and then work our way toward smaller scales.
Both of these approaches face difficulties with explaining some of the available data, and disagreements between their proponents have lead to a surging interest in the foundational methodological questions among cosmologists themselves.

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COMMUNICATION AND EXCHANGES AMONG SCIENTIFIC CULTURES: SHARING, RECYCLING, TRADING, AND OTHER FORMS OF CIRCULATION. CHARACTERIZING AS A CULTURAL SYSTEM THE ORGANIZATION OF MATHEMATICAL KNOWLEDGE: A CASE STUDY FROM THE HISTORY OF MATHEMATICS

Session 29G

Congress section(s): B1, B3, C1

This talk aims at challenging the use of the notion of "culture" to describe a particular organization of mathematical knowledge, a knowledge shared by a few mathematicians over a short period of time in the second half of the nineteenth century. This knowledge relates to "geometrical equations," objects that proved crucial for the modalities of encounters between a part of algebra and a part of geometry at that time. The description of the mathematical collective activities linked to geometrical equations, and especially the technical aspects of these activities, will be made on the basis of a sociological definition of culture. More precisely, after an examination of the social organization of the mathematicians considered, I will argue that these activities form an intricate system of patterns, symbols, and values, for which I suggest a characterization as a cultural system. I will finally suggest that the cultural traits of this cultural system may be seen as cases of geometrical reinterpretations of algebraic patterns made by geometers trying to cope with a part of algebra, which they found difficult

to understand. This will eventually raise the question of interpreting the cultural system as the outcome of a process of acculturation of geometers into an algebraic culture.

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MODEL EXISTENCE IN MODAL LOGICS

Session 24L

Congress section(s): A2

According to [3], there are BCI based propositional/predicate logics which satisfies classical model existence property (every consistent set has a classical model). Such logics are weak: the usual deduction theorem (as a property) does not hold. In this talk we will study model existence property in BCI (or BCIW) based modal logics. Glivenko's Theorem with respect to the corresponding systems will also be investigated.

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SIMULATED DATA

Session 5I

Congress section(s): B1

Empirical data are often problematic in various different ways. This paper provides a discussion on the epistemic issues modelers face when they generate simulated data in an attempt to solve such problems. We argue that in order to count as epistemically justified and evidentially relevant, a simulation model does not necessarily have to mimic the target of some empirical investigation, and simulated data do not need to be generated by mimicking the data generating processes for empirical data. Simulated data may successfully mimic the target system even if the simulation model does not even aim to mimic it. In such cases simulated data typically improves the representational relation between empirical data and the target. The fact that a computer simulation can only contain those variables and relationships that the modeler puts into it has been used in arguing for the epistemic superiority of experiments. We show that this very same feature provides some simulation studies with an epistemic edge over experiments.

The origin of empirical data lies in reality such that its production requires causal interaction with measuring or detection devices, or with human sensory capacities, while the origin of simulated data lies in a computer simulation model. A simulation is not in causal interaction with the system that the simulated data is taken to represent, nor with the empirical data if that is what the simulation aims to represent.

When a simulation model aims to represent a real target system, it may do this by representing the data generating process (DGP) responsible for the empirical data concerning that target. Here the aim is to establish a similarity between the simulation model and the target. Alternatively, they may forgo such an attempt, and instead aim to produce a dataset that describes the target better than empirical data. In some such cases the simulated data mimic the target even though the model from which they derive does not.

Simulated data may also aim to mimic empirical data that the target generates without aiming to mimic the target or its data generating process. Furthermore, a simulation may aim to mimic several possible DGPs, and the plural is important here. We believe that such heterogeneity a good enough reason to elevate the study of simulated data into an independent research topic, a topic that can be discussed without concern for the comparison to experiments.

The aim of this paper is to provide a framework for understanding the use of computer simulation in dealing with problems related to empirical data. We discuss cases from several different disciplines, focusing on the reasons for using simulated data and the relevant mimicking relations. The aim is to provide a general framework that might allow us to understand how to enhance the evidential and epistemic relevance of different types and uses of simulated data.

We will also consider cases with hybrid data, where they derive partly from simulation and partly from some other source. Simulated data are thus combined with empirical data, the aim being to obtain a corrected, enhanced, or unbiased dataset as a whole. Such hybrid data are then typically used for different evidential and epistemic purposes instead of empirical data.

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WHAT IF MEANING IS INDETERMINATE? RAMSIFICATION AND SEMANTIC INDETERMINACY

Public plenary lecture (joint event with Logic Coloquium 2019)

Can we still use classical logic and semantics if the interpretation of natural language terms, mathematical terms, and scientific terms is not uniquely determined? This talk will develop a positive answer by applying a method that goes back to F. P. Ramsey: we only need to "Ramsify" semantics.

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THE CONTOUR OF THE TARSKI BOUNDARY

Session 26J

Congress section(s): A1

Abstract. The aim of this talk is to present some recently discovered connections between various sets of axioms for the truth predicate for the language of arithmetic and the arithmetical consequences of the resulting theories of truth. We focus exclusively on the truth theories built over Peano Arithmetic (PA). One of the most important theoretical concepts in this study is the Tarski Boundary: it is the "line" demarcating the conservative extensions of PA from the non-conservative ones.(Let us recall that a theory of truth Th is a *conservative extension* of PA if its set of arithmetical consequences coincides with those of PA.) By definition, theories of truth below this line are those which generate no new epistemic commitments of PA. The first part of our talk will be devoted to the results concerning the extensions of the basic compositional theory of truth for $PA, CT^{-}(PA)$. Axioms of this theory are simply Tarski's inductive truth conditions written in the arithmetical language extended with the truth predicate.(The theory is also known as $\restriction$ (see [Halbach 2011]). Both the - and $\restriction$ signs are meant to denote the lack of induction axioms for formulae with the truth predicate.) Most importantly we state the Many Faces Theorem that shows that (surprisingly) many natural extensions (including extensions with reflection principles of various kinds as well as extensions with purely compositional principles) of $CT^{-}(PA)$ turn out to be equivalent to the theory CT_0 - the extension of $CT^{-}(PA)$ with induction for $Delta_0$ formulae with the truth predicate (see [Łełyk 2017]). This theory is in fact quite strong: its set of arithmetical consequences can be axiomatized by ω-many iterations of the uniform reflection scheme over , i.e. $\bigcup^{n}_{n\in\omega}(PA)$, where ${}^{0}(PA) = PA^{n+1}(PA) = \cup\forall\bar{x}(Prov_{n}{}_{()}(A(\bar{x})) \rightarrow A(\bar{x}))|A(\bar{x}) \in Form_{L_{PA}}$ In the second part we turn to typed compositional theories of truth which do not prove that the truth predicate commutes with the negation sign. In particular in a model of such a theory there might be sentences which are neither true nor false or both true and false. We show that the Many Faces theorem may fail in this context. More concretely, we show that principles that was equivalent over $CT^{-}(PA)$, over compositional theories of positive truth, $PT^{-}(PA)$ and $WPT^{-}(PA)$ (The first theory is also known as $PT\restriction$ (see [Halbach 2011]), the second is its variant based on the weak Kleene logic.) give not only different theories, but also theories differing in arithmetical consequences. Having obtained some fairly natural finite axiomatizations of different arithmetical theories extending PA, we ask the complementary question: which arithmetical theories extending PA can be finitely axiomatized by a natural axiomatic theory of truth? The answer turns out to be particularly simple: we prove that every r.e. subtheory of $REF^{\omega}()$ can be axiomatized by an extension of $CT^{-}(PA)$ with a sentence of the form "Every sentence from δ is true" where $delta(x)$ is a $Delta_1$ formula which defines an axiomatization of PA being proof-theoretically reducible to the standard axiomatization of PA with the induction scheme. We discuss the applications of this result to the discussion on implicit commitments contained in [Dean 2015] and [Nicolai, Piazza 2018].

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SEMANTIC INTEROPERABILITY: THE OLDEST CHALLENGE AND NEWEST FRONTIER OF BIG DATA

Session 27A

Congress section(s): C6

A key task for contemporary data science is to develop classification systems through which diverse types of Big Data can be aligned to provide common ground for data mining and discovery. These systems determine how data are mined and incorporated into machine learning algorithms; which claims – and about what – data are taken as evidence for; whose knowledge is legitimised or excluded by data infrastructures and related algorithms; and whose perspective is incorporated within data-driven knowledge systems. They thus inform three key aspects of data science: the choice of expertise and domains regarded as relevant to shaping data mining procedures and their results; the development and technical specifications of data infrastructures, including what is viewed as essential knowledge base for data mining; and the governance of data dissemination and re-use through such infrastructures. The challenge of creating semantically interoperable data systems is well-known has long plagued the biological, biomedical, social and environmental sciences, where the methods and vocabulary used to classify data are often finely tailored to the target systems at hand and thus tend to vary across groups working on different organisms and ecosystems. A well-established approach to this challenge is to identify and develop one centralised system, which may serve as a common standard regardless of the specific type of data, mining tools, learning algorithms, research goals and target systems in question. However this has repeatedly proved problematic for two main reasons: (1) agreement on widely applicable standards unavoidably involves loss of system-specific information that often turns out to be of crucial importance to data interpretation; and (2) the variety of stakeholders, data sources and locations at play inevitably results in a proliferation of classification systems and increasing tensions among different interest groups around what system to adopt and impose on others. Taking these lessons into account, this paper takes some steps towards developing a conceptual framework through which different data types and related infrastructures can be linked globally and reliably for a variety of purposes, while at the same time preserving as much as possible the domain- and system-specific properties of the data and related metadata. This enterprise is a test case for the scientific benefits of epistemic pluralism, as advocated by philosophers such as John Dupré, Hasok Chang, Ken Waters and Helen Longino. I argue that "intelligent data linkage" consists of finding ways to mine diverse perspectives and methods of inquiry, rather than to overcome and control such diversity.

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THE SHIFTING SEMANTICS OF PLANT (DATA) SCIENCE

Session 18E

Congress section(s): C3

Within the last two decades, plant science has increasingly sought to apply fundamental biological insights and new techniques developed through laboratory studies of model organisms to research on crops. This move was accompanied by a growth in efforts to(1) move research outside of the standard laboratory environment and into hybrid spaces (such as field stations, farm platforms and smart glasshouses) that are perceived to better capture features of the 'natural environment'; (2) integrate agronomicresearch with 'basic' plant science, so as to harness cutting-edge insights into molecular mechanisms and related technologies to increase food security; (3) study plant species of economic and cultural interest to parts of the world other than Europe andthe United States, such as cassava and bambara groundnut; (4) increase knowledge about gene-environment interactions, using phenotypic traits as conduits to understand the impact of genetic modifications and/or environmental changes on plant structures and behaviors; and (5) produce 'global' infrastructures and venues where data, germplasm and knowledge about plant species used in different parts of the world can be shared and discussed. This paper will discuss the epistemic implications of these trends, focusingon the issues arising from attempts to share phenomic data about crops across different locations, and particularly between high-resourced and low-resourced research environments.

In particular, I discuss thecase of the Crop Ontology and its efforts to document and link the diversity of tools, terminologies and variables used to describe widely diverse species in different parts of the world. I argue that such practices do not relate in straightforward ways to traditional taxonomic practices, and in fact defy existing understandings of systematisation in biology and beyond. Here is a case where reliance on a universal approach to identifyingand labelling traits has repeatedly proved problematic, and yet the attempt to articulate semantic differences is generating new ways to develop and communicate biological knowledge.“

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DIGITAL DETERMINATION AND THE SEARCH FOR COMMON GROUND

Session 9H

Congress section(s): B4

When discussing the possible solutions to the problem of bridging the communicative gap between academic communities, we should consider the contemporary context, and in particular, digital transformations of social ontology. New technological paradigms give rise to a digital world, where digital determination affects all kinds of interactions, leading to computerization of life itself. “Digital optimists” view the process of digitalization as a tool to improve all kinds of scientific interaction. R. Collins sees “social networks” as a kind of referee in the process of accepting innovations, capable of demonstrating a certain level of resistance. “Digital pessimists” emphasize that digital determination give rise to such phenomena as “content viewers” and “the lease of knowledge”, which cause cognitive deformations. Content viewers scan the information in a superficial way, without translating it to personal knowledge. Due to the fact that «the man of the digital era» seeks to find and adopt ready-made information resources copyright has lost its value, which is a disturbing symptom for scientific cohorts.
However, for the academic community, the principle of “Publish or Perish” is still important. Plagiarism, compilation, aggressive scientific discourse is unacceptable. References as an “academic component of science” are absolutely essential. References are viewed as an indicator of authorship and reliability of scientific discoveries, as well as scientists’ social responsibility. The level of academic qualifications is essential for assessing the development of science. At the same time, disciplinary cohorts have always been self-sufficient and spoke different languages. This hindered the understanding of scientific discoveries in terms of human culture, but had its roots in the algorithm of academic education.
Today, methodological strategy of bridgingthe gap between academic communities is associated with convergence. As the basis for convergence, the paradigm of environmentalism, accompanied by an ideology of conscious regulation, is proposed. Attention is also drawn to the synergistic approach and thematic analysis as effective convergence strategies.
In our opinion, availability of specialized disciplinary knowledge can be ensured by an open scientific narrative that enables one to speak about the situation in science, about discoveries and prospects in plain language. The scientific narrative as a kind of “mapping science” is aimed at overcoming the “semantic trap” of a complex disciplinary language. It should be emphasized that the scientific narrative contributes to creating a scientific worldview. Its advantages are due to the fact that scientific worldview includes the elements of methodological and logical analysis, “smart techniques” of conviction and expert decisions.
All the above said brings us to the following conclusions. Firstly, in the context of digitalization the narrative acts as the form of metascientific knowledge aimed at integrating and popularizing the achievements of academic community. Secondly, success depends on a scientist’s personality, his professional and humanitarian culture. Thirdly, the narrative as the ground for bridging the gap between academic communities is needed for coordinating scientific activities in complex situations in the digital era.

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IN DEFENSE OF ABSTRACTIONS: SOFIA YANOVSKAYA BETWEEN IDEOLOGY AND CYBERNETICS

Session 17E

Congress section(s): A4, B5, C1

Sofia Yanovskaya (1896-1966), who was a Professor at Lomonosov Moscow State University, made a significant contribution to the philosophy of mathematics in the USSR at the time of Marxist-Leninist ideology. Educated as a “Red Professor”*, she began with criticizing ‚bourgeois‘ types of thought, namely all forms of idealism, inculcating the ideology of the dialectic of Marxism-Leninism in the field of mathematics. But suddenly Yanovskaya reversed her strategy. The later period of her life was dedicated to the defense of mathematical logic and philosophy of mathematics from the bashing by those in favor of dialectical materialism. In that time she used different strategies, including a dialectical one, to defend abstract objects and other conceptions of mathematical Platonism.
At the beginning of 1930 one applied mathematician asked Yanovskaya, ‘What are you going to do?’ and she answered, ‘Mathematical logic’. ‘How could you study mathematical logic at this period of time?’ he said. This story gives us an opportunity to understand the ideological context of science in the Soviet Union that time. Josef Stalin was a “leading intellectual” who controlled the state and science as well. In his speech “On the questions of the agricultural policy” (December 27th, 1929) he said that ‘practical success lag theoretical thought far behind’. His interpreters extended his words to the necessity to turn ‘theoretical thought’ into the practice of ‘socialist construction’. The purge in Soviet science began with a repression of several leading scientists of that time. Ernest Kolman, mathematician-ideologist, wrote in his article ‘Sabotage in science’ (1931), “There is no more impenetrable curtain as the curtain of mathematical abstraction. Mathematical equations... are letting hostile theses appear as if they are of objective, unprejudiced, precise character, hiding their true existence”. In the ideological journal for philosophy, “Under the Banner of Marxism”, Professor Mitin wrote on the disjuncture between form and content in the theoretical work “From the living method of knowledge, dialectic has been transformed to the aggregate of the abstract formulas”.
Stalin’s fight with formalism in mathematics was not as heavy as it was in the arts and in 1947 Yanovskaya continued to develop mathematical logic, publishing a Russian translation of the Grundzüge der theoretischen Logik by Hilbert and Ackermann along with organizing a seminar on Mathematical Logic (1944) and the History of Mathematics at the Mechanico-Mathematical Faculty. A year later Alfred Tarski’s “Introduction to logic and to the methodology of deductive sciences” was published with a foreword by Yanovskaya. In this period of her professional life, Yanovskaya concentrated herself on justifying the usage of abstract objects in philosophy (while dialectical materialists criticized abstractions) in the article “The fight of materialism and idealism in mathematics”. She defends abstract objects and mathematical logic by appealing to the practice in the paper “The role of practice in the history of the emergence of pure mathematics”. She develops dialectical criticism of idealism and Platonism in mathematics from her own position, at the same time proving that abstract objects are necessary in the research of computability and computing. In the foreword to the volume “Can Machines think?” (Russian translation, 1960) she, after Alan Turing, discusses the character of the human mind. She asks whether the human mind is based on the rules of logic such that we could construct rules, or an algorithm, following which, a ‘machine’ will imitate the cognitive activity of a human. Could human cognition be understood as algorithmic? Describing the position of Turing, she gives the negative answer, but notes that computability and the development of cybernetics has a crucial importance for Soviet society. This was her argument to prove the necessity for the abstract formulas of mathematical logic.
*The Institute of Red Professors of the All-Union Communist Party (Bolsheviks), an institute of graduate level education, has worked from 1921 to 1938 in Moscow.

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TAMENESS, COMPACTNESS, AND COCOMPLETENESS

Session 18A

Congress section(s): A1

We discuss the emerging characterization of large cardinals in terms of the closure of images of accessible functors under particular kinds of colimits. This effects a unification, in particular, of large-cardinal compactness and colimit cocompleteness, bringing the former somewhat closer to the structuralist heart of modern mathematical practice. Mediating these equivalences is the phenomenon of tameness in abstract elementary classes, which, not least for historical reasons, has provided an indispensable bridge between the set-theoretic and category-theoretic notions, beginning with work of myself and Rosicky, Brooke-Taylor and Rosicky, and Boney and Unger. We summarize the current state of knowledge, with a particular focus on my paper “A category-theoretic characterization of almost measurable cardinals” and forthcoming joint work with Boney.

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SEMANTIC PARADOXES OF UNDERDETERMINATION

Session 7M

Congress section(s): A2

In this talk I discuss the following paradoxes (working in terms of propositions as truthbearers thoughout):
(i) The truth-teller: the paradox of a proposition that says of itself that it is true:
P = <P is true>
(ii) The no-no paradox: the paradox of two propositions, each of which says of the other that it is false:
P1 = <P2 is false>
P2 = <P1 is false>
In the case of the truth-teller, there are two possible assignments of truth-value, T or F, but it seems that there is no evidence that tells us which is correct. Similarly, in the case of the no-no paradox, there are two possible assignments of truth-value (P1 is T and P2 is F; or P1 is F and P2 is T) but no evidence that tells is which is correct. These examples can therefore be called 'paradoxes of underdetermination'.
In this talk I discuss Graham Priest's work on these paradoxes (Mortensen and Priest 1981, Priest 2005, Priest 2006) and I make some critical remarks about the responses he proposes. Then I turn to the recent account of truth and paradox given in Liggins forthcoming, which involves radically restricting the schema
(E) <p> is true iff p.
I explain how this approach deals with the paradoxes of underdetermination; and I argue that these proposed solutions have advantages over Priest's.
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CONVERGENCE TO THE CAUSAL TRUTH AND OUR DEATH IN THE LONG RUN

Session 3B

Congress section(s): B1

Learning methods are usually justified in statistics and machine learning by pointing to some of their properties, including (but not limited to) convergence properties: having outputs that converge to the truth as the evidential inputs accumulate indefinitely. But there has long been the Keynesian worry: we are all dead in the long run, so who cares about convergence? This paper sharpens the Keynesian worry and replies to it.
The Keynesian worry challenges the epistemic value of convergence properties. It observes that a guarantee of obtaining the truth (with a high chance) in the long run does not seem to be of epistemic value, because the long run might be too long and we might not live long enough to actually believe in the truth. Worse, some empirical problems pursued in science are very hard, so much so that there is no learning method that is guaranteed to help us obtain the truth---even if we are immortal. Many problems about learning causal structures, for example, are that hard. This is the Keynesian worry on causal steroid. (Reichenbach almost anticipates such hard problems [1], but his example does not really work, or so I argue.) The standard reply guarantees eventual convergence by assuming the Causal Faithfulness Condition [2]. But this amounts to simply assuming away the skeptical scenarios that prevent our belief in the truth.
I defend the epistemic value of various modes of convergence to the truth, with a new reply to the Keynesian worry. Those modes of convergence are epistemically valuable *not* for a consequentialist reason---i.e. not because they provide us any guarantee of such epistemically good outcome as our actually believing in the truth. There is simply *no* such guarantee. The epistemic significance of convergence lies elsewhere.
I argue that modes of convergence to the truth are epistemically valuable for a *non-consequentialist* reason. A good learning method must be one that responds appropriately to evidence, letting evidence play an important role: the role of evidence as a reliable indicator of truth, possibly not perfectly reliable, but becoming reliable in *progressively* more states of the world if the amount of evidence were to increase---all done by making progress in the *best* possible way. This is a role that evidence should play; our longevity plays no role in this picture. And I argue that, thanks to a new theorem, evidence plays that important role only if it serves as input into a learning method that has the right convergence property. In the context of causal discovery, the right convergence property is provably so-called almost everywhere convergence [2,3] plus locally uniform convergence [4].
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THE INCREASING POWER OF CHOMSKY HIERARCHY: A CASE STUDY OF FORMAL LANGUAGE THEORY USED IN COGNITIVE BIOLOGY

Session 7H

Congress section(s): B1, B6, C5, C6, C8

Interdisciplinary research programs have become a significant force in knowledge generation. An example is cognitive biology, one strand of which emerges from applying Chomsky hierarchy, i.e., a classification of formal languages, in experimental psychology and neurolinguistics. This paper is a philosophical analysis of the augmenting effect on the role that the Chomsky hierarchy plays through the cross-disciplinary transfer and interdisciplinary development.
Originated in linguistics for studying natural languages, Chomsky hierarchy was constructed to classify mathematically defined languages based on the generative power of their grammars and the computing power of the abstract machine

(i.e., automaton) that is required to parse the expressions.[1][2] The construction of such a hierarchy drew mathematically inclined theorists to the study of formal languages, which eventually became a crucial, theoretical component of computer science.[6] Recently, biologists have started applying Chomsky hierarchy in the design of artificial grammar learning experiments for probing the cognitive infrastructures in human and nonhuman animals.[3][4][5]
Using the applications of Chomsky hierarchy in those aforementioned disciplines as examples, this paper analyzes three different roles it plays as a classification system, i.e., explanatory, engineering, and explorative. This paper then argues that unlike in linguistics or computer science, scientists in cognitive biology make use all three of these roles in their applications of the hierarchy.
For instance, in linguistics, Chomsky hierarchy is typically used for an explanatory purpose, e.g., providing explanations for particular linguistic phenomena, such as ambiguity. In computer science, it is used for an engineering purpose, e.g., developers adhere to Chomsky hierarchy in their design of programming languages and compilers. Lastly, in neurolinguistics, it is used for an explorative purpose, e.g., when scientists use it to locate neural substrates of linguistic ability.
In cognitive biology, scientists (i) design experiments, (ii) explain the differences in cognitive infrastructure between humans and nonhuman subjects based on the results of the experiments, and (iii) investigate the neural substrate for such differences, all according to Chomsky hierarchy. This paper concludes by suggesting that the augmentation of the knowledge-producing roles of Chomsky hierarchy is a result of two sources: its cross-disciplinary transfer, especially from linguistics and computer science to biology, and the interdisciplinary development of it, particularly between experimental psychology and neurolinguistics.
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IN DEFENSE OF A THOUGHT-STOPPER: RELATIVIZING THE FACT/VALUE DICHOTOMY

Session 4G

Congress section(s): B1, B4, B5, B6

Acknowledging the entanglement of factual and evaluative statements in science and society, we defend a weak, semantic version of the fact/value dichotomy. Just like Carnap’s analytic/synthetic dichotomy, the proposed fact/value dichotomy is relativized to a framework.
Putnam, in his book ‚The collapse of the fact/value dichotomy‘ (2002), credits Hume for having introduced the dichotomy between facts and values into western philosophy. Hume’s approach to drawing the line between facts and values is a principled one, in the sense that he presupposes empiricism in order to obtain the one “correct” dichotomy. Putnam and others have criticized the dichotomy not only for its untenable presuppositions, but also for its allegedly terrible societal consequences due to its functioning as a thought-stopper.
Whereas we concur with the characterization of the dichotomy as an, albeit only potential, thought-stopper, we demur incongruities and tendentious representations in Putnam’s account of the history of the dichotomy. Furthermore, we maintain that, contrary to their bad reputation, some thought-stoppers should indeed be embraced. Instances of useful, preliminary thought-stoppers include the proof that there is no largest prime number, the well confirmed claim that there is no perpetuum mobile, and arguably the adoption of frameworks which distinguish factual from evaluative statements.
Even so, the proposed dichotomy between factual and evaluative statements is not a principled but a relativized one. Accordingly, which framework and which fact/value dichotomy to choose becomes an external question to be decided on pragmatic grounds.

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MODAL NEGATIVE TRANSLATIONS AS A CASE STUDY IN THE BIG PROGRAMME

Session 25C

Congress section(s): A1

This talk is about negative translations—Kolmogorov, Gödel-Gentzen, Kuroda, Glivenko and their variants—in propositional logics with a unary normal modality. More specifically, it addresses the question whether negative translations as a rule embed faithfully a classical modal logic into its intuitionistic counterpart. As it turns out, even the Kolmogorov translation can go wrong with rather natural modal principles. Nevertheless, one can isolate sufficient syntactic criteria for axioms (“enveloped implications”) ensuring adequacy of well-behaved (or, in our terminology, “regular”) translations. Furthermore, a large class of computationally relevant modal logics—namely, logics of type inhabitation for applicative functors (a.k.a. “idioms”)—turns out to validate the modal counterpart of the Double Negation Shift, thus ensuring adequacy of even the Glivenko translation. All the positive results mentioned above can be proved purely syntactically, using the minimal natural deduction system of Bellin, de Paiva and Ritter extended with Sobociński-style additional axioms/combinators. Hence, mildly proof-theoretic methods can be surprisingly successfully used in “the Big Programme” (to borrow F. Wolter and M. Zakharyaschev’s phrase from the “Handbook of Modal Logic”) .
Most of this presentation is based on results published with my former students, who provided formalization in the Coq proof assistant. In the final part, however, I will discuss variants of a semantic approach based either on a suitable notion of subframe preservation or on a generalization of Wolter’s “describable operations”. An account of this semantic approach and comparison with the scope of the syntactic one remain unpublished yet.

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CAN CATEGORICAL PROPERTIES CONFER DISPOSITIONS?

Session 7F

Congress section(s): B4

Gabriele Contessa (2015) argues that what he calls the Nomic Theory of Disposition Conferral (NTDC) (according to which, in each world in which they exist, properties confer specific dispositions on their bearers; yet, which disposition a property confers on its bearers depends on what the (contingent) laws of nature happen to be) is incoherent. On the basis of this result, he claims that only powers (that is, genuine dispositional properties irreducible to categorical bases) can confer dispositions on their bearers. In this paper, I examine a potential challenge to any realist view about categorical features which can be based on Contessa’s conclusion: Let us first assume that NTDC is the only account of disposition conferral that fits the case of categorical features and, furthermore, is exclusively associated with them. Given this first assumption, Contessa’s conclusion is tantamount to saying that categorical features cannot confer dispositions because the only account of disposition conferral that is proper to them is incoherent. Let us further assume that all natural properties should confer dispositions in the minimal sense that some disposition ascriptions are true of their bearers. Given these assumptions and provided that the conclusion of Contessa’s argument is true, we have an argument against the existence of categorical features.
My aim in this paper is to undermine the above argument. To this end, and given that the aforementioned assumptions are considered as relatively uncontroversial by a number of metaphysicians, I shall focus on Contessa’s original argument and question two main claims of his argumentation: first, the claim that intuition can undeniably support an analogy between

the (according to NTDC) role of laws in the determination of the dispositions that properties confer to their bearers and cases of mimicking in the literature about dispositions and, second, the claim that the intuition itself is supported by the fact that, in the context of NTDC, laws are extrinsic to objects. I further undermine the aforementioned analogy by presenting a possible interpretation of the role of laws in which, though extrinsic to objects, laws do not 'bring about' a scenario of disposition-mimicking. Given all these remarks I cast doubt on the two most significant premises of Contessa's argument and, consequently, show that the conclusion he arrives (that is, only powers can confer dispositions) is controversial.

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SCIENTIFIC PERSPECTIVISM. METAPHYSICAL ASPECTS

Session 9J

Congress section(s): B4

Perspectivism has been always an attractive epistemological position. Also, it has become a very suggesting option in the field of philosophy of science. Two recent perspectivist approaches to science are those of Nancy Cartwright and Ronald Giere. However, there are in perspectivism some metaphysical problems very difficult to deal with. One problem is whether all reality could consist of a number of perspectives. Another one is how to understand the subjects that adopt the perspectives. Are they only contents of other perspectives? Can that be repeated indefinitely? In both problems we face a kind of ontological version of the old trilemma of Agrippa: either circularity, or regression, or some sort of non-perspectival foundation.

In our contribution, 1) we will present some important results in recent analyses of the notions of points of view and perspectives, and 2) we will discuss the above introduced two metaphysical problems. In order to face those problems, we propose a strategy different from the options involved in the Agrippa's trilemma. The strategy consists in understanding perspectives as a number of lanterns or torches illuminating certain areas while we move. This model is really powerfull. If perspectives are understood that way, then it is easy to reject the possibility of a complete circular construction of reality as a world of perspectives as well as the possibility of a complete circular construction of the subjects adopting them. Also, we can reject the strategy of regression on the basis that only some short series of perspectives on other perspectives can be in fact carried on. Finally, we can reject non-perspectival foundations. In particular, we can reject the transcendentalist Kantian foundationalist picture of a world entirely configurated or constructed by the subjects having access to it.

If perspectives are understood according to that model, then we arrive to a metaphysical conception of perspectivism as a kind of never complete applied ontology. However, this would not be a limitation of defect. That perspectivism as ontology only can make sense as a sort of applied ontology of some specific domains would be a direct consequence of the real ways we have access to the world. In the context of such a perspectivism, we will defend the plausibility of a non trascendentalist interpretation of Davidson's "triangulation strategy". Both the notions of an objective world and the notion of a subject having some contents in perspective about that world can be understood as the result of particular and concrete dynamics between at least two subjects in discursive interaction and a certain entity that is in the focus of the perspectives of those subjects.

According to the real ways we have access to the world, the metaphysics of perspectivism, in particular of a scientific perspectivism, has to be closely linked to the idea of an ontology always applied to specific domains.

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A FORMAL AXIOMATIC EPISTEMOLOGY THEORY AND THE CONTROVERSY BETWEEN OTTO NEURATH AND KARL POPPER ABOUT PHILOSOPHY OF SCIENCE

Session 16I

Congress section(s): B2

The controversy mentioned in the title had been related exclusively to the science understood as empirical cognition of the world as totality of facts. Obviously, verifiability of knowledge implies that it is scientific one. Popper developed an alternative to the verification-ism, namely, the falsification-ism emphasizing that falsifiability of knowledge implies that it is scientific one. Neurath criticized Popper for being fixed exclusively on falsifiability of knowledge as criterion of its scientific-ness. Neurath insisted that there was a variety of qualitatively different forms of empirical knowledge, and this variety was not reducible to falsifiable knowledge. In my opinion the discrepancy between Popper and Neurath philosophies of science is well-modeled by the axiomatic epistemology theory Ξ as according to this theory it is possible that knowledge is neither verifiable nor falsifiable but empirical one. The notion "empirical knowledge" is precisely defined by the axiom system Ξ considered. The symbols Kq, Aq, Eq in Ξ stand, respectively, for: "agent knows that q"; "agent a-priori knows that q"; "agent has experience knowledge that q". In Ξ the epistemic modality "agent empirically knows that q" is defined by the axiom 4 given below. In this axiom: $\neg\Box\neg Sq$ represents the verifiability principle; $\neg\Box q$ represents the falsifiability one; $\neg\Box(q \leftrightarrow Pq)$ represents an alternative meant by Neurath but missed by Popper. The symbol Pq in Ξ stands for "it is provable that q". Thus, according to the theorems by Gödel, arithmetic-as-a-whole is an empirical knowledge. The theory Ξ is consistent. A proof of its consistency is the following. Let in the theoryΞ the meta-symbols α and β be substituted by the object-one q. Also let Ωq be substituted by Pq. In this case the axiom-schemes of Ξ are represented by the following axioms, respectively.

1: $Aq \rightarrow(\Box q \rightarrow q)$.
2: $Aq \rightarrow (\Box(q \rightarrow q) \rightarrow (\Box q \rightarrow \Box q))$.
3: $Aq \leftrightarrow (Kq \,\&\, (\Box q \,\&\, \Box\neg Sq \,\&\, \Box(q \leftrightarrow Pq)))$.
4: $Eq \leftrightarrow (Kq \,\&\, (\neg\Box q \vee\neg\Box\neg Sq \vee\neg\Box\neg(q \leftrightarrow Pq)))$.

The interpretation Σ is defined as follows.

$\Sigma\neg\omega = \neg\Sigma\omega$ for any formulae ω. $\Sigma(\omega \oplus \pi) = (\Sigma\omega \oplus \Sigma\pi)$ for any formulae ω and π, and for any classical logic binary connective ⊕.

Σq = false. ΣAq = false.ΣKq = true. ΣEq = true. $\Sigma\Box q$ = true. $\Sigma\Box\neg Sq$ = true.

$\Sigma\Box(q \rightarrow q)$ = true. ΣPq = true. $\Sigma\Box(q \leftrightarrow Pq)$ = false (according to Gödel theorems). In Σ all the axioms of Ξ are true. Hence, Σ is a model for Ξ. Hence Ξ is consistent.

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ON THE HISTORICAL RELEVANCE OF GLIVENKO'S TRANSLATION FROM CLASSICAL INTO INTUITIONISTIC LOGIC: IS IT CONSERVATIVE AND CONTEXTUAL?

Session 24C

Congress section(s): A1

For several years we have studied interrelations between logics by analysing translations between them. The first known 'translations' concerning classical logic, intuitionistic logic and modal logic were presented by Kolmogorov (1925), Glivenko (1929), Lewis and Langford (1932), Gödel (1933) and Gentzen (1933). In 1999, da Silva, D'Ottaviano and Sette proposed a very general definition for the concept of translation between logics, logics being characterized as pairs constituted by a set and a consequence operator, and translations between logics being defined as maps that preserve consequence relations. In 2001, Feitosa and D'Ottaviano introduced the concept of conservative translation, and in 2009 Carnielli, Coniglio and D'Ottaviano proposed the concept of contextual translation. In this paper, providing some brief historical background, we will discuss the historical relevance of the 'translation' from classical logic into intuitionistic logic introduced by Glivenko in 1929, and will show that his interpretation is a conservative and contextual translation.

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HOW SCIENCE LOSES BY FAILING TO ADDRESS THE GENDER (AND OTHER) GAPS

Session 3D

Congress section(s): B1, B5

We standardly think of the gender gap (and other participation gaps) as a harm to those groups not fully participating in the sciences. I want to argue that the sciences are also harmed by failures to be more fully inclusive.

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WHAT TIME SYMMETRY CAN (AND CANNOT) TELL US ABOUT TIME'S STRUCTURE

Session 28K

Congress section(s): C2

The relevance of symmetries to know what the world's structure is like has greatly grown in the last years (Baker 2010). We are said, for instance, that by knowing the space-time symmetries of a theory's dynamics, we are allowed to draw metaphysical conclusions about space-time's structure (North 2009). Shamik Dasgupta (2016) has called this sort of inferences "the symmetry-to-reality inference". Time symmetry is a special case of this: if a theory's dynamics is invariant under time reversal, then the direction of time is superfluous. Therefore, the time's structure of the world according to the theory is actually directionless.

In analyzing the inference for time symmetry thoroughly, we find a mix of premises. Given an equation of motion L, we first find formal premises claiming that a symmetry holds, that is, that a variable in L may freely vary preserving L's structure. In this case, by freely varying the sign of time (t by –t), we also get physically equivalent solutions for L. The sign of t (standing for the direction of time) is hence said to be variant. Second, to the extent to which we adhere to the principle that symmetries in the laws must go hand-in-hand with the symmetries of the world's structure (Earman 1989, North 2009), we interpret that a variant feature occurring in L is surplus structure. As we are advised to go with the least structure (by an Ockham's razor), we infer that the direction of time does not belong to the world's structure. Concluding, the direction of time is not part of the fundamental reality.

In this presentation, I shall analyze the symmetry-to-reality inference for the case of time symmetry, focusing on the formal premises. In particular, I shall first argue that there are actually two divergent ways to conceive symmetries in physics: either as contingent properties of the dynamics or as principles guiding theory construction (Brading and Castellani 2007). Whereas the inference would work well when symmetries are considered in the first way, being thus a powerful tool for metaphysicians of science, it might rather be viciously circular when they are understood as guiding principle, and metaphysicians of science should be very careful in drawing metaphysical conclusions from them. In the second place, I shall show that time symmetry is typically understood as a principle guiding theory construction in fundamental theories (Arntzenius and Greaves 2009). Therefore, the inference wouldn't work for drawing metaphysical conclusions about time's structure.

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A SCIENTIFIC-UNDERSTANDING APPROACH TO EVO-DEVO MODELS

Session 29I

Congress section(s): C3

The aim of this paper is to characterize Evolutionary Developmental Biology (evo-devo) models in order to show the role they fulfill in biology —especially how they can provide functional elements which would enrich the theoretical explanation of large-scale evolution. For this purpose, we analyze two evo-devo models: (i) Polypterus model, which explains anatomical and behavioral changes in the evolution of ancient stem tetrapods (Standen et al. 2014), and (ii) Lepidobatrachus model, which accounts for the processes of embryogenesis and morphogenesis of early vertebrates (Amin et al., 2015).
In the last two decades, evo-devo has represented an interesting shift in the way we understand evolution —mainly driven by experimental research in Causal Embriology and Genetics of Development. At the same time, evo-devo has also inspired new challenges in the study of scientific explanation, modeling, experimentation, and as well on the ontological commitments that scientists assume when they undertake theoretical generalizations.
Specifically, explanatory models in evo-devo attempt to represent emergent phenomena, such as phenotypic plasticity. This kind of complex phenomenical relationships, which are of a causal-functional kind, prevents the analysis of scientific explanation from being restricted to the syntactic structure or the semantic content of the theories and models. Thus, we assert that it is required to include the notion of understanding, in order to account the salient role that models play in evo-devo (Diéguez 2013; de Regt et al. 2009). Understanding belongs to a cognitive but also pragmatic domain: the analysis of models must include issues such as the scientist's intentions (to explain/understand a phenomenon) (Knuuttila and Merz 2009), and the material and abstract resources she uses to achieve her goals.
Thereby, from a pluralist and pragmatic approach of the meaning and use of models in science, our ultimate goal is to provide some minimum criteria that evo-devo models must fulfill to provide a genuine or effective understanding that must be conceived as the state of a cognitive subject, but it refers to the utility and manipulability of theories and models evaluated by the scientific community.
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"CULTURED PEOPLE WHO HAVE NOT A TECHNICAL MATHEMATICAL TRAINING": AUDIENCE, STYLE, AND MATHEMATICS IN THE MONIST (1890–1917)

Session 20D

Congress section(s): C1

The Monist appeared in 1890, founded by the German-American zinc magnet Edward Hegeler. This quarterly magazine "Devoted to the Philosophy of Science" was edited by Hegeler's son-in-law, Paul Carus (who was later joined by his wife Mary Carus), who also edited Open Court Publishing. The magazine was a family affair, nevertheless celebrated within the emerging landscape of American periodicals for "sound scholarship" and "fruitful suggestiveness."
From the first volume, The Monist regularly included mathematical content, including popularizations, original research, translations of French or German works, book reviews of recent textbooks. Most of the regular contributors of this content might be considered enthusiastic amateurs, few were professional mathematicians. Yet, such turn-of-the-century names as Poincaré, Hilbert, and Veblen also occasionally wrote for The Monist.
The audience for the mathematics was understood to be "cultured people who have not a technical mathematical training" but nevertheless "have a mathematical penchant." With these constraints, a uniform and inviting style emerged among the varied contributions, described in contrast to the "very repellent form" of elementary textbooks.
This talk will begin by outlining the main features of the style of mathematics that appeared in The Monist between 1890 and 1917. In particular, articles suggested an active conversation between author and reader, encouraging the latter to work through proposed problems or thought experiments with the assistance of diagrams, metaphors, and narratives. A look at readership, evolving content, and competing publications will also help to evaluate to what extent this style succeeded, or was perceived to be successful, in reaching the intended audience.
The focus on style naturally leads to a consideration of what kinds of mathematical content were susceptible to this style and (more tentatively) how this style within a philosophy of science publication may have shaped contemporary developments in the philosophy of mathematics.

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LAWS, CAUSATION AND EXPLANATIONS IN CLASSICAL GENETICS: A MODEL-THEORETIC ACCOUNT

Session 24H

Congress section(s): C3

The aim of this communication is to analyze the kind of explanations usually given in Classical Genetics. Explanations in biology have intriguing aspects to both biologists and philosophers. A summary of these aspects are found in the introduction to the anthology Explanation in Biology: An Enquiry into the Diversity of Explanatory Patterns in the Life Sciences (Braillard & Malaterre 2015):
We will outline four of the most salient problems in the current debate. These problems are related to (1) whether natural laws exist in biology, (2) whether causation plays a specific explanatory role in biology, (3) whether other forms of explanation – e.g., functional or teleological – are also needed, and (4) whether the recent mechanistic type model of explanation that brings together some form of law-like generalizations and of causation fulfill all expectations. (p. 9)
With our analysis of explanations in Classical Genetics the last problem, which relates to the first two ones, will be addressed straightforward. But instead of doing it with "the recent mechanistic type model of explanation", it will be done with a model-theoretic, structuralist account of explanation.
First, explanations in Classical Genetics will be presented in the traditional format of explanations as summarized by arguments.
Later on, the nature of these explanations will be discussed by using explanations in another area of science, namely, Classical Mechanics.

To clarify the situation, and to carry out an analysis of explanations in Classical Genetics, notions of the structuralist view of theories – especially those of theory-net, fundamental law (or guiding principle), specialization, and special law – will be applied to Classical Genetics. In this application, Classical Genetics' fundamental law/guiding principle will be made explicit.

Next, in order to make more transparent the ontological commitments of Classical Genetics (some of which would play a causal role), explanations will be presented in a model-theoretic, structuralist format as ampliative embeddings into nomic patterns within theory-nets.

Finally, it will conclude with a discussion of the presented analysis, arguing in favor of the model-theoretic, structuralist account of explanation "that brings together some form of law-like generalizations and of causation".

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DEFINABLE BISTATIONARY SETS

Session 25B

Congress section(s): A1

The results presented in this talk are motivated by the question whether sets that are constructed with the help of the Axiom of Choice can have simple definitions. In this talk, I want to focus on the definability of bistationary subsets of uncountable regular cardinals, i.e. stationary subsets of such cardinals whose compliment is also stationary. I will first present results that show that the right interpretation of the above question is to ask whether canonical extensions of the axioms of ZFC imply that for certain uncountable regular cardinals κ, no bistationary subset of κ is definable by a Σ_1-formula that only uses κ and sets of hereditary cardinality less than κ as parameters. Next, I will present results that show that extensions of ZFC through large cardinal assumptions or forcing axioms imply that no bistationary subset of the first uncountable cardinal ω_1 is simply definable in this sense. Finally, I will present very recent work that can be used to establish equiconsistency results between the existence of infinitely measurable cardinals and the non-existence of very simply definable bistationary subsets of successors of singular cardinals.

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REGARDING MINIMAL STRUCTURAL ESSENTIALISM IN PHILOSOPHY OF SPACETIME

Session 19H

Congress section(s): B4, C2

Main goals of my presentation are: a) to analyze and to criticize David Glick's (2016) position dubbed "minimal structural essentialism" (henceforth: MSE), a recent contribution of his to the debate about the nature of spacetime and to a more general problem of metaphysical explanations; b) to draw some positive morals from MSE. My main claim in this context is that MSE points towards a possibility of in-world structural individuation of fundamental objects.

As Glick submits, MSE is a candidate for a viable account of metaphysical explanation of general permutability (GP) of objects in the domains of our best fundamental physical theories – GR and quantum mechanics (with respect to quantum statistics of indistinguishable particles). GP is an interpretative principle, according to which "[for] every permutation P of the a entities in S, R(a), R(Pa) represent the same possible state of the world" (Stachel 2002), R being an ensemble of relations. When it comes to GR the entities in question are spacetime points. The reason why it is important to keep GP connected to GR is that if spacetime points did not behave in accordance with GP, determinism in GR would be threatened, as the famous hole argument reveals (Earman, Norton 1987; Earman 1989; Stachel 2014).

MSE applied to GR draws inspiration from metrical essentialism (Maudlin 1988; 1990; Bartels 1996), one of the many responses to the hole argument, in order to explain metaphysically why GP obtains in GR. Central claim in MSE is that "for any relational structure S and any object a embedded in S, a has its place in S essentially whenever S obtains" (Glick 2016: 217). In my presentation, after elaborating more on MSE, especially on some further considerations about the notion of essentialism employed here and how it allows to explain GP, I will raise several objections against MSE.

I shall argue: i) why MSE buys into arbitral selectivism towards space of solutions of Einstein field equations; ii) why the distinction between actual and possible structure is effectively rendered useless in MSE; iii) why the explanation of GP provided by MSE is highly obscured by the ambiguity of what type of structures really count when it comes to the possibility of being "obtainable" in the world. Finally, I will formulate a moral from MSE – that structural individuation of spacetime points can be viewed as in-world individuation, yielding, as I claim, very special objects – a type of non-individuals Lowe called "quasi-individuals" (Lowe 2016: 59). I shall argue why non-essentialist approaches are more suitable in this context.

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THE REACH OF SOCRATIC SCIENTIFIC REALISM: FROM AXIOLOGY OF SCIENCE TO AXIOLOGY OF EXEMPLARY INQUIRY

Session 18G

Congress section(s): B1, B3, B4, B6, C7, C9

This paper constitutes an effort to engage directly on the conference theme, "bridging across academic disciplines." I will argue that a specific refined axiological scientific realism—that is, an empirical meta-hypothesis about the end toward which scientific reasoning is directed—can be extended across domains of inquiry. The ultimate focus here will be on those domains that do not generally fall under the rubric of "science."
I will begin by clarifying the nature of axiological meta-hypotheses in general, defusing a set of concerns philosophers tend to express about them. I will then introduce the refined realist axiological meta-hypothesis and will emphasize that it is one asserted to be independent of its epistemic counterpart (i.e. the scientific realist's epistemic thesis that, roughly, we can justifiably believe successful scientific theories). The axiological meta-hypothesis I advocate specifies as the end toward which scientific theorizing is directed, not merely truth per se, but instead a particular sub-class of true claims, those that are experientially concretized as true. I will then identify a set of theoretical virtues that must be achieved were this end to be achieved; these in turn become desiderata required of the pursuit of the posited end. I will also point to a set of virtues the quest for the posited end encourages or promotes, even if those virtues are not required of its achievement. After showing that my axiological meta-hypothesis both explains and justifies these crucial and agreed upon aspects of theory choice in science, I will argue that it does so better than its primary live competitors—that it fares better at living up to what both it and its competitors, themselves, demand.
I will then turn to apply this axiological meta-hypothesis to disciplines beyond "science" to demonstrate its promise as a theory of inquiry in general, with a special emphasis on the humanities. I will focus on one of the theoretical virtues as pivotal here, one closely related to the familiar notion of "likelihood," but, more specifically, the degree to which a theoretical system implies what it explains and, in the case of axiology, justifies. After showing how the axiological meta-hypothesis I embrace can be liberated from the epistemic baggage by which it is traditionally encumbered, and after indicating the ways in which myths about the scientific method and about demarcation criteria have led us away from seeing this axiological bridge, I will illustrate the prospects for this bridge with respect to history, focusing specifically on a set of issues in the history of science. I will also show how the axiological meta-hypothesis can be used to adjudicate between metaphysical theories as well as meta-ethical theories. I will close by noting the unique justificatory position afforded by the kind of axiological turn I propose—by appealing, not to an epistemic or ontic justificatory foundation, but, instead, to one that is purely axiological.

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EMPIRICAL IDENTITY AS AN INDICATOR OF THEORY CHOICE

Session 16C

Congress section(s): B3

There are many theories about theory choice in philosophy of science, but no any indicator of scientific theory has been precisely defined, let alone a index system. By the example of empirical identity, I shall show that a range of scientific indicators to decide theory choice can be precisely defined by some basic concepts. I think that these indicators can provide us a better description of the principles of philosophy of science. The certain pursuit of theories' empirical identity and novelty leads the cumulative view of scientific progress; under non-cumulative circumstance, it is totally practicable to judge a theory's empirical identity as well as empirical novelty; empirical identity underdetermines the acceptance of a particular theory. It is possible that all the principles of philosophy of science could be explained again through the system of index of theory choice, thus a more rigorous theory of philosophy of science could be established.

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LEWIS, STALNAKER AND THE PROBLEM OF ASSERTION & DEFECTIVE INFORMATION IN THE SCIENCES

Session 24D

Congress section(s): B1

Here I will argue that some of Stalnaker's rules for speech acceptable to rational communicators exclude too much discourse where informative assertions are in fact possible, specifically scientific discourse, given impossible objects, inconsistent theories and cases of informational, metaphysical or semantic indeterminacy, which arguably are required in some of those discourses. The problem of giving an account of meaning in context is one which Lewis (1980) and Stalnaker (1978) are very much concerned to address. In what follows I give a brief summary of their positions and thence I proceed to discuss some philosophical worries in the context of understanding the linguistic phenomenon of assertion when it comes to dealing with defective (inconsistent or indeterminate) scientific information.
The main objective is to contrast Stalnaker (1978, 2003) and Lewis (1980, 1979) on content and context, specifically on whether they have the metaphysical resources to support a revision to Stalnaker's rules of communication when it comes to modeling assertion in the context of defective scientific information. Stalnaker has two relevant rules: one says a proposition asserted is true on some but not all possible worlds in the context set, the second excludes truth value gaps i.e. a proposition must have a truth value. For the former I will argue that gluts might be needed to recover relevant discourse; for the latter, that gaps might be needed (Beall, 2010). I concentrate specifically on a discussion on the alleged inconsistency of the early calculus, In the algorithm employed for making use of this technique one of the steps is for all purposes an assertion which does not rule out every possible world incompatible with it, contra Stalnaker. The early calculus is admittedly an important mathematical theory I discuss among other examples of assertion in the context of defective (inconsistent or indeterminate) information (Brown & Priest, 2004).
I argue that while both Lewis (1986) and Stalnaker (2003) have resources to solve this. Lewis can do so without embracing primitive modality or revising logic by modifying modal realism in the sense outlined by Berto (2010) where he recovers impossible worlds in a Lewisian system and with suitable adjustments for modeling indeterminate or incomplete information. So a modification of Lewis fares better than Stalnaker's account when it comes to salvaging these linguistic phenomena in terms of primitive ideology, since it does not require primitive modality and given the way in which rules for rational communication for assertion in the context of defective information should be modified.
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AGENCY IN EVOLUTIONARY BIOLOGY

Session 12C

Congress section(s): B6

In response to Karl Popper, Denis Noble, Raymond Noble and others who have criticised evolutionary biology's treatment of the agency of organisms, I analyse and defend what is sometimes called 'Neo-Darwinism' or 'the Modern Synthesis' from my own perspective – as an active researcher of evolutionary theory. Since the Enlightenment, the natural sciences have made progress by removing agency from nature and understanding the world in terms of materialistic chains of cause and effect. With influence from William Paley, this mechanistic way of thinking became the bedrock of Charles Darwin's theory of

evolution by natural selection. Evolutionary biology has tended to understand the 'choices' underlying form and behaviour of organisms as deterministic links in the chain between genotypic causes and phenotypic effects, albeit permitting the genotype to exhibit a range of predetermined responses dependent upon the environmental context (as a generalised form of Richard Woltereck's reaction norm). As selection acts on phenotypes, there is little room for concepts like 'free will' or 'meaningful choice' within this form of mechanistic explanation. Instead, agency becomes a useful 'thinking tool' rather than a 'fact of nature' – a metaphor that can be helpfully applied to biological entities beyond organisms, like genes which can be thought of as 'selfish'. Whilst there is reasonable grounds to find this world-view aesthetically objectionable, critics like Karl Popper have suggested that evolutionary theory has gone further in (unscientifically) denying the existence of what it cannot explain (namely, agency). Here, I evaluate this line of criticism, highlighting four different aspects of arguments against the concept of agency within modern evolutionary theory: i) issues of language that reflect phrasing rather than semantics, ii) misunderstandings of the significance of biological facts, iii) areas of acknowledged conflict between world views, and iv) unresolved criticisms. To the last point, I present a personal response to demonstrate how I use a working concept of agency to guide my own research.

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NON-CLASSICAL PROBABILITIES OVER DUNN BELNAP LOGIC

Session 28D

Congress section(s): A2

Bellnap and Dunn [1] introduced a four valued propositional logic allowing, in addition to the classical truth values True and False, the attribution of non-classical truth values Neither and Both accounting for possibly incomplete or contradictory information concerning a particular proposition.
The Bellnap-Dunn four-valued logic has been extensively studied since its introduction and has been proved fruitful in the study of rational agency and the rational agents attitude towards the truth or falsity of propositions in more realistic contexts. More recently there has been attempts to study also the probabilistic extensions of this logic by Dunn [2] and Childers, Majer and Milne [3]. In particular Dunn investigates this probabilistic extension by introducing non-classical probability functions that assign to each proposition in the language a normalised four valued vector that encodes a probability mass function on the four possible truth values. This is in contrast to the classical case where the probability function on the language assigns to each proposition two values expressing a mass function on the proposition and its negation. Dunn [2] studies the logical structure of this probabilistic setting. However to define the logical connectives he makes some very strong independence assumptions that end up having undesirable consequences. In particular in that setting every proposition ends up probabilistically independent of every other proposition. Even of its logical consequences and even of itself. Our work picks up on the non-classical probability functions defined by Dunn but redefine the logical connectives in a way to avoid such undesirable independence consequences. In this new setting we introduce the necessary ingredients for defining conditional probabilities and will show the standard properties for it. Furthermore we propose strategies for aggregating these four valued probability assignments and show the standard properties for the proposed aggregation procedures. We also study the connection with the approach given in [3] and will show that the two setting are inter-translatable.
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SCIENTIFIC COMMUNICATION IN THE PROBLEMATIC FIELD OF EPISTEMOLOGY: INSIDE AND / OR OUTSIDE

Session 6H

Congress section(s): B2, B3, C7

Introduction. Scientific communication problem statement acquires special significance in the context of the competitive struggle of scientific and unscientific knowledge. The complexity of a scientific language contributes to the emergence of various communication problems in interdisciplinary and transdisciplinary aspects. Scientific communication is not limited by the function of information transfer, it provides a solution to the problem of new knowledge legitimizing in science, validity recognition of the theory or research results. The analysis of the intrascientific legitimation of knowledge requires a new arrangement of accents in traditional ideas about the immanent logic of knowledge development and its substantiation, their shift to the mechanisms of epistemic and social structures convergence.
Methods. Communication approaches of scientific interactions study problems are based on the ideas of critical theory (Adorno), poststructuralism (Foucault, Bourdieu), social epistemology (Latour and Bloor), and study the processes of scientific communication exteriorization. Also, authors use scientometric approach.
Discussion. Members of scientific community perceive themselves and their colleagues as sole holders of responsibility for system development and its transmition from teachers to students and followers. T. Kuhn considered the communication in such groups being complete and professional judgments being relatively unanimous. However, since the attention of scientific communities is concentrated on different subject areas, the communication between separate scientific groups is difficult.
Approaches to the study of scientific communication can be divided into two types: the normative approach and the descriptive approach. The normative approach assumes not a description of the studied phenomenon, as with the descriptive one, but the provision of recommendations on how the system should be arranged and how it should function. It is the development of a logical etalon language capable to dispense researchers with the necessity of solving many problems arising from the imperfections of an ordinary language. The lapidary language of logic is only suitable for interaction within the disciplinary areas, it fails at the level of interdisciplinary interactions.
This problem is solved by the second approach to the language of science and scientific communication - the descriptive approach. It involves an assessment-descriptive method of research aimed at empirical research and description of the behavior of individuals and groups in the decision-making process, and began to be actively developed in the second half of the 20th century. Then there appeared such a discipline as scientometrics, which is the application of the cybernetics principles to the study of science. Within the framework of the descriptive approach, all processes of scientific information interaction rely on a communication scheme proposed by Shannon which consists components: information source, message, transmitter, signal, channel, noise, receiver, etc. This model of communication and statistical research methods allow scientometrics to combine the analysis of the growth of publications number with the analysis of their content, channels of scientific communication (scientific journals), and a system of bibliographic references.
Historically, the scientific community was originally formed as a closed culture, inaccessible to the masses. While attempting to study the internal structure of the scientific community and their motives, the researchers faced the so-called "LaPierre paradox" - the discrepancy between the attitudes of the scientist and his real actions. In modern times the connection between science and society has increased significantly. We are witnessing a process of mutual integration of the two structures. If we consider the influence of society on science, then science is understood not as an objective form of knowledge, the emphasis shifts to the bias of scientific knowledge, to the involvement of science in social and political relations. The approach to scientific communication, which combines critical and scientometric approaches, is based on semiotics. Representatives of this direction have built models of the communication process, which are easily adapted for the scientific communication analysis. If we take classical communication model and consider an act of scientific communication consisting of an addressee, addresser, context, contact, message, noise, and code, we can explain and identify difficulties encountered in the process of scientific communication that could not be explained in terms of logical-normative

communication model. These are difficulties caused primarily by the increasing role of interdisciplinary research and the increased flow of information in general.
Conclusion. Thus, problems arising in the process of science interaction both within and outside the scientific community are solved with the help of various communicative approaches to the study of science communication.

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ASYMMETRIES IN INTERDISCIPLINARITY

Session 25E

Congress section(s): B1, B5, C9

Interdisciplinarity is increasingly considered recommendable for pursuing the goals of science. It is encouraged and supported by science policy makers and research funding agencies, while the rest of the institutional structure of science is not perfectly prepared for accommodating it. Interdisciplinarity is often conceived and recommended in terms of programmatic ideals that depict it as symmetric and equal between disciplines. These relationships can be considered in many ways, such as in terms of symmetries in collaboration, understanding, appreciation, contribution, and benefit. These symmetries are often presented as virtues of genuine or otherwise advisable or successful interdisciplinarity. Just as interdisciplinarity in general, these claims about symmetry mostly remain under-examined, and they furthermore are much of the time mistaken. Philosophy of science is in the position to provide some useful community service on the matter. I will make two claims and sketch arguments for them.
[1] Asymmetries abound between disciplines and research fields, and they are vastly diverse. Just a glance at actual scientific practice reveals major asymmetries. Considering the simple case of just two disciplines D1 and D2, the possible asymmetries between them range from instrumental asymmetries, wherein D1 provides D2 with techniques, principles, auxiliary theories, or evidence; to critical asymmetries, wherein D1 sets out, or is used, to criticize or revise the contents or ways of functioning of D2; to imperialistic asymmetries, wherein D1 dominates or invades or subsumes D2; to discriminatory asymmetries, wherein D1 dismisses D2 or discriminates against D2. Naturally, the boundaries between such asymmetries are not sharp; and they can be divided into further sub-types, depending on the precise relationship between D1 and D2.
[2] Each such asymmetry requires a distinct normative evaluation in terms of (ultimate) epistemic advantage. Another diversity complicates this task, that of epistemic advantage. Given that there are numerous kinds and criteria of epistemic advantage, and that they come in different degrees of (in)directness, no generalized
evaluation of either symmetry or asymmetry between disciplines is available. Many asymmetries are not just tolerable but recommendable, while others are problematic. Such judgements are however not easy to make, as yet another complication encumbers the epistemic evaluation, namely the involvement of disciplinary emotions in interdisciplinary relations.
Examples come from inherently interdisciplinary disciplines such as archaeology and sustainability science wherein many kinds of asymmetry prevail between natural and social sciences; and from applications of rational choice theory and game theory across social sciences and humanities. These cases also illustrate the epistemic and emotional contestability of many claims about interdisciplinary asymmetries.

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WHY THE BEHAVIOURAL TURN IN POLICY TAKES BEHAVIOURAL SCIENCE WRONG AND WHAT IT MEANS FOR ITS POLICY RELEVANCE

Session 25I

Congress section(s): B5

Insights from the behavioural sciences are currently reshaping much public policy around the world (Jones, Pykett & Whitehead 2013). Behavioural approaches are drawn upon in a variety of policy fields such as health and environmental policy, labour regulations, and consumer protection law. The behavioural policy units have been established worldwide, in the UK, US, Germany, France, the Netherlands, Australia, Japan and Singapore, as well as at the World Bank and among different teams within the United Nations, at the OECD, and the European Commission.
The application of behavioural research to policy is promoted as a way of making policies more effective, that is, formulating policies which achieve policymakers' aims (Thaler & Sunstein 2008; Shafir 2012). Proponents of the application of the behavioural sciences to policy believe that behavioural research provides the scientific evidence needed to design effective policies. In particular, they claim that a subset of the behavioural sciences (cognitive psychology and behavioural economics) they rely on offers an 'adequate', 'accurate', or 'realistic' account of behaviour and therefore it should be a basis of policy design. They are wrong, however. There is no adequate, or accurate account of human behaviour that any approach within the behavioural sciences could provide.
In her most recent book (2013) Helen Longino presents an epistemological, ontological and social analysis of five approaches to studying aggressive and sexual behaviour, adopting a social epistemological methodology to understand the differences and similarities between them. Her work is an inquiry into the kind of knowledge that these sciences provide about human behaviour. Longino endeavours to understand what we can learn about the causal factors of behaviour from the accumulated knowledge produced by diverse approaches within the behavioural sciences. She argues that each approach represents the causal space differently and we cannot put them all together to achieve a complete causal explanation of a given behaviour. Each approach gives us only partial knowledge.
Longino's analysis is important to understand why pluralism of behavioural findings is a challenge for practical applications of the behavioural sciences. The type of incommensurability that Longino demonstrates in her work (and which characterises most behavioural research, as I will argue and show) calls into question the idea that there are well-justified epistemic reasons for treating one of the behavioural approaches as the 'adequate', or 'accurate' one. This means that we have to completely rethink the widespread view on the ways in which findings from the behavioural sciences could, and should, inform policy. I intend to suggest how this could be done.
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SHOULD A MATHEMATICIAN READ THIS BOOK?

Session 31A

Congress section(s): A1

I myself am a mathematician, specifically a model theorist. In this presentation, I want to address the question: Should a mathematician read this book, a book which, at first glance, appears to be a book explaining model theory to philosophers? Here are some answers I am not going to defend.
• the book is of general intellectual interest and we should read it for culture.
• it is interesting to see how our field interacts with another field.
• it is a good opportunity for mathematicians to get a feel for how philosophers think because the examples analyzed are ones we know well.
• it can call our attention to philosophical aspects of our own work.
These are not unreasonable points but when we weigh them against the work we have in front of us on any given day they may have little urgency. There are many things each of us "should" do for culture.
To me, the answer which does have urgency is the most interesting answer:
for its mathematics.

This is a book written by a mathematician who has been doing core work in the subject for almost fifty years and who, under the umbrella of illustrating various philosophical ideas, gives detailed mathematical information to illustrate his impressions of how various breakthroughs arose. I will discuss (with commentary) some of the interesting examples covered in the book, likely including:

• some long-term influences of Hilbert's work in the field
• some reasons the mathematics around first-order logic is so developed
• some key moves in the work of Robinson and Shelah
• how we might regard appearance of set theory in uncountable models and in contexts like AECs

and suggest some consequences for future work.

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COMPLEXITY AND MODEL THEORY

Session 28E

Congress section(s): A1

The ultraproduct construction gives a way of averaging an infinite sequence of mathematical structures, such as fields, graphs, or linear orders. The talk will be about the strength of such a construction.

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QUANTIFIERS AND CONCEPTUAL EXISTENCE

Session 10F

Congress section(s): A2

The point of departure of our research is María Manzano's paper "Formalización en Teoría de Tipos del predicado de existencia conceptual de Mario Bunge" (Manzano 1985). We recall the main concepts of this article and propose new perspectives on existence offered by a wide variety of new formal languages.

Firstly, we place Bunge's ideas within the historical debate about existence. It seems to us that Bunge is in favor of combining together the traditional view on existence, where it was considered a first-order predicate, with the Fregean account, where existence acts as a second-order predicate.

In the second place, as in Manzano (1985), we make use of the language of Type Theory, TT, to formulate Bunge's distinction between the logical concept of existence and the ontological one. Both the quantifier and the ontological existence are predicates in TT, but to formulate the first one we need only logical constants while for the second one we need non-logical constants. In particular, the existential quantifier could be introduced by definition, using the lambda operator and a logical predicate constant.

Thirdly, we explore another possibility and try to incorporate in the formal system the tools needed to define the ontological existence predicate using only logical constants. In Hybrid Partial Type Theory, HPTT, assuming a semantics with various domains, the predicate of existence can be defined by means of the existential quantifier.

Since a modal model contains many possible worlds, the previous formula could be true at a world (for instance, the world of physical objects) but false at another world of the same structure (for instance, the world of conceptual objects).

Moreover, thanks to the machinery of hybrid logic we have enhanced our formal system with nominals, such as i, and with satisfaction operators, @. Nominals give us the possibility of naming worlds and satisfaction operators allow us to formalize that a statement is true at a given possible world. In this logic, we have formulae that could be used to express that the individual object named by the term t exists at the world of physical objects named by i.

In HPTT, we could use the existential quantifier, the equality and the satisfaction operator to express that an object has ontological existence, either physical or conceptual. We do not need specific non-logical predicate constants given that the satisfaction operator is forcing the formula to be evaluated at i-world.

Lastly, we analyze existence in the language of our Intensional Hybrid Partial Type Theory, IHPTT. This opens a new possibility concerning existence which we have not taken into account so far. It is related with considering existence as a predicate of intensions. In our IHPTT, existence can also be predicated of intensions, and we should expand our previous definition to include terms of type (a, s).

Our formal languages have tools for dealing with existence as a predicate and also as a quantifier. In fact, it is possible to give a coherent account of both alternatives. Therefore, from the point of view of the logical systems we have presented in this paper, the relevant issue is that we have tools for dealing with Bunge's distinctions in a variety of forms.

We have shown that hybridization and intensionality can serve as unifying tools in the areas involved in this research; namely, Logic, Philosophy of Science and Linguistics.

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THE TRUTH IN UNDERSTANDING

Session 12F

Congress section(s): B1, B4

Elgin has argued that scientific understanding is, in general, non-factive because it often partly consists in idealizations ("felicitous falsehoods"). In contrast, Strevens argues that idealizations can be eliminated from models by which we understand phenomena of interest, and hence that understanding is "correct," or quasi-factive. In contrast to both, I argue that the factivity debate cannot be settled, as a matter of principle.

The factivity debate concerns whether felicitous falsehoods can ever constitute our understanding. Elgin (2004, pp. 113-114) cites "the laws, models, idealizations, and approximations which are... constitutive of the understanding that science delivers." Yet, as Strevens notes, the evidence Elgin adduces for non-factivity is consistent with idealizations falling short of constituting understanding.

In contrast, for Strevens (2013, p. 505), to understand why something is the case is to "grasp a correct explanation" of it. For Strevens, explanation is model-based, hence so is the understanding that explanation provides. The role of idealizations is heuristic: to provide simplified models that preserve factors that causally and counterfactually make a difference to the phenomena theorized. Strevens (2013, p. 512) distinguishes the explanatory and literal contents of idealized models. The literal content of the model includes idealizations and their consequences. We obtain its explanatory content by devising a translation manual that eliminates idealizing assumptions and replaces them by conditional statements that are actually true. Understanding is correct (quasi-factive) to the extent that the explanatory content of the model by which we understand is accurate.

I now move to my own contribution. In appraising the debate about whether understanding is factive, we should differentiate between our conceptions – the stuff of thought – and the cultural artifacts we use as props for thinking: our models and theories. When many alternative models of the same phenomena are available, some models are more "cognitively salient" than others (Ylikoski and Kourikoski 2010). Subjectively, they come to mind more easily; objectively, their easier access is due to their greater explanatory power.

With the theory/ mind difference in view, I distinguish two questions: (i) whether idealizations are constitutive to the models scientists use; and (ii) whether idealizations are constitutive of the cognitive representations by which scientists understand. If there's nothing more we can say about the cognitive aspects of understanding, then we lack a procedure for finding which parts of a model are internalized as cognitive representations.

This matters for the factivity of understanding: we have no way of telling whether idealizations (be they in-principle eliminable or not) are in fact cognitively represented by scientists conceiving of the phenomena thus idealized. That is, we have no basis to settle the issue of whether understanding is quasi-factive or non-factive.

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ONLINE MISINFORMATION AS A PROBLEM OF EMBODIED COGNITION

Session 15M

Congress section(s): C5, C8

This paper argues that the creation and propagation of misinformation in online environments, particularly in social media, is confronted with specific challenges which are not to be found in offline communication. Starting from the widely accepted definition of misinformation as ‚deliberate production and distribution of misleading information' (Floridi, 2013) which we designate as the semantic view of misinformation, we aim to provide a different definition of misinformation based primarily on the pragmatics of communication and on the role of the technological environment. While misinformation in online environments is also false and misleading, its main characteristic is the truncated way in which it is perceived and re-interpreted and, we will argue, this way of processing information belongs foremost to the online environment as such rather than to a defective way of information-processing from the side of the epistemic agent. From this pragmatic perspective, sometimes misinformation is true information which is interpreted and propagated in a biased way. One of the major features of the online environments which makes it for a medium prone to mis-interpretation and bias concerns a way of leading to impoverished sensory information processing. Assuming an embodied cognition view – in its compatibilist version, see (Varela et al., 1991; Clark, 1997) - then the environment in which we exercise our cognitive abilities has a deciding role for our ability to function as epistemic agents because through our bodies and we acquire cognitive states dependent on the environment to which our bodies are exposed. Following this embodied cognition assumption, then the online environment presents itself as a challenge through the ways in which it prioritises certain senses while obliterating others: the visual senses are primordial to the detriment of other senses such as touch, smell, and even hearing; moreover, we interact with others in online environments through text messages which favor explicit meanings while tacit communication and other pragmatic aspects of communication relying on body-language and non-verbal signs are lost.
This presentation will describe the constellation of aspects which characterise the pragmatics of communication in online environments and then show why this kind of communicational situation is biased leading to what we will call an ‚incomplete pragmatics' of communication. In online environments, we will argue, misunderstandings are the rule and not the exception, because of the dis-embodied and text-biased forms of communication. We will illustrate our theory of incomplete pragmatics of online communication with several case studies of online misinformation based on factually true information which is systematically misunderstood.
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ON CALCULI AND RANKS FOR DEFINABLE FAMILIES OF THEORIES

Session 31M

Congress section(s): A1

Abstract. Let $\mathcal{T}$ be a family of first-order complete theories in a language L, $\mathcal{T}_L$ be the family of all first-order complete theories in a language L. For a set Φ of L-sentences we put $\mathcal{T}_\Phi = \{T \in \mathcal{T} \mid T \models \Phi\}$. A family of the form $\mathcal{T}_\Phi$ is called *d-definable* (in $\mathcal{T}$). If Φ is a singleton $\{\varphi\}$ then $\mathcal{T}_\varphi = \mathcal{T}_\Phi$ is called *s-definable*. We consider properties of calculi for families $\mathcal{T}$ with respect to the relations $\vdash_\mathcal{T}$, where $\Phi \vdash_\mathcal{T} \Psi \Leftrightarrow \mathcal{T}_\Phi \subseteq \mathcal{T}_\Psi$. We use terminology from [1, 2] including E-closure $\mathrm{Cl}_E(\mathcal{T})$, rank $\mathrm{RS}(\mathcal{T})$, and degree $\mathrm{ds}(\mathcal{T})$. For any sets Φ and Ψ of sentences and a family $\mathcal{T}$ of theories the following conditions are equivalent: (1) $\Phi \vdash_\mathcal{T} \Psi$; (2) $\Phi \vdash_{\mathcal{T}_0} \Psi$ for any finite $\mathcal{T}_0 \subseteq \mathcal{T}$; (3) $\Phi \vdash_{\{T\}} \Psi$ for any singleton $\{T\} \subseteq \mathcal{T}$; (4) $\Phi \vdash_{\mathrm{Cl}_E(\mathcal{T})} \Psi$. For any sets Φ and Ψ of sentences in a language Σ the following conditions are equivalent: (1) $\Phi \vdash \Psi$, i.e., each sentence in Ψ is forced by some conjunction of sentences in Φ; (2) $\Phi \vdash_{\mathcal{T}_L} \Psi$; (3) $\Phi \vdash_\mathcal{T} \Psi$ for any (finite) family (singleton) $\mathcal{T} \subseteq \mathcal{T}_L$; (4) $\Phi \vdash_\mathcal{T} \Psi$ for any (finite) family (singleton) $\mathcal{T}$. A subfamily $\mathcal{T}' \subseteq \mathcal{T}$ is d-definable in $\mathcal{T}$ if and only if $\mathcal{T}'$ is E-closed in $\mathcal{T}$, i.e., $\mathcal{T}' = \mathrm{Cl}_E(\mathcal{T}') \cap \mathcal{T}$. For any ordinals $\alpha \le \beta$, if $\mathrm{RS}(\mathcal{T}) = \beta$ then $\mathrm{RS}(\mathcal{T}_\varphi) = \alpha$ for some (α-ranking) sentence φ. Moreover, there are $\mathrm{ds}(\mathcal{T})$ pairwise $\mathcal{T}$-inconsistent β-ranking sentences for $\mathcal{T}$, and if $\alpha < \beta$ then there are infinitely many pairwise $\mathcal{T}$-inconsistent α-ranking sentences for $\mathcal{T}$. Let $\mathcal{T}$ be a family of a countable language Σ and with $\mathrm{RS}(\mathcal{T}) = \infty$, $\alpha \in \{0, 1\}$, $n \in \omega \setminus \{0\}$. Then there is a d-definable subfamily $\mathcal{T}_\Phi$ such that $\mathrm{RS}(\mathcal{T}_\Phi) = 1$ and $\mathrm{ds}(\mathcal{T}_\Phi) = n$.
This research was partially supported by Committee of Science in Education and Science Ministry of the Republic of Kazakhstan (Grants No. AP05132349, AP05132546) and Russian Foundation for Basic Researches (Project No. 17-01-00531-a).

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DESIGNING THE STRUCTURALIST STYLE: BOURBAKI, FROM CHEVALLEY TO GROTHENDIECK

Session 18D

Congress section(s): C1

Nicolas Bourbaki, the name under which a group of young French mathematicians decided to publish a series of volume on mathematics, had a profound influence on the development and presentation of the second part of 20th century mathematics. In this talk, I will argue that right from the start, that is in 1934-35, Bourbaki knew he was introducing a

new style of doing, presenting and developing mathematics. This style is usually identified with the systematic usage of the axiomatic method. But this description comes short, for what Bourbaki is proposing is really a conceptual approach to mathematics which is, in the end, encapsulated in a specific form of structuralism. The main purpose of this talk is to explain the technical details underlying this approach as well as its philosophical consequences.
In 1935, one of the original members of Bourbaki, Claude Chevalley, published a paper in a philosophical journal entitled "Variations du style mathematique", in which he articulates some of the elements that will become Bourbaki's vision. The latter is usually identified with the paper published by Bourbaki in 1950, but in fact written by another member of Bourbaki, Jean Dieudonne, and entitled "The Architecture of Mathematics". We will briefly present the main theses of these papers, but our target will rather be what one finds in the published volumes and the Bourbaki archives, in particular in the volume on logic and set theory and its various preliminary versions, which took 20 years to complete. It is in this volume that one finds the articulation of the axiomatic method and, in particular, its structuralist version. It turns out that Bourbaki built in the notion of isomorphism in the various mathematical theories he was interested in. We will discuss in what sense this is a version of mathematical structuralism.
However, it turns out that Bourbaki's version, although essentially correct when it was formulated, faced a problem with the advent of category theory. One of its young members, Alexander Grothendieck, took the bull by the horns and kept Bourbaki's spirit by using categories to construct new mathematical theories. Grothendieck left the Bourbaki group, since some of the original members disagreed with the way he was using categories in mathematics. I will argue that he was nonetheless faithful to the original project launched by Bourbaki and that he was in fact adding a stone to the structuralist edifice erected by his predecessors, an edifice which is still under construction as I write.
With this special episode at hand, we will suggest a characterization of the structuralist style of abstract mathematics. We will contrast our analysis with those proposed by Gilles- Gaston Granger, Paolo Mancosu and David Rabouin.

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FROM OUGHTS TO GOALS

Session 11B

Congress section(s): A2

Suppose I believe sincerely and with conviction that today I ought to repay my friend Ann the 10 euro that she lent me. But I do not make any plan for repaying my debt: Instead, I arrange to spend my entire day at the local spa enjoying aromatherapy treatments.
This seems wrong. Enkrasia is the principle of rationality that rules out the above situation. More specifically, by (an interpretation of) the Enkratic principle, rationality requires that if an agent sincerely and with conviction believes she ought to X, then X-ing is a goal in her plan. This principle plays a central role within the domain of practical rationality, and has recently been receiving considerable attention in practical philosophy (see Broome 2013, Horty 2015).
This presentation pursues two aims. Firstly, we want to analyze the logical structure of Enkrasia in light of the interpretation just described. This is, to the best of our knowledge, a largely novel project within the literature. Much existing work in modal logic deals with various aspects of practical rationality starting from Cohen and Levesque's seminal 1990 paper.
The framework presented here aims to complement this literature by explicitly addressing Enkrasia. The principle, in fact, bears some non-trivial conceptual and formal implications. This leads to the second aim of the talk. We want to address the repercussions that Enkrasia has for deontic logic. To this end, we elaborate on the distinction between so-called "basic oughts" and "derived oughts", and show how this distinction is especially meaningful in the context of Enkrasia. Moreover, we address issues related to the filtering of inconsistent oughts, the restricted validity of deontic closure, and the stability of oughts and goals under dynamics.
In pursuit of these two aims, we provide a multi-modal neighborhood logic with three characteristic operators: A non-normal operator for basic oughts, a non-normal operator for goals in plans, and a normal operator for derived oughts. Based on these operators we build two modal logical languages with different expressive powers. Both languages are evaluated on tree-like models of future courses of events, enriched with additional structure representing basic oughts, goals and derived oughts. We show that the two modal languages are sound and weakly (resp. strongly) complete with respect to the class of models defined. Moreover, we provide a dynamic extension of the logic by means of product updates.

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ON REVISION-THEORETIC SEMANTICS FOR SPECIAL CLASSES OF CIRCULAR DEFINITIONS

Session 15L

Congress section(s): A2

Circular definitions are definitions that contain the definiendum in the definiens. The revision theory of circular definitions, created by Anil Gupta, shows that it is possible to give content to circular definitions and to use them to solve the semantic paradoxes. Let us consider definitions of the form Gx := A(x,G), where A is a first-order formula in which G itself can occur. Given a model M for the language, possible extensions for the predicate G are given by subsets of the domain of the model (which are called hypotheses). Given a hypothesis h, the revision of h (denoted D(h)) is the set of all the elements of the domain which satisfy the definiens in the model M+h (i.e., the model M with the hypothesis that the extension of G is h). Revision can be iterated, generating the sequence of revision: h, D(h), D(D(h))... which is represented as $D^0(h)$, $D^1(h)$, D^2(h)... Roughly speaking, the key idea of revision theory is that one can categorically assert that an object is G when, for every hypothesis h, the object eventually stabilises in the sequence of revision that starts with h, i.e., it belongs to all the hypotheses in the sequence after a certain ordinal.
Gupta (in "On Circular Concepts", in Gupta and Chapuis, "Circularity, Definition and Truth", Indian Council for Philosophical Research, 2000) defined a special type of definitions, called finite definitions, and proved that this class of definitions has nice formal properties, for instance, there is a natural deduction calculus sound and complete for their validities.
The aim of the talk is to introduce several generalizations of finite definitions that still preserve many of their good properties. Given a type of hypotheses T, we will define the following four classes of special circular definitions:
(i) A definition is a T-definition iff for each model M, there is a hypothesis of type T.
(ii) A definition is a uniformly T-definition iff for each model M and each hypothesis h, there is n such that D^n(h) is of type T.
(iii) A definition is a finitely T-definition iff for each M there is n such that, for each h, D^n(h) is of type T.
(iv) A definition is a bounded T-definition iff there is n such that for every M and h, D^n(h) is of type T.
A finite definition is, in this notation, a finitely T-definition, where T is the type of reflexive hypotheses (i.e., those that generate cycles in the directed graph that connects h to D(h)). We will analyze the relations among the different classes of definitions, focusing in the types of reflexive hypotheses and descending hypotheses (i.e. hypotheses which belong to Z-chains in the directed graph).

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DIALOGICAL JUSTICATION LOGIC, A BASIC APPROACH

Session 26I

Congress section(s): A1, A3

In this paper we will sketch the basic system of Dialogical Justication Logic (DJL) (Artemov, 2008) (Artemov & Fitting, 2016) (Studer, 2012), which is a logical framework for reasoning with justied assertions in dialogues. Following the dialogical perspective, dialogues are disputes in which two parties argue over a central claim (Keiff, 2011) (Rahman & Keiff, 2005) (Rahman, McConaughey, Klev, & Clerbout, 2018). We show multiple and simple examples of dialogues around some thesis, where explicit justications are given. In particular, we argue that 1) in DJL we achieve an interesting and clear interpretation

of the constant specication structures of JL; and 2) DJL it is useful to clarify traditional paradoxical situations (McNamara, 2014). We provide examples for two deontic cases: Forrester's and Miner's Paradoxes (Forrester, 1984) (Kolodny & MacFarlane, 2010) (Klev, 2016). This provides a clear motivation of the logical derivations of the system and makes it clear why and how we need formal and material notions in different classes of CS for formal and material dialogues.

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MAKING SENSE OF DEFECTIVE INFORMATION: PARTIALITY AND BIG DATA IN ASTROPHYSICS

Session 27D

Congress section(s): B1

While the presence of defective (inconsistent, conflicting, partial, ambiguous and vague) information in science tends to be naturally seen as part of the dynamics of scientific development, it is a fact that the greater the amount of defective information that scientists have to deal with, the less justified they are in trusting such information. Nowadays scientific practice tends to use datasets whose size is beyond the ability of typical database software tools to capture, analyze, store, and manage (Manyika et al., 2011). Although much current scientific practice makes use of big data and scientists have struggled to explain precisely how do big data and machine learning algorithms actually work, they still rationally trust some significant chunks that these datasets contain. The main question we address is: In the era of big data, how can we make sense of the continued trust placed by scientists in defective information in the sciences consistently with ascribing rationality to them?

In order to respond to this question, we focus on the particular case of astrophysics as an exemplar of the use of defective information. In astrophysics, information of different types (such as images, redshifts, time series data, and simulation data, among others) is received in real-time in order to be captured, cleaned, transferred, stored and analyzed (Garofalo et al. 2016). The variety of the sources and the formats in which such information is received causes the problem of how to compute it efficiently as well as the problem of high dimensional data visualization, that is, how to integrate data that have hundreds of different relevant features. Since such datasets tend to increase in volume, velocity and variety (Garofalo et al. 2016), that makes it even harder to achieve any deep and exhaustive understanding of what they contain. However, this has not prevented astronomers from trusting important chunks of the information contained in such datasets.

We defend that such trust is not irrational. First, we argue that, as astrophysics is an empirical science, empirical adequacy of astronomical chunks of information plays an important role in their rational acceptance. Second, we contend that, despite their defectiveness, the chunks of information that astronomers trust are empirically adequate. In order to defend this, we appeal to a particular formulation of empirical adequacy (first introduced in Bueno, 1997) that relies on resources of the partial structures framework to accommodate inconsistent, partial, ambiguous and vague information in the current scientific practice of astrophysics.

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THIN OBJECTS AND DYNAMIC ABSTRACTION VERSUS POSSIBLE STRUCTURES

Session 20A

Congress section(s): C1

One perennial research question in the philosophy of mathematics is the one about the existence of mathematical entities. If they exist, they are abstract entities; but abstract entities are philosophically problematic because, among other reasons, of the difficulties involved in providing an account of how it is possible for us to know them. As a result, some authors contend that what is relevant to explain the role of mathematics in our intellectual lives is not whether mathematical entities exist, but whether mathematical statements are objectively true.

Hellman (1989) is one of them. He, following Putnam (1967), proposed a modal paraphrases of arithmetic and set theory (among other mathematical theories) that intends to guarantee the truth of mathematical statements, not to compromise with the existence of mathematical entities in this world and to provide an adequate epistemology. Linnebo (2018) contends that mathematics is true and should be read at face value. Nevertheless, he claims that we do not need to give up on mathematical entities to provide an appropriate epistemology; all we need is a thin account of mathematical entities, an account such that "their existence does not make a substantial demand on the world" (Idem, xi). To prove his point, he reconstructs arithmetic in abstractionist terms and set theory in abstractionist and modal terms.

Both reconstructions of set theory are modal though Hellman chooses Second Order S5 while Linnebo goes for Plural S4.2. These choices are motivated by the different ways in which they conceive of sets. Hellman understands them as positions in possible structures and defines them as the reification of the result of a selection process; Linnebo sees sets as a result of abstractions and introduces them by means of a predicative, plural, and modal version of Frege's Law V. This allows him to accept non-predicative versions of the Comprehension Axiom for "$\in$", while Hellman says that whether they are compatible with his definition is an open question. Nevertheless, both avoid compromise with infinite quantities while asserting that their respective proposals manage to reconstruct the most abstract levels of the set hierarchy.

As it is well known, Gödel established it is impossible to paraphrase arithmetic in trivial terms, hence, any reformulation of arithmetic (or of any mathematical theory that includes it) is going to be controversial in one aspect or other. It can be controversial (Rayo 2015) because of the linguistic resources it uses, because of the metaphysical assumptions that underlie it, or because of the subtracting strategy proposed.

Our purpose is to analyze in detail the two reconstructions of set theory provided, list out the logical tools used by each of them and see how their choices relate to the philosophical constraints each of them advocates.

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AN ALGEBRAIC MODEL FOR FREGE'S BASIC LAW V

Session 31M

Congress section(s): A1, C1

1
As it is well known, the system of Frege's Grundgesetze is formally inconsistent. Indeed,BLV,(εF(ε) = εG(ε)) ↔ ∀x(Fx ↔ Gx),plus the full–CA, ∃X∀x(Xx ↔ φ(x)), leads to inconsistency: Russell Paradox is derivable: ∃X ∀x(X x ↔ ∃X [x = y–(Xy) ∧ ¬Xy].
In recent years, Heck, Ferreira and Wehmeier have pointed out that BLV is consistent with some predicative restriction. However, they have succeeded to recover at most Robinson's Q.
Contrary to any predicative setting, I shall employ a full-impredicative approach by using some crucial algebraic intuition in order to give a model for the theory TK . My aim is a model-theoretic consistent representation of Frege's Grundgesetze. Both BLV and CA won't be syntactically restricted: I shall only impose semantical restriction on BLV.
2
The above mentioned characterisation proceeds in two different stages.
Firstly, I shall fix a domain of interpretation M = ⟨D, ⊆⟩ where D = ℘(ω) is a poset, ⊆ is a relation, reflexive, antisymmetric, and transitive over D. Subsequently, I shall define over M a monotone function φ order-preserving. According to Moschovakis, φ has the least fixed point property. Thus, my purpose shall be to apply φ to TK -predicates: only φ-monotone predicates that have least fixed point property delivers concepts.
An interpretation for the syntax of TK shall be given in agreement with the for- mer structure: the pair (E , A), extension and anti-extension, interprets any second- order variable Fi, where E(Fi) ⊆ M2 (second-order domain); A(Fi) ⊆ M2 and E(Fi) ∩ A(Fi) = ∅; the function ν : π → M1 (first-order domain) interprets ε, where π ⊆ M2, is the set of all φ-monotone predicates with extension or anti- extension fixed – it is also clear how BLV is restricted; the interpretation of the quantifiers is given in standard SOL definitions.
Secondly, in order to fix denotation for any ε-term (VR-term) I shall generalise M: the triple ⟨M, ⊆, F⟩, where ⟨M, ⊆⟩ is a poset, ⟨M, F⟩ is a field of sets, with F ⊆ ℘(ω) and F = π. According to this representation of the Boolean Algebra, to any point in M corresponds a M1 individual of TK and, to any complex in F correspond a φ-monotone predicate. Thus, such structure is a model of TK . Finally, TK results both consistent and strong enough to recover FA. The Russell paradox is blocked for the following reason: let R = ∃F [y = x̂(F x) ∧ ¬F x].R is not φ-monotone, it does not delivers any concept and there is no correspond- ing VR-term:R ∈/ Eσ ∩ Aσ. Furthermore, TK manages to recover FA: I may form the concept N(x) =def P red+(0, x) because only with a predicative fragment I have at least Dedekind-infinitely many M1 individuals that fall under it. If P red+(0, x) = ∃F ∃u(F u ∧ y = #F ∧ x = #[λz.F z ∧ z≠ u]), TK proofs that F is φ-monotone, namely, there is a corresponding VR-term.
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SUPER-HUMEANISM: A NATURALIZED METAPHYSICAL THEORY?

Session 3H

Congress section(s): B4

In order to know what reality is like, we can turn either to our best physical theories or to our best metaphysical ones. However, this alternative is not exclusive, since we can always glean knowledge about our physical reality from naturalized metaphysical theories, which are metaphysical theories grounded in physics' empirical investigations. But how can we evaluate whether a metaphysical theory is sufficiently naturalistic? This question occupies a special role in debates in the metaphysics of science, especially after Ladyman and Ross' vigorous defence of naturalized metaphysics (Ladyman and Ross 2007). My talk will ask whether Super-Humeanism, which is a doctrine that posits only permanent matter points and distance relations in the fundamental ontology, is a naturalized metaphysical theory. While its proponents claim so (Esfeld and Deckert 2018) on the grounds that all the empirical evidence ultimately reduces to relative particle positions and their change, Wilson (2018) argues that it is an a prioristic and insufficiently naturalistic theory. On the one hand, Esfeld and Deckert (2018) have already successfully shown that Super-Humeanism is compatible with our most successful physical theories. On the other hand, demonstrating such compatibility is not enough to reach the conclusion that Super-Humeanism is a naturalized metaphysical theory, especially since its advocated ontology diverges from a standard reading of those theories. In my talk, I will show that this debate is surely difficult to settle if we appeal to general methodological principles. Indeed, the dependence of naturalized metaphysics on science can be spelled out in very different ways. For instance, while Ney (2012) evokes a neo-positivist attitude, Esfeld and Deckert (2018) openly reject this approach; at the same time, on the one side Allen (2012) presupposes that naturalized metaphysics shares the same methodology with scientific disciplines and Ladyman and Ross (2007) regard it as driven by science, on the other side, Morganti and Tahko (2017) argue for a moderately naturalistic metaphysics which is only indirectly connected to scientific practice. Given the difficulty of settling the debate by appealing to general principles, in order to evaluate whether Super-Humeanism is a naturalized metaphysics theory I will examine specific cases which show how Super-Humeanism is implemented in physical theories. One of them will touch a central notion of Super-Humeanism which is the impenetrability of particles; in particular, I will discuss whether the commitment to impenetrable particles can be justified 'naturalistically', within a Super-Humean framework, which rejects any natural necessity in the fundamental ontology.
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LAKATOS' PHILOSOPHY OF MATHEMATICS AND "POLITICAL IDEOLOGIES"

Session 17E

Congress section(s): C1

Imre Lakatos (1924-1973) left Hungary in December 1956 after 12 rather adventurous years in politics and science. His intention was to leave politics and to work in mathematical analysis. But his career turned into a slightly different direction: it was philosophy of mathematics. His political interests were apparently asleep.(1) But he wrote in a footnote of Proofs and Refutations: „The analogy between political ideologies and scientific theories is more far-reaching than is commonly realized..." (2) My paper will show how this claim applies to Lakatos' own work.
In the files of the Lakatos Archive of the London School of Economics, there are a lot of little fragments, scraps, glosses coming from Lakatos' first years in Britain displaying some thoughts motivating his interest in the philosophy of

mathematics. A recurring pattern in his remarks is the opposition of system and method – a topos coming from the Marxist criticism of Hegel. In this opposition system stands on the wrong, reactionary, stereotyped side and method takes the revolutionary, ever-changing, good side. Lakatos uses this opposition to characterize by analogy both the opposition between the Stalinist system of Hungary and the revolutionists of '56 (to whom he counts himself) and at once the opposition between the classical, „formalist" philosophies of mathematics and his ideas.

Let me present here three of these notes in my translation (except of some words that are in English in the original).

"Twofold expectations against proof theory:

[On the left side of the scrap:] system/ constancy/ foundation/ (formal logic, definition, as you like it [in English])

[On the right side:] method/ change/ opening of doors/ The main task of philosophy is to bring over to this. (revolutionary change [in English]) "

"It seems that philosophy occurs in mathematics and in the natural sciences always as a gendarme: Vienna Circle, dialectics, etc. "

"The concept of the philosopher in the Vienna Circle and in the Soviet: the gendarme. "

This analogy is present in several more notes and remarks and it is perhaps a fundamental motivation to his work. It elucidates the mission Lakatos ascribes to his philosophy of mathematics that was in making in the years these notes were written.

(1) Cf. Lee Congdon, "Lakatos' Political Reawakening", in G. Kampis, L. Kvasz, M. Stöltzner (eds.), Appraising Lakatos. Kluwer, 2002.

(2) Imre Lakatos, Proofs and Refutations (ed. by J. Worrall and E. Zahar), CUP, 1976, p. 49.

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HOW CAN WE MAKE SENSE OF THE RELATIONSHIP BETWEEN ADAPTIVE THINKING AND HEURISTIC IN EVOLUTIONARY PSYCHOLOGY?

Session 16M

Congress section(s): C5

Evolutionary psychology was initiated by its pioneers as a discipline to reverse-engineer human psychological mechanisms by largely adopting a forward-looking deductive inference but has subsequently shifted toward a positivism-oriented discipline based on heuristic and empirical testing. On its course, however, the very characteristics which initially defined the methodological advantage of the discipline seem to have been lost; namely, the prospect to predict the human mental constitution from the vantage point of the ancient selection pressures imposed on our ancestors. This is what was supposed to enable the discipline to claim the methodological advantage both over sociobiology and the contemporary cognitive psychology by providing testable predictions about our psychological makeup by way of looking into its deeper root of our evolutionary past. However, with the subsequent trend to emphasize its aspect as heuristics, the roles played by such adaptive thinking has been gradually set aside.

According to Rellihan (2012), the type of adaptive thinking typical of evolutionary psychology is in fact what can be termed as 'strong adaptationism,' which is the idea that the force of natural selection is so powerful and overwhelming of any obstacles that the destination of adaptive evolution is uniquely predictable no matter what phenotypes a given population may have started with in the long past --- much stronger version than the one evolutionary psychologists typically think themselves committed to. Thus, the role of adaptive thinking played is more decisive than is normally perceived. Provided this is true, how can we make sense of the relationship between adaptive thinking and the heuristic aspect in evolutionary psychology?

In this talk, I will build on Rellihan's analysis that "Heuristics are simply less reliable inference strategies and inference strategies are simply more reliable heuristics" (Rellihan 2012) and argue that the distinction between heuristic and adaptive inference may be expedient. If heuristics are not based on largely adaptive thinking that evolutionarily makes sense, they will not bring forth meaningful hypotheses that deserve to be called evolutionary. Evolutionary psychologists make it a rule to name comparative studies, hunter-gatherer studies, or archeology as the sources of inspiration for their hypothesis generation, not just evolutionary theory (e.g., Machery, forthcoming). Still, if adaptive thinking doesn't constitute an integral part, evolutionary psychology will end up with a mere hodgepodge of heterogeneous bodies of knowledge, which makes us wonder why the whole enterprise ought to be called evolutionary.

In another line of defense, some (e.g., Goldfinch 2015) argue that the task for evolutionary psychology as a heuristic program can end with proposing some interesting hypotheses where the task for other relevant adjacent disciplines of actually confirming them starts. This 'division of labor' view of the confirmation strategy will do to some extent but may eventually risk letting go of its disciplinary integration.

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CONSTRUCTING A COMPLIMENTARY RELATION BETWEEN BAYESIAN PHILOSOPHY AND STATISTICS

Session 30F

Congress section(s): C1

Historically speaking, despite their having 'Bayes' rule' in common, Bayesian philosophy and statistics should probably be taken as having developed independently rather than diverged from a common ancestor. This is particularly conspicuous when we see how empirical Bayesian methods have flourished ever since 1960s, after Neo-Bayesian Revival was achieved (Fienberg). Although we might be able to trace back to other key persons involved in both philosophical and statistical foundations of Bayesianism, there is no guarantee that we can find directly from their thoughts a truly meaningful linkage of them. It seems more promising to search for the linkage based on how they are actually used in or could be applied to science.

Seeing Bayesianism in this way, we should recognize Bayesian philosophy, concerning updating of degree of belief, is not confined to philosophical arguments or historical reviews of past theories, but is also concerned with actual inferences made by scientists, particularly when the degree of uncertainty matters. For example, in the guidance paper for IPCC's third report, they officially endorsed a subjective Bayesian method in evaluation and prediction of the global climate change.

What is interesting about this example is that in it a distinction of analysis is made, depending on whether probability distribution is available or not. This means, they distinguish the qualitatively summarized part of their analysis, which roughly corresponds to a philosophical version of Bayesianism, from the quantitative one, which is statistical. In their view, the relation between the two is neither independent nor inclusive, but rather complementary, in that each will be required as a reference point to the other. Such a practical attitude in science might well be refined philosophically, but this can be a good model to reconstruct the linkage from scratch.

If we argue strictly along this line, some constraints need to be put on our traditional views of Bayesianism. Bayesian philosophy has long tried to establish a formal inductive inference in a complete manner based on subjective prior probabilities. But in the complementarity view, this inference should be given as a good reference to making a statistical model, not just for obtaining subjective posteriors. Then perhaps we need some constraints on the way we give priors.

Likewise, Bayesian statistics should shift their goal to a broader one in which they contribute to evaluation of higher levels of hypotheses. Stricter conditions should probably be put in determining or reexamining prior distributions, for one thing (such an attempt is partly being made by philosophical statisticians like Gelman).

In this talk, I examine how this complementarity could be achieved, by focusing on the conditions newly required for priors and also by reexamining the 'Likelihood Principle' from other perspectives than previously had.

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MARIO BUNGE AND THE ENLIGHTENMENT PROJECT IN SCIENCE EDUCATION

Session 10F

Congress section(s): B7, C7

The unifying theme of Bunge's life and research is the constant and vigorous advancement of the eighteenth-century Enlightenment project; and energetic criticism of cultural and academic movements that reject the principles of the project or devalue its historical and contemporary value. Bunge is unashamedly a defender of the Enlightenment, while over the past half-century, many intellectuals, academics, educators, and social critics have either rejected it outright or compromised its core to such an extent that it can barely give direction to the kinds of personal, philosophical, political or educational issues that historically it had so clearly and usefully addressed. In many quarters, including educational ones, the very expression 'the Enlightenment' is derogatory and its advancement is thought misguided and discredited.
This paper begins by noting the importance of debates in science education that hinge upon support for or rejection of the Enlightenment project. It then distinguishes the historic eighteenth-century Enlightenment from its articulation and working out in the Enlightenment project; details Mario Bunge's and others' summation of the core principles of the Enlightenment; and fleshes out the educational project of the Enlightenment by reference to the works of John Locke, Joseph Priestley, Ernst Mach, Philipp Frank and Herbert Feigl. It indicates commonalities between the Enlightenment education project and that of the liberal education movement, and for both projects it points to the need to appreciate history and philosophy of science.
Modern science is based on Enlightenment-grounded commitments: the importance of evidence; rejection of simple authority, especially non-scientific authority, as the arbiter of knowledge claims; a preparedness to change opinions and theories; a fundamental openness to participation in science regardless of gender, class, race or religion; recognizing the inter-dependence of disciplines; and pursuing knowledge for advancement of personal and social welfare. All of this needs to be manifest in science education, along with a willingness to resist the imposition of political, religious and ideological pressures on curriculum development, textbook choice and pedagogy.
Defense of the Enlightenment tradition requires serious philosophical work. Questions of epistemology concerning the objective knowability of the world, questions of ontology concerning the constitution of the world, specifically regarding methodological and ontological naturalism, questions of methodology concerning theory appraisal and evaluation, and the limits, if any, of scientism, questions of ethics concerning the role of values in science all need to be fleshed out, and Enlightenment answers defended against their many critics
That Enlightenment banner continues to be carried by Mario Bunge. He champions Enlightenment principles, adjusts them, and adds to them. In Latin America of the mid- and late twentieth century, he was one of the outstanding Enlightenment figures, and has been the same in the wider international academic community.

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PHILOSOPHY IN SCIENCE TEACHER EDUCATION

Session 12E

Congress section(s): B7, C7

Philosophical questions arise for all teachers. Some of these arise at an individual teacher/student level (what is and is not appropriate discipline?); some at a classroom level (what should be the aim of maths instruction?); some at a school level (should classes be organised on mixed-ability or graded-ability lines?); and some at a system level (should governments fund private schooling, and if so on what basis?).

These philosophical, normative, non-empirical questions impinge equally on all teachers, whether they are teaching mathematics, music, economics, history, literature, theology or anything else in an institutional setting.
The foregoing questions and engagements belong to what can be called general philosophy of education; a subject with a long and distinguished past, contributed to by a roll-call of well-known philosophers and educators such as: Plato, Aristotle, Aquinas, Locke, Mill, Whitehead, Russell, Dewey, Peters, Hirst and Scheffler (to name just a Western First XI).
But as well as general philosophy of education, there is a need for disciplinary philosophy of education; and for science education such philosophy is dependent upon the history and philosophy of science. Some of the disciplinary questions are internal to teaching the subject, and might be called 'philosophy for science teaching'. This covers the following kinds of questions: Is there a singular scientific method? What is the scope of science? What is a scientific explanation? Can observational statements be separated from theoretical statements? Do experimental results bear inductively, deductively or abductively upon hypotheses being tested? What are legitimate and illegitimate ways to rescue theories from contrary evidence?
Other of the disciplinary questions are external to the subject, and might be called 'philosophy of science teaching'. Here questions might be: Can science be justified as a compulsory school subject? What characterises scientific 'habits of mind' or 'scientific temper'? How might competing claims of science and religion be reconciled? Should local or indigenous knowledge be taught in place of orthodox science or alongside it, or not taught at all? Doubtless the same kinds of questions arise for teachers of other subjects – mathematics, economics, music, art, religion.
There are many reasons why study of history and philosophy of science should be part of preservice and in-service science teacher education programs. Increasingly school science courses address historical, philosophical, ethical and cultural issues occasioned by science. Teachers of such curricula obviously need knowledge of HPS. Without such knowledge they either present truncated and partial versions of the curricula, or they repeat shallow academic hearsay about the topics mentioned. Either way their students are done a disservice. But even where curricula do not include such 'nature of science' sections, HPS can contribute to more interesting and critical teaching of the curricular content.
Beyond these 'practical' arguments for HPS in teacher education, there are compelling 'professional' arguments. A teacher ought to know more than just what he or she teaches. As an educator, they need to know something about the body of knowledge they are teaching, something about how this knowledge has come about, how its claims are justified, what its limitations are and, importantly, what the strengths and contributions of science have been to the betterment of human understanding and life. Teachers should have an appreciation of, and value, the tradition of inquiry into which they are initiating students. HPS fosters this.

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MUSIC COGNITION AND TRANSPOSITION HEURISTICS: A PECULIAR CASE OF MIRROR NEURONS

Session 16M

Congress section(s): C5

The aim of my presentation is to analyse how models are constructed in contemporary embodied music cognition research. I introduce and discuss the idea of "transposition heuristic" in cognitive science (of music). Utilizing this heuristic, researchers in cognitive science tend to copy and apply ways of thinking about particular concepts from general cognitive science to their own (sub)field of research. Unless done with proper caution, however, such transposition may lead to particular problems.
I will illustrate the use of transposition heuristic with reference to contemporary works in embodied music cognition (e.g., Schiavio et al., 2015; Matyja, 2015). I will show how music cognition researchers tend to take particular concepts (e.g., imagination or simulation) from general cognitive science and apply them to their own field of research (e.g., introducing rather ambiguous concepts of musical imagination or musical simulation). Often, music cognition researchers do not see the need of specifying those concepts. They do, however, construct models on the basis of those unspecified concepts.
In my presentation I argue that transposition research heuristic employed while constructing models in embodied music cognition is often fallible. Initially, such transpositions may be inspiring. They, however, are not enough to provide exhaustive

models (the “how-actually” explanations) of how musical processing and musical imagination is embodied. I conclude that the transpositions from general cognitive science to its subdisciplines should be performed with proper caution.
The talk will be structured in the following way.
(1) I begin with introducing the general ideas behind the embodied music cognition research paradigm in cognitive science (e.g., Maes et al., 2014) and its relations to hypothesized simulative function of musical imagination (e.g., Molnar-Szakacs & Overy, 2006; Matyja, 2015).
(2) I will show that in addition to research heuristics in cognitive science already discussed in the literature (Bechtel & Richardson, 2010; Craver & Darden, 2013), a careful analysis of recent developments in music cognition research fleshes out what I dub to be the “transposition heuristics”. (3) I will show that by their nature research heuristics are fallible, sometimes leading to inadequate formulations of both research problems and corresponding theories. In order to illustrate this problem, I return to previously discussed case studies from music cognition research.
(4) I discuss the mechanistic criteria for complete and adequate explanations and show how they relate to my case studies. In particular, I show that contemporary models in embodied music cognition lack accounts on how body and its physical and spatial components (e.g., physical responses to music) shape musical processing.
(5) In the light of what has been discussed, I conclude that transpositions from general cognitive science to its particular sub-disciplines should be performed with proper caution.
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ECONOMETRIC MODELING FALSIFIES STRUCTURAL REALISM

Session 20H

Congress section(s): B4, C7

The argument put forth in this article shows that the despite the voices of Ross (2008) and Kincaid (2008), structural realism is incongruent with the realistically reconstructed project of econometrics (the empirical branch of economics).
In detail, a detailed analysis of the actual research practices and developments of econometrics indicates that strains of empirical literature devoted to many topics experience ‘reversals’ so that the relation (partial correlation in the case of linear models) between, let me say, X and Y is positive according to some studies and then newer econometric models suggest a negative sign (Goldfarb 1995; Goldfarb 1997; Maziarz 2017; Maziarz 2018). During the presentation, I will first abstract an account of empirical reversals on the ground of analyzing empirical literature devoted to the phenomenon of ‘emerging contrary result’/emerging recalcitrant result (ERR) phenomenon and later (2) show that such changes in the econometric models of data violate the set-theoretic definition of structure. Therefore, the argument presented states that structural realism is not adequate to the empirical branch of economics. Either a realist position should be elaborated on by incorporating a pragmatist dimension (Hoover 2012) or an antirealist position should be put forward.
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COUNTERFEIT CHANCE

Session 7D

Congress section(s): B4

In the postscript to “A Subjectivist’s Guide to Objective Chance”, Lewis describes a kind of “counterfeit chance”, which, while not genuine chance, is still “suitable for use by determinists” and “will do to serve the conversational needs of determinist gamblers” (Lewis 1986, 121). Recently, philosophers (Glynn 2010; Eagle 2011; Emery 2015; List and Pivato 2015) have urged that such chances are genuine, preferring the less pejorative “deterministic chance” to describe the objective probabilities invoked in a variety of applications, from humdrum coin flips to classical statistical mechanics. Briggs urges that Lewisians should in fact love deterministic chance, and she dismisses as a “non-starter” the complaint that deterministic chance is mere counterfeit chance, resembling real chances while in fact something else (Briggs 2015, 279).
I think Lewisians, and everyone else for that matter, should have a place in their hearts for counterfeit chance. Lewis called it counterfeit because he thought it was not genuine, metaphysical chance. Of course, whether we choose to name these probabilities “chances” is just a matter of convention, but whether we regard them as genuine or merely as a particular form of non-ontic, objective probability is surely a substantive metaphysical issue.
The cases I consider concern chance setups where the underlying physical model is understood to be fully deterministic. Roulette wheels, coin flips, dice rolls, and other games of chance are paradigmatic examples. In such cases, where does the probabilistic element come from? What does it represent? There is, after all, nothing genuinely chancy in nature governing the behavior of these things—we assume they operate deterministically. I claim that the probabilistic element is entirely imposed by us; it represents nothing in the system directly. One chooses, as it were, “random-looking” initial conditions of these systems or “reasonable” probability distributions over them and uses these to predict outcomes.
Allowing this degree of “subjectivity” seemingly threatens the claimed objectivity of ascriptions of probability in these contexts. How does one choose the right probability distribution over initial conditions? I show how such probabilities can be justified in two ways to make them objective probabilities. The first case depends on a particular probability distribution approximating the correct actual (non-probabilistic) distribution of initial conditions (or frequencies). The second case, the method of arbitrary functions, depends on nearly any choice of probability distribution giving the correct results. Both cases demonstrate how probabilities can be applied to describe and explain non-chancy phenomena.
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EXPLANATION IN HUMANITIES

Session 11M

Congress section(s): C7

There are two approaches in the treatment of humanities. The naturalistic one denies any substantial differences between human and natural sciences. The most consistently this line was realized in positivist philosophy, where the classic scheme of scientific explanation (Popper – Hempel scheme) was formulated. According to it, explanation of every event may be deductively inferred from two classes of statements: universal laws and sentences fixing initial conditions of an event.
Anti-naturalistic thinkers proved that understanding is a specific means of cognition in humanities. Dilthey treated it in a radically irrationalistic mode – as empathy. Such understanding was regarded by naturalists not as a genuine method, but as a heurictic prelude to explanation. Discussions on the interrelations of understanding and explanation and on the applicability of the classical scheme of explanation in humanities are still going on. Usual arguments against naturalism are following.
What is explained in humanities is not an outward object, external to us, like that of natural sciences. What is studied here (society, cultural tradition) is a part of ourselves, something that has formed us as subjects of knowledge. Social reality cannot become an ordinary object of knowledge because we belong to it. History and social life are not a performance for a subject who does not take part in it. Knowledge about people and society has transcendental character in Kant's sense: it refers to general conditions of our experience. The specific nature of subject-object relations in human and social sciences manifests itself also in the fact, that our knowledge and conception of social reality is an important part of this reality.
The universal scheme of scientific explanation is connected to the technological model of knowledge: the goal of explanation is practical use of phenomena, manipulation. To realize this model in human sciences we should have divided society into subjects and objects of knowledge and manipulation. And the latter should be deprived of the access to knowledge about themselves. After all, in contrast to other objects of knowledge people are able to assimilate knowledge about themselves and to change their behavior.
Explanation should be used in human and social sciences. But manipulation cannot be its purpose. The model of critical social sciences of Apel and Habermas presumes the use of explanational methods in a hermeneutic context. The goal here is not to explain others, but to help us to understand ourselves better. For example, Marx's and Manheim's critic of ideology gives causal explanation of the ideological illusions' formation. This explanation has the same character as in natural sciences. But the main principle of the sociology of knowledge denies the possibility of objective and neutral social knowledge. A subject of such knowledge cannot occupy an ideologically undetermined position in order to expose other's ideological illusions. Such subject should be attentive to possible social determination of his own ideas, to their possible ideological nature.
Such is the goal of human and social sciences. Explanational methods serve there general hermeneutic task – their function is to deepen human self-understanding.

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LOGIC-BASED ONTOLOGIES IN THE BIOMEDICAL DOMAIN: FROM DEFECTS TO EXPLICIT CONTRADICTIONS

Session 24D

Congress section(s): B1

The focus of this contribution will be on theories that exhibit certain kinds of defects, but that are not explicitly inconsistent. The term "theory" will be used in a broad sense to refer to any set Cn LΓ, where Γ is a set of non-logical axioms and where CnLΓ refers to the deductive closure of Γ under L. A theory Cn LΓ will be called explicitly inconsistent if and only if it contains an explicit contradiction---a sentence of the form ∃ (A∧¬ A), where ∃ A denotes the existential closure of A.

Different kinds of defects will be distinguished and it will be shown under which conditions which defects will surface as explicit contradictions. I shall argue that, although there is no general methodology for "correcting" defective theories, extending defective theories in such a way that they become explicitly inconsistent can contribute in important ways to their adequate handling. A distinction will be made between two kinds of contexts where adequate handling of defective theories is needed (those in which one is forced to live with the defects and those in which one is trying to correct some of them).
I shall argue that these contexts require different kinds of underlying logics, show that the adequate handling of defective theories benefits from a nonmonotonic approach, and discuss the prospects of the adaptive logics framework ([1]) for this.
I shall only look at a specific kind of theories in the health sciences, but argue that some of the conclusions may be generalized to other sciences and other domains. The kind of theories that I shall concentrate on fall under the category of logic-based ontologies. A key example of such an ontology for the health sciences is SNOMED CT (Systematized Nomenclature Of Medicine, Clinical Terms). SNOMED CT is currently considered to be the most comprehensive clinical healthcare ontology (more than 300.000 active concepts and millions of domain-specific relations between these concepts) and covers clinical findings, symptoms, diagnoses, procedures, body structures, organisms, substances, pharmaceuticals, devices, ... SNOMED CT is based on the description logic EL (a decidable, highly inexpressive fragment of first-order logic---no negation, no disjunction, and only very restricted use of the existential quantifier) and one of its main aims is to support the recording of data in the health sciences, and more specifically to provide the terminological backbone for electronic health records.
Because of the inexpressive nature of its underlying logic, SNOMED CT is not (and will never be) explicitly inconsistent. Still, SNOMED CT exhibits different kinds of defects. Based on the literature, but without aiming at an exhaustive taxonomy, I shall discuss five classes of defects---false subsumptions (IsA relations), false relations other than subsumptions (PartOf-relations, DueTo-relations, ...), structural inconsistencies, ambiguous concepts, and inconsistent concepts. I shall say that a subsumption or other relation is false when it is not compatible with accepted clinical practice and use "structural inconsistency" to refer to any violation of basic rules for the organisation of hierarchies (for instance that equivalent concepts should not have different parents). The term "ambigous concept" will be used to refer to cases where a single concept of SNOMED CT is ambiguous between two or more meanings in clinical practice and "inconsistent concepts" to refer to concepts that, in view of some implicit assumption behind SNOMED CT (for instance, that at each level in the taxonomy siblings exclude one another), cannot have instances. I shall discuss recent proposals to strengthen the underlying logic of SNOMED CT (see, for instance, [2]), show that such a strengthening will result in a theory that is explicitly inconsistent, discuss the advantages that this may have for its adequate handling, and consider the question which kinds of logics are most suitable for this handling.
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USING LINGUISTIC CORPORA TO UNDERSTAND MATHEMATICAL EXPLANATION

Session 15A

Congress section(s): C1

The notion of explanation in mathematics has received a lot of attention in philosophy. Some philosophers have suggested that accounts of scientific explanation can be successfully applied to mathematics (e.g. Steiner 1978). Others have disagreed, and questioned the extent to which explanation is relevant to the actual practice of mathematicians. In particular, the extent to which mathematicians use the notion of explanatoriness explicitly in their research is a matter of sharp disagreement. Resnik and Kushner (1987, p.151) claimed that mathematicians "rarely describe themselves as explaining". But others disagree, claiming that mathematical explanation is widespread, citing individual mathematicians' views (e.g., Steiner 1978), or discussing detailed cases in which mathematicians explicitly describe themselves or some piece of mathematics as explaining mathematical phenomena (e.g. Hafner & Mancosu 2005). However, this kind of evidence is not sufficient to settle

the disagreement. Recently, Zelcer (2013) pointed out that a systematic analysis of standard mathematical text was needed to address this issue, but that such analysis did not exist. In this talk we illustrate the use of corpus linguistics methods (McEnery & Wilson 2001) to perform such an analysis.

We describe the creation of large-scale corpora of written research-level mathematics (obtained from the arXiv e-prints repository), and a mechanism to convert LaTeX source files to a form suitable for use with corpus linguistic software packages. We then report on a study in which we used these corpora to assess the ways in which mathematicians describe their work as explanatory in their research papers. In particular, we analysed the use of 'explain words' (explain, explanation, and various related words and expressions) in this large corpus of mathematics research papers. In order to contextualise mathematicians' use of these words/expressions, we conducted the same analysis on (i) a corpus of research-level physics articles (constructed using the same method) and (ii) representative corpora of modern English. We found that mathematicians do use this family of words, but relatively infrequently. In particular, the use of 'explain words' is considerably more prevalent in research-level physics and representative English, than in research-level mathematics. In order to further understand these differences, we then analysed the collocates of 'explain words' –words which regularly appear near 'explain words'– in the two academic corpora. We found differences in the types of explanations discussed by physicists and mathematicians: physicists talk about explaining why disproportionately more often than mathematicians, who more often focus on explaining how. We discuss some possible accounts for these differences.

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EXPANSIONS OF RELEVANT LOGICS WITH A DUAL INTUITIONISTIC TYPE NEGATION

Session 17M

Congress section(s): A2

Abstract. Da Costa's paraconsistent logic Cω is axiomatized by adding to positive intuitionistic logic H_+ the "Principle of Excluded Middle" (PEM), $A \vee \neg A$, and "Double Negation Elimination" (DNE), $\neg\neg A \rightarrow A$ (cf., e.g, [2]). Richard Sylvan (*né* Routley) notes that "Cω is in certain respects the dual of intuitionistic logic" ([4], p. 48) due to the following facts (cf. [4], pp. 48-49). (1) Both Cω and intuitionistic logic H expand the positive logic H_+; (2) H rejects PEM but accepts the "Principle of Non-Contradiction" (PNC), $\neg(A \wedge \neg A)$; and (3) H accepts "Double Negation Introduction" (DNI), $A \rightarrow \neg\neg A$, but rejects DNE. Sylvan adds ([4], p. 49) "This duality also takes a semantical shape: whereas intuitionism is essentially focused on evidentially incomplete situations excluding inconsistent situations, the C systems admit inconsistent situations but remove incomplete situations."The aim of this paper is to define an unreduced Routley-Meyer semantics for a family expansions of minimal De Morgan relevant logic B_M with a basic dual intuitionistic negation in Sylvan's sense. In order to fulfill the aim stated above, we shall proceed as follows. First of all, it has to be remarked that it is not possible to give an RM-semantics to logics weaker than (not containing) Sylvan and Plumwood's minimal De Morgan logic B_M (cf. [1]). Consequently, the minimal dual intuitionistic logic in this paper is the logic Db, which is the result of expanding B_M with a basic dual intuitionistic negation in Sylvan's sense ("D" stands for "dual intuitionistic negation"; and "b" for basic). Once Db is defined, we shall built a family of its extensions included in a dual intuitionistic expansion of positive (i.e., negationless) Gödelian logic G3 (cf. [5]), which can be here named $G3^D$. All logics in this family are given an unreduced RM-semantics w.r.t. which they are sound and complete. Also, all logics in this family are shown paraconsistent in the sense that there are non-trivial inconsistent theories definable upon each one of them. Finally, it will be proved that the dual intuitionstic negation introduced in this paper and the De Morgan negation characteristic of relevant logics are independent in the context of $G3^D$. It has to be noted that Sylvan's extension of Cω, CCω, does not include Db and, consequently, neither does it contain any of the logics defined in the paper.

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HOW ARE MATHEMATICAL STRUCTURES DETERMINED

Session 17K

Congress section(s): C1

Despite rather general consensus among the philosophers of mathematics that the objects of mathematics are structures (patterns, structural possibilities) (cf. e.g. Hellman 2005, Horsten 2012, Landry 2016, Shapiro 2016), there seems to be some disagreement about how mathematical structures are determined.
The traditional view reflects that since all concepts of an axiomatized mathematical theory are defined implicitly, by their mutual relations specified by the axioms, the whole theory is nothing but a general structure determined by these relations. The meaning of all the concepts being strictly internal to the theory, their names being arbitrary tokens. As Hilbert neatly put it, “every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and ... the basic elements can be thought of in any way one likes,” (cf. e.g. Reck and Price 2000, Shapiro 2005).
The potentially competing view is connected with the modern abstract algebra of category theory. Some categories may be interpreted as domains of objects sharing some particular mathematical structure (e.g. group structure) taken together with their mutual (“structure-preserving”) morphisms. Despite their original motivation, within any category, the objects are primitive concepts, lacking any “internal properties”, determined strictly by their mutual relations in the form of the category morphisms. Given a category, say, of all groups and group homomorphisms, we can define the group structure wholly in categorial terms, without ever invoking the “group elements” or the “group operations”. The advantage is that by avoiding to mention its “underlying substance”, structures are determined without any unnecessary “non-structural” connotations. Moreover, in this way the relevant notion of isomorphism is also automatically obtained (cf. e.g. Awodey 1996, Landry and Marquis 2005).
Exponents of the respective views regard them as competing (Shapiro 2005, p. 68), even “radically different” (Awodey 2013, p. 4). I want to argue that the difference is rather in the question asked than in the actual answer provided. There are two levels on which we can consider determination of mathematical structures. First, any theory determines a structure. There exist many models of a theory -- actual mathematical systems satisfying the structural requirements of the theory. These models are not mutually isomorphic and each exhibits some properties irrelevant vis-a-vis the given theory. It is on us, human mathematicians, to ascertain that a given concrete system matches the structural requirements of the given theory. To do this, we have to step outside of the theory proper; in relation to this theory, we enter the realm of meta-mathematics. Second, if we want mathematically to speak about structures, we need to stay within some theory which means we have to embed them into its structure. As objects of a theory, they are determined strictly by their positions within the whole structure of the theory. Although being particularly apt for this purpose, category theory does not differ in this sense from other theories (c.f. Resnik, 1997, p. 201--224). To describe a structure as a position within another mathematical structure, without invoking other underlying structures, and without overstepping the limits of mathematics proper, constitutes a laudable exercise. Category theory in particular is instrumental in this. Yet, to start determining structures using a theory, we need to grasp the structure of the theory to begin with. To determine its structure cannot, ultimately, be relegated to another mathematical theory. Moreover, the theory can only determine structures of the same or lesser complexity: we have to be operating within a more complex structure than the one we want to mathematically determine.

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THE PLURALIST CHEMISTRY AND THE CONSTRUCTIONIST PHILOSOPHY OF SCIENCE

Session 6J

Congress section(s): B1, C2

Recent philosophy of chemistry promotes the view that chemistry is inherently pluralist (e.g. van Brakel, Chang, Lombardi, Ruthenberg, Schummer), the pluralism in it being due, firstly, to its character as a science producing a multitude of new entities and thence new constellations in the world; and secondly, the factual plurality of aims of chemical inquiry and thence of research methods. I put this pluralism into my acquired philosophical background – the constructive realist account of science. Here I focus primarily on Rein Vihalemm’s methodological pluralism and practical realism, and Ronald Giere’s perspectival pluralism and model-based account of science. According to Vihalemm, chemistry has a dual methodological nature: it is at once an exact science and a natural science, the latter aspect related to the “stuffness” of its research object – the substances. His practical realist insistence converges with Giere’s perspectival pluralism which leans mainly on the plurality of scientific apparatus and their interactions with the research object, rendering varying representations of it. In chemistry, one of the essential research apparatuses is the substances themselves, which are at the same time the object of research and hence the part of the world modelled. Substances have (potentially) a plurality of (types of) interactions among them, with the other laboratory apparatuses and methods, and with the rest of the world. Those engender various aims of modelling, and necessitate different research methods. This plurality serves not only pragmatic, but also epistemic aims, hence we should embrace a pluralist stance toward chemistry.
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WHAT CAN PUBLICATION RECORDS TELL ABOUT THE GENDER GAP IN STEM?

Session 3D

Congress section(s): B1, B5

A solid publication record is a key factor for a successful academic career.
Various studies have revealed a systemic gender imbalance in the publication distribution of scientists in various fields.
In the interdisciplinary project “A Global Approach to the Gender Gap in Mathematical, Computing, and Natural Sciences: How to Measure It, How to Reduce It?” we use various large data sources to study publication patterns in particular in mathematics, physics and astronomy with respect to gender, and across countries and regions. In this talk we will present first results in the project and discuss what the differences in publication behaviour can tell us about the current state and possible future developments of the gender gap in the respective fields.

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AVATARS OF GENERALITY: ON THE CIRCULATION AND TRANSFORMATION OF LIST-MAKING PRACTICES IN THE CONTEXT OF ENUMERATIVE GEOMETRY

Session 28J

Congress section(s): B3, B6, C1

The talk will examine how a cultural practice, namely that of list-making, circulated from one scientific culture to another, and the factors in relation to which it underwent transformations. In the 1860s, French geometer Michel Chasles created a ‘theory of characteristics’ that, after German mathematician Hermann Schubert’s 1879 “Kalkul der abzählenden Geometrie” soon became the bedrock of a branch of geometry known as ‘enumerative geometry’. In the last years of his life, Chasles expanded on the methods at the heart of this theory and published dozens of papers, filled with thousands of propositions, each expressing a property of a geometrical curve. Following a practice that was closely related to his ideal about mathematical knowledge, Chasles merely listed these propositions one after the other, and sorted them into various categories, with very little in the way of commentaries, proofs, or examples. These lists were hardly read or referenced by anyone, save for Schubert himself, who expanded on Chasles’ results and created a symbolic calculus that would enable him to enumerate the curves satisfying various geometrical conditions. Schubert’s book inherits from Chasles’ list-making practice, but also alters it, as the lists it displays consist of huge tables of numbers and symbolic formulae, given without verbal descriptions or explanations.
Chasles’ and Schubert’s lists aim to address the same geometrical problems, but they differ both in their textuality and in the epistemic tasks they fulfil. Indeed, Chasles’ must be read against the backdrop of a specific epistemic culture, which one could trace back to the education he received at the Ecole Polytechnique. In Chasles’ epistemology of geometry, the generality of a method is demonstrated by the fact that large numbers of propositions can be derived, almost without proof, from its systematic and uniform application. In this case, generality was expressed through a specific list-making practice. Schubert, instead, viewed Algebra as a free human creation, bounded only by the requisite that certain symbolic forms, drawn from the realm of concrete, natural numbers, be regarded as valid when extended to more abstract entities. Consequently, Schubert’s lists of formulae answer to a different calling than Chasles’: they express the formal rules of a geometrical calculus, and become meaningful and relevant only once viewed through the lens a different set of epistemic virtues.
Therefore, as Chasles’ lists reached and informed Schubert’s mathematical practice, the transfer between epistemic cultures resulted in their rewriting. This transformation operates at the levels of both the textuality itself and the status of computations that these lists enable, extending to the values of generality they embody and even the images of Algebra they reflect. By untangling the complexities of this transformation, this case-study illuminates how the literary devices used to structure and convey mathematical knowledge change according to the concerns and values of different scientific cultures, and how such an investigation into changes in mathematical styles can shed new light on the transformations of the form and structure of mathematical knowledge.

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AN ATTEMPT AT EXTENDING THE SCOPE OF MEANINGFULNESS IN DUMMETT’S THEORY OF MEANING.

Session 12H

Congress section(s): C1

Michael Dummett proposed a radically new approach to the problem how the philosophical foundations of a meaning theory of a natural language are to be established. His central point is threefold. First, a theory of meaning should give an account of the knowledge (i.e., understanding) that competent speakers of the language have of it. Second, the knowledge consists in certain practical abilities. If someone counts as a competent speaker, it is because, by using the language, she/he is able to do anything that all and only those who understand the language can do. Therefore what a theory of meaning should account for is those practical abilities that a competent speaker. Then, what do those practical abilities consist in? This question leads us to Dummett’s third point. Ordinarily, one is entitled to possess some ability by exhibiting (making manifest the possession of) the ability. : i.e., by having done, often doing or being likely to do something that can be done by virtue of the ability. Truly, there is an intricate problem of what one should do to be entitled to possess the ability. Let us set aside the problem. Dummett tackled with another (related but more profound) problem: in almost all natural languages and formalized languages, there are various sentences that are, while well-formed and hence associated with certain precise conditions for them to be true, definitely beyond the scope of possible exhibition of those abilities that (if there were any at all) the understanding of the sentences would consist in.
He objected to the common opinion that meaning of a sentence could be equated with its truth-conditions and instead claimed that the meaning should be accounted for as consisting in its (constructive) provability condition, that is, according to Dummett someone knows the meaning of a sentence just in case he knows what has to be done (what construction has to be realized) to justify the sentence (i.e. to establish constructively the sentence holds.)
I basically agree with these lines of Dummett’s thought, although I should point out that his view on the scope of meaningfulness (intelligibility) of sentence is too restrictive. Dummett proposes that in giving provability conditions of a sentence we should adopt the intuitionistic meaning condition of the logical connectives. The reason is that the intuitionistic connectives are conservative with respect to constructivity: If sentences derived intuitionistically from some assumptions, then the sentence is constructively justifiable provided those assumptions are. However, I think we can point out there are some sentences that are while beyond the criterion, that can be established by virtue of an agent’s behavior that conclusively justifies the sentence. In that case the agent’s behavior could be said to make her understanding of sentence manifest. One of the typical examples of such sentences is, one might say certain kind of infinitary disjunctions that are treated prominently by the proponents of the geometric logic such as S.Vickers. I will investigate into the matter more closely in the talk.

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TOWARDS AN ANALYTIC SCIENTIFIC METAPHYSICS

Session 30H

Congress section(s): B4

In the last decades, philosophy of science often concentrated its attention on abortive epistemological problems: scientific explanations as different from scientific descriptions, scientific truth, the relation between phenomena and data of science, the relation between scientific theories and scientific models; etc. As a result, the interest towards philosophy of science waned. (Don Howard)

Analytic metaphysics, that follows a priori methods, was advanced as an alternative to this development. Unfortunately, it fails to keep touch with the latest achievements of science. As a result, it doesn't really contribute to human knowledge. (Ladyman & Ross)
The present study tentatively tracks down an alternative direction of this sub-discipline. Against these developments, we suggest a program for scientific metaphysics that closely follows the new results of science but at the same time also uses the method of conceptual analysis. To be more exact, we see its objective as advancing new ontologies of the phenomena investigated by science that are elaborated with logical means.
In support of what we mean as analytic scientific metaphysics we shall draw the idea of "connective" conceptual analysis. Modified this way, scientific metaphysics holds, among other things, that every new significant discovery in science can be conceptually connected with discoveries of other sciences. There are clear historical precedents of this approach. An example: Inspired by Ernst Cassirer, in the 1920s Kurt Lewin developed the concept of "genidentity", adopted by both Carnap and Reichenbach, this "evangelist of science" (van Fraassen). It was applied interdisciplinary, both in physics and in biology. Further historical remarks go in order at this place. After Hegel, philosophers were—correctly—shy to intervene in the scientific theories. In fact, they effectively stopped to explore scientific problems head on. (Hempel 1966) Unfortunately, the result is that nowadays "philosophy is dead". (S. Hawking) Our project zeroes on exactly this problem, without, however, to try to develop autonomous "philosophical" science.
In support of our project we refer to the pioneer of the "scientific method in philosophy", Bertrand Russell. Among other things, he maintained that "philosophy consists of speculations about matters where exact knowledge is not yet possible" (Bertrand Russell speaks his mind, p. 11); and that philosophy "should be bold in suggesting hypothesis as to the universe which science is not yet in a position to confirm or confute" ("Logical Atomism", p. 341). Scientists develop some of these speculations and hypotheses further examining their application to reality.

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COMMENT ON "POPPER ON THE MIND-BRAIN PROBLEM"

Session 12C

Congress section(s): B6

In a wide-ranging interview (Popper, Lindahl, & Århem 1993) published near the end of his life, Popper drew attention to several similarities between the unconscious mind and forces or fields of forces: minds, like forces, have intensity; they are located in space and time; they are unextended in space but extended in time; they are incorporeal but existing only in the presence of bodies; and they are capable of acting on and being acted on by bodies (what he elsewhere called 'kicking and being kicked back', and proposed as a criterion of reality). Granted these similarities, Popper proposed that the unconscious mind should be understood literally as a field of forces. A related idea, extending also to the conscious part of the mind, was proposed also by Libet (1996), who elucidated in his (1997) the connections between what he described as 'Popper's valuable hypothesis' and his own.
In this comment on the lead paper 'Popper on the Mind-Brain Problem', I hope to explore some similarities between these theories of minds as force fields and the proposal that the propensities that are fundamental to Popper's propensity interpretation of probability should be likened to forces. This latter proposal was made indirectly in one of Popper's earliest publications on the propensity interpretation, but never (as far as I am aware) very decisively pursued. Instead, in A World of Propensities (1990), Popper adopted the idea that propensities (which are measured by probabilities) be likened to partial or indeterministic causes. It will be maintained that this was a wrong turn, and that propensities are better seen as indeterministic forces. There is nothing necessitarian, and there is nothing intrinsically unobservable either, about forces.
One of Popper's abiding concerns was the problem of how to account for human creativity, especially intellectual and artistic creativity. The speaker rightly notes the centrality of 'the Popperian thesis that present-day physics is fundamentally incomplete, i.e. the universe is open'. But this is hardly enough. It is not hard to understand how propensities may be extinguished (that is, reduced to zero) with the passage of time, but harder to understand their initiation and generation. It may be that the identification of propensities with forces, which disappear when equilibrium is achieved and are at once revived when equilibrium is upset, may help to shed some light on this problem.

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THREE PROBLEMS WITH THE IDENTIFICATION OF PHILOSOPHY WITH CONCEPTUAL ANALYSIS

Session 14J

Congress section(s): B1, B4

In this paper I am going to argue that, although we do conceptual analysis when we do philosophy, the world plays a central role in the constitution of our philosophical concepts, statements and theories. Particularly, their meaning is partially determined by the way the world is. I will object to what the advocates of conceptual analysis, specifically the members of the Canberra Plan (Jackson and Chalmers (2001); Jackson, F., (1998)) –who, arguably, have advanced the most influential antinaturalistic metaphilosophical view in our days– consider to be the conceptual elements of philosophical practice: the two steps of philosophical analysis and the deductive implication of the folk vocabulary by the scientific one. I will advance three main problems for the purely conceptual and aprioristic status of these components:
(P1) Science also does conceptual analysis (Jackson, 1998; Larkin, McDermott, & Simon, 1980; Tallant, 2013).
(P2) Philosophy also depends on the world, which is known by us through observation and experimentation (specifically, our implicit folk theories depend on the world (Arthur, 1993; Chassy & Gobet, 2009; Goldman, 2010).
(P3) The deduction of the folk and philosophical vocabulary from the vocabulary of the sciences presupposes factual and a posteriori elements (Williamson, 2013).
The main conclusion is that even if we agree that philosophy does conceptual analysis, empirical evidence has shown us that philosophy still depends on the way the world is. So, conceptual analysis doesn't differentiate philosophy –neither in method, nor in subject matter– from scientific practice in the way that the conceptual analysts wanted it to. The world partially determines, a posteriori, the nature of the two-step methodology of conceptual analysis. Therefore, the possible identification of philosophy with conceptual analysis cannot establish a difference in kind between philosophy and science, be it semantic or epistemic. This leaves us with the problem of explaining why these activities seem so different. I think that this question can be seen as a matter of degree, but this will be the subject for another conference.

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AN ATTEMPT TO HIGHLIGHT AMBIGUITIES IN APPROACHES TO RESOLVE CHISHOLM PARADOX

Session 8M

Congress section(s): A2

This paper focuses on Chisholm's paradox and offers to resolve some ambiguities concerning a philosophical comparative analysis using two logical frameworks in which the paradox can be formalized. In the standard system of deontic logic, Chisholm paradox is one of the most interesting and challenging paradox among all. The paradox, ever since its discovery, has been shown to affect many if not most deontic systems. Various approaches like Van Fraassen's dyadic deontic approach, Peter L. Mott's counterfactual conditional approach etc. were formulated to avoid inconsistency in the formal representation of Chisholm set of sentences. The aim of this paper is to focus the ambiguities lying in the above approaches and to highlight a serious problem in Judith Wagner's attempt while resolving the paradox. Judith Wagner argued that Peter L Mott's solution is unsatisfactory as it allows general factual detachment. Here, I shall argue that Wagner's solution faces serious challenges when it comes to fulfill minimal adequacy conditions and shifting the operator can only temporarily fix the problem. In this paper, first I will discuss the approaches and then attempt to resolve respective ambiguities in them.

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INTEGRATING PERSPECTIVES: LEARNING FROM MODEL DIVERGENCE

Session 2

Congress section(s): B1

Science deals in the currency of representational models that describe relations thought to hold among natural phenomena. I will argue for the necessity of model pluralism, based on the partiality of scientific representation. Given there are many models, what are their relationships to each other? Reduction/Unification and Elimination/Inconsistency have been richly explored in philosophy of science. Inspired by Giere, I develop an account of perspectivism that provides new resources for understanding a dynamic, integrative form of model-model relationship. As an example I will consider practices in experimental structural biology to detail how divergent protein models can and are integrated to yield more accurate predictions than either perspective could deliver by itself. Jointly refining the inferred representations from x-ray crystallography and nuclear magnetic resonance spectroscopy relies on and preserves the plurality of scientific models.

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THE INDEPENDENCE OF EXCLUDED MIDDLE FROM DOUBLE NEGATION VIA TOPOLOGICAL DUALITY

Session 28C

Congress section(s): C1

In standard approaches to logic, the Law of the Excluded Middle (LEM) is equivalent to the Law of Double Negation (LDN). A careful look at the proof that one of the laws renders the other admissible shows that, in fact, LDN from LEM hinges on a third law: the Law of Non-contradiction (LNC). Moreover, the proof in the other direction can be adapted to show that LDN renders both LNC and LEM admissible. In very rough summary, LEM+LNC = LDN.
We review topological semantics for propositional logic (essentially, Stone duality) and locate the actual source of the coincidence LEM+LNC=LDN in the zero-dimensionality of topological semantics for propositional logic. By a suitable generalization of Stone duality, moving to general compact Hausdorff spaces, we obtain a category of logical systems in which LDN holds, but neither LEM nor LNC do. The well-known formalization of intuitionistic logic (in Heyting algebra) already shows that it is possible to have LNC without LEM, the dual of this also shows that it is possible to have LEM without LDN.

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NATURAL DEDUCTION RULES AS MEANS OF KNOWING

Session 10I

Congress section(s): C1

Does deductive reasoning has any capability to provide new knowledge? This doubt is prompted by a perceived tension between the conditions for validity of a deductive argument and the conditions for the argument's ability to deliver new knowledge. Validity requires the conditions of truth of the premises of the argument to be included in the conditions of the truth of the conclusion. So, in case of a valid deductive argument when the premises are true the conclusion is also thereby true. On the other hand, for the argument to deliver new knowledge the conclusion has to carry some information which is perceivably not carried by the premises – in which case the truth of the premises is not a guarantee for the truth of the conclusion. This condition for delivering novelty seems to go against the condition for validity.
One resolution to this lies in seeing that there is a difference between truth and knowledge of truth. Truth of premises, in a valid case, also provides truth of the conclusion; but knowledge of the truth of premises does not necessarily provide knowledge of the truth of the conclusion. This answer, however, commits oneself to a realist theory of truth and meaning: meaning of a sentence is its truth-conditions, and the truth-conditions are constituted by relevant objects and their properties irrespective of our knowledge of them. That is why the truth-conditions of a premise can also be the truth-conditions of the conclusion – without us being in a position to know the truth of the conclusion while knowing the truth of the premise. This because the (accredited) means of knowing the truth of the premise may not be the (accredited) means of knowing the truth of the conclusion. New knowledge through the validly available conclusion can now be seen to be forced upon us as a decision to accept the truth of the conclusion.
This paper explores an alternative by looking at the role of rules of inference as formulated, say, in terms of the natural deduction rules (cf. Dag Prawitz, Natural Deduction, Almquist and Wiksell, Stockholm, 1965, p. 20). The natural deduction rules formulated for the individual logical constants can be seen to be not only rules (permissions) for recording validly inferable conclusions from premises, but also as rules (permissions) for proving (knowing) sentences using the constants in question. The rules then are also the (only) accredited means of knowing (proving) (the truth of) sentences using the respective constants. No other means of knowing the truth of sentences using the constants are recognized as available in the language. This understanding of rules accommodate capability of delivering novelty along with validity.
But here is a caveat. Natural deduction systems and their cognates like sequent calculus, etc., are studied with proof-theoretic motivations. Questions about the proof-theoretic justification of such rules (conditions of uniqueness, conservative

extension/harmony, and stability) lead to the prospect of having intuitionistic logic as the justified logic – which in turn tells us to abandon realism.

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THE COMPUTATIONAL EFFECTIVENESS OF GEOMETRIC DIAGRAMS

Session 7C

Congress section(s): C1

Mathematical proofs are often presented with notation designed to express information about concepts the proofs are about. How is such mathematical notation to be understood, philosophically, in relation to the proofs they are used in?
A useful distinction in approaching the question is that between informational and computational equivalence, formulated in (Larkin and Simon, 1987). Informational equivalence holds between two representations if and only if "all of the information in the one is also inferable from the other, and vice versa." (ibid., p. 67) Computational equivalence holds between two representations if and only if "they are informationally equivalent and, in addition, any inference that can be drawn easily and quickly from the information given explicitly in the one can also be drawn easily and quickly from the information given explicitly in the other, and vice versa." (Ibid., p. 67). Applying this to the case of mathematical proofs, we can take informational equivalence to be determined by the content of proofs as revealed by a logical analysis, and computational equivalence to be determined by the notations used to express the content of proofs.
In what is perhaps the standard view, we need only consider mathematical proofs modulo informational equivalence in developing a philosophical account of them. The capacity of a notation to present a proof more clearly and effectively is a pragmatic matter, something perhaps for psychologists to consider, but not philosophers. An alternate view, in line with philosophical work attempting to illuminate mathematical practice, regards distinctions of computational nonequivalence as philosophically significant. On such a view, the structure of mathematical methods and theories is inextricably linked with the way the mind takes in and holds mathematical information.
I aim to make some small steps in elaborating the second view by applying some observations made by Wittgenstein in Remarks on the Foundations of Mathematics to the diagrammatic proofs of elementary geometry. A central challenge in elaborating the view is being precise about what, exactly, the effectiveness of effective mathematical notations amounts to. The notion of informational equivalence has the well developed philosophical resources of logic to support it. No such philosophical resources exist for the notion of computational equivalence. The only option available for investigating it at present would seem to be a bottom-up, case study approach. Accordingly, one looks at cases that dramatically illustrate computationally nonequivalent notations and aims to articulate the features that account for the nonequivalence. Wittgenstein can be understood to be doing exactly this in section III of RFM. The cases that figure in his discussion are proofs of arithmetic identities like 27 +16 =43. He in particular contrasts calculations of such identities using Arabic numerals with derivations of them using a purely logical notation, and identifies features that recommend the former over the latter from the perspective of computational effectiveness. After presenting Wittgenstein's observations, I contrast diagrammatic and purely sentential proofs in elementary geometry and argue that analogous observations distinguish the former as computationally effective.
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AGAINST REDUCTIONISM: NATURALISTIC METHODS IN PRAGMATIC COGNITIVE SOCIOLOGY

Session 24K

Congress section(s): C7

We all need space for thought, for debating, for reading, for creating. Naturalistic studies on cognition are paramount in sociology (Schutz, 1973; Clark, 2008, Sennett, 2004; Muntanyola-Saura, 2014). However, the deeply influential culturalist branch of cognitive sociology (CCS) reduces cognition to a cognitivist psychological level (Lizardo & Strand, 2010). Individuals are supposed to react automatically, with no systematicity nor reflexivity, to external stimuli from the social environment. The socialization process and the linguistic and conceptual content of thought becomes secondary. We pinpoint here the key ontological, epistemological, and methodological mistakes of CCS and provide some theoretical alternatives from pragmatic sources. We claim that it is possible for the social sciences to draw from a naturalistic paradigm on cognition without falling for reductionistic or atomistic accounts. Following Kirsh (1995) and Searle (2010) the CCS cognitivist position is readily refutable. Decision-making as defined by CCS is an unconscious, rule-following, individual activity that is not directly shaped by social factors. However, cognition is not only a local psychological product, but is a part of an institutional context. As Feyerabend (1987) puts forward, cultural cognition is a historical legitimate practice of citizens that haven been socially recognized as performers of a particular role. An individual steps out of the constrained functional role of a human trader or robot as defined by CCS by engaging in a conversation-friendly framework. Subjective cognition becomes legitimate in terms of judgement if it is publicly shared in argumentation. Moreover, conversation is an everyday tool for legitimation (Berger, 2016). Pierce's principle of habitualization (Talisse and Hester, 2004) builds everyday conversation and practice. The rules of the trade of real life cognition happen in interactions where participants are trying to know more about the the sensorial or conceptual object that is being aprehended (Lieberman, 2013). Cognitive actors are dependent on an institutional context that is always fleeting. So cognitive decision-making varies in space and time, and is based on reciprocity in a mutual determining relationship (DeNora, 2014). Ethnographic and ethnomethodological work on artists, experts and laypeople alike show how cognition takes place in a circle of distributed attention. Judgment comes with a shared act of attention (Hennion, 2005). Dialog is multimodal (Alac, 2017) and selective. Artists and experts develop a public language among themselves, and they do so by filtering and sharing their individual experience. In James' (1890: 139) words, consciouness is at all times a selecting agency. The rules of talking include evaluation and classification. Every conversation must either converge into consensus or diverge towards disensus. Knowing is to know in the first instance collectively rather than individually (Talisse and Hester, 2004). The inferences and/ or judgments that we form progressively on interactions are transformed in structural accounts (Cicourel, 2002). In other words, the actuality of the exchange and the availability of judgement shape the sequentiality of cognition Judgement happens at the present moment and doesn´t necessarily preexist at the neuronal or individual level.In all, the pragmatic paradigm is able to capture the detail of cognition and break down its specific dimensions as a valid object for the social sciences.
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NOTATIONS & TRANSLATIONS AS CATALYSTS OF CONCEPTUAL CHANGE

Session 28J

Congress section(s): B3

In this talk I consider the role played by novel systems of notation, both as obstacles to direct translations as well as catalysts for conceptual change. Before we can answer the question, "How does knowledge travel?," we must consider whether knowledge is a peripatetic thing. The idea that knowledge travels may mislead us into thinking that knowledge divides into so many seeds that may be carried away from one place only to be planted elsewhere. The image may be extended to absurdity as we imagine Euclid's Elements going into Adelard of Bath's baggage only to emerge, much the same as it went in, except in Latin rather than Arabic, in England rather than Spain. However, the more we study the history of mathematics, the more we realize that knowledge is not so much transmitted as transmuted and but rarely preserved unaltered over time. Ideas are not so much shared as appropriated; credit may not be given where due. Priority disputes erupt often but are only possible when each disputant claims to be the first to have gotten hold of the same piece of knowledge: "I entertained a somewhat similar notion at roughly the same time" is not how these arguments go. Taken too far, the distinction between the knower and the known misleads in other ways. For example, after Newton and Leibniz both discover the calculus, some might think that their followers had naught but nationalism to motivate their choice of notation as the mathematics itself was otherwise the same; those on the continent were lucky their guy had the more elegant and nimble symbolism. But as more recent scholars have shown, mathematical differences between Newton's method of fluxions and Leibniz's infinitesimal calculus support distinct conceptual approaches to the subject. Looking back further, to Adelard's introduction of Arabic numerals to his European colleagues, we should question whether "widespread reluctance to adopt the new symbolism" should survive as an example of how cultural biases inhibit mathematical progress. This simple story seems unlikely now that scholars have shown how cross-cultural social networks had different degrees of enthusiasm for the newly imported system depending on how they put these numerals to use. At first my remarks may seem to undermine the history of rational inquiry by binding knowledge so tightly to particular communities of knowers such that only insiders ever know anything. However, I conclude there's a case to be made for the growth of mathematics as a complex whole, if not its safe passage as easily packaged and readily transported knowledge seeds.

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PSYCHOLOGY, ANTHROPOLOGY AND DELUSIONS

Session 18I

Congress section(s): C7

The account of delusions that is standard within psychiatry is doxastic; it sees delusions as pathological beliefs. As such, it resembles familiar projects in philosophy of science and epistemology that are concerned with justification. Where the philosophical tradition has sought for the properties that legitimate beliefs, theorists within psychiatry try to find that extra property of a false belief that converts it from a false belief into a delusion. There should be something that not only fails to legitimate a belief but also shows it to be pathological. It is also current wisdom in psychiatry that delusions are part of psychotic conditions that can be found everywhere in the world. One way in which that could be true is if there exists a universal human epistemology, part of our shared cross-cultural psychology that prescribes and polices epistemic norms.

Turri (forthcoming) has argued that folk epistemology is a human universal, built around a "core primate knowledge concept" that we have inherited from our simian ancestors. Turri argues on the basis of comparative psychological evidence that this core knowledge concept is that of "truth detection (across different sensory modalities) and retention (through memory) and may also include rudimentary forms of indirect truth discovery through inference. In virtue of their evolutionary heritage, humans inherited the primate social-cognitive system and thus share this core knowledge concept". He concludes that "humans possess a species-typical knowledge concept". Turri also appeals to experimental philosophy, adducing results (e.g Machery et al. 2015) that suggest the existence of cross-cultural agreement on knowledge judgements. Mercier and Sperber (2017) also argue that the faculty of reason is a human universal
If Turri is right, there is a universal knowledge concept common to members of all human communities in virtue of their shared primate ancestry. If that's true we can expect the attribution of delusion to be very widely shared. The conjecture would be that every human group is made up of people who inherited the primate knowledge concept and that they have an interest in policing it. Concepts mean norms, and norms mean monitors. People who do not detect and transmit the truth might be corrected or at least marked out by the rest of the group, and perhaps incorrigible failures would be graded as pathological. So, if there is a shared human knowledge concept then we should expect to see a shared folk epistemology, and a shared interest in the people who fail to respect it.
I agree that some epistemic systems are likely to be universal parts of our heritage, potentially going wrong in ways that strike observers as deviant. However, the specific concept of belief that is tied to textbook accounts of delusion is very likely a function of particular patterns of cognitive development in modern societies, tied to culturally recent and local ways of legitimating belief.
Delusions are universal in the sense that they can occur anywhere but they are not universal in the sense of arising from a shared, evolved folk psychology. Psychiatry needs to widen its repertoire of concepts dealing with pathological epistemic states.

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WHAT IS A PLACEBO?

Session 4C

Congress section(s): B6

In "The Placebo Concept in Medicine and Psychiatry" Grünbaum offers a general account of the placebo effect as therapuetic improvement brought about by something other than the theory on which the therapy is based. I will review some criticisms of Grünbaum's account, which concentrate on its failure to consider expectation effects, and wonder how far the account can be adapted to take psychological effects into account.

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DEFAMILIARIZATION IN SCIENCE FICTION: NEW PERSPECTIVES ON SCIENTIFIC CONCEPTS

Session 11D

Congress section(s): B6, B7, C2, C7

The notion of defamiliarization, developed by Victor Shklovsky in his book "Theory of Prose" (1917), refers to a technique, used in literature, that has the effect of estranging things that are so familiar that we don't even notice them anymore. They become automatized, due to familiarity and saturated contact. For Shklovsky, however, literature and art in general are able to disturb our common world views. John Dewey, in Art as Experience (1934), doesn't mention the Russian author, but presents a similar standpoint. Dewey expounds the idea of "Aesthetic Experience", which appears to have many similarities to Shklovsky's approach, asserting that art awakens from the familiar and allows more meaningful and complete experiences.

This paper aims to analyze the use of scientific conceptions in science fiction, leading to a new way to look at them. This new glance modifies the trivial connections of current paradigms in science and also in everyday life. The shift to science fiction's context would contribute to their defamiliarization, giving way to new possibilities of understanding. According to the examined authors, defamiliarization and aesthetic experience are responsible for bringing to consciousness things that were automatized, putting a new light over them. That appears also to be the case in science fiction, in which the break of expectations may have consequences not only to the paradigms of the sciences, but to the reflection about the role of science in ordinary life. In many cases, scientific notions are already made unconsciously accepted in quotidian life, just like everyday assumptions. Besides, science fiction, exaggerating or pushing scientific theories as far as can be imagined, brings about important and profound considerations regarding philosophical questions as well.
H. G. Wells' works from around the turn of the 20th century seem to adequately illustrate this process. For instance, in H. G. Wells' novel The invisible man (1897), scientific objects such as matter and light, as well as the prevalent scientific rules they obey, are displaced to another context, breaking usual expectations about them. In fiction, it is possible for matter and light to behave in a different way as the established paradigm in physics at the end of the 19th century permitted. It is also interesting to notice that the book was published in a time of crisis in physics, and it seems that Wells absorbed ideas that would change the paradigm in a few years. To claim that science fiction influenced some scientists to initiate scientific revolutions is maybe too large a step to take in this paper. Nevertheless, it is possible to say that the process of defamiliarization in the reading of science fiction can lead to a new understanding of scientific concepts, inducing reflections that would not be made if the regular context and laws of science were maintained.

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ON EXPLANATION AND UNIFICATION

Session 19J

Congress section(s): B1

The notion of explanation is one of the most central notions in philosophy of science. The notion of explanation is closely connected to different other notions, like scientific theory and theory construction, interpretation of theories and observational-theoretical distinction, and scientific reasoning. The classical papers Hempel (1942) and Hempel and Oppenheim (1948) gave logically strict and philosophically interesting formulation of the notion (Hempel 1965). The model is known as the Covering Law Model which says that explanation is a logical argument in which, at least, one scientific law is used.
In science, there has been a tendency that new theories are more general than the older ones. A new theory unifies older theories. Analogous notion of unification in the theory of explanation is sometimes called reductive explanation in which the notions like superseding and screening off are of central importance; (a theory T2 supersedes a theory T1 in explaining g given e: $P(g/T2 \& e) > P(g/ T1 \& e) > P(g/e)$ and a theory T2 screens off a theory T1 from g given e: $P(g/T1 \& T2 \& e) = P(g/T2 \& e) \neq P(g/T1 \& e)$) (Niiniluoto 2016 and referents therein).
In the theory of explanation also a different notion of unification is used. For example, according to Kitcher (1989) unification is based on explanatory patterns (or explanatory stores). Explanatory patterns unifiy the underlying knowledge base (a set of accepted sentences). Kitcher says that explanation is a deductive argument. So, Kitcher's approach is nicely connected to Covering Law Model. However, the notion of explanatory pattern (or explanatory store) need to be further specified (Psillos 2002). To do the task we will analyse closely scientific reasoning or, more precisely, explanatory reasoning. In the task we will use the Interrogative Model of Inquiry developed by Hintikka. Interrogative Model allows us to explicate where the explanatory power of explanations comes (Halonen & Hintikka 2005).
The Interrogative Model explicates reasoning process as a knowledge construction process. And detailed analysis of the explanatory reasoning process shows detailed logical and conceptual information about explanatory process. This is connected to pragmatics of explanation; our analysis can be called logical pragmatics. Especially we get important information about the role of general laws in explanation. Hence, we get better understanding of Covering Law Model. General analysis of the explanatory reasoning processes which allows us to consider more closely Kitcher's notion of explanatory pattern and hence the notion of unification. Moreover, Interrogative Model throw some new light to the relationship between the two notions of unification referred above.

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PRACTICAL REALISM AND METAPHYSICS IN SCIENCE

Session 18G

Congress section(s): B4

In early XXI century the Estonian philosopher of science and chemistry Rein Vihalemm initiated a new account of science that he called practical realism. Vihalemm claimed having the influence of the realist or practice based approaches of Rom Harré, Joseph Rouse, Ilkka Niiniluoto and Sami Pihlström but most notably of the criticism of modern science of Nicholas Maxwell (Vihalemm 2011). Vihalemm's main idea is that science is, although based on theories, a practical activity that cannot be totally value free and independent of the cultural-historical context. In the course of practical research, we get access to an aspect of the world that becomes available to us through the prism of the theories we apply. This is a limited access, not the God's Eye view but according to Vihalemm it is the real world that offers some access to the researcher. Still, there may be something more that is necessary, in order to make a proper sense of science and its progress that we can hardly deny. According to Nicholas Maxwell, scientists consistently presume that the universe is comprehensible and prefer unified theories to disunified ones and simple theories to complicated ones, although the latter are often empirically more successful than the former ones. This means that science actually includes assumptions that cannot be empirically tested, i.e. they are metaphysical in the Popperian sense. The acknowledgement of metaphysical assumptions in science is an inherent part of a novel approach to science of Nicholas Maxwell that he calls aim-oriented empiricism (see for instance Maxwell 2017). Rein Vihalemm accepts Maxwell's critique of the prevalent common understanding of science that the latter calls standard empiricism although Maxwell's approach is not necessarily realist and does not emphasize the practical side of research. Vihalemm likes the normative side of Maxwell's account. The latter agrees that science cannot be and has not to be value free. Vihalemm seems to be positive concerning aim-oriented empiricism as well. However, he rejects the need for acknowledging the metaphysical assumptions in science. Just the opposite is true. Vihalemm's intention is that practical realism has to be free of metaphysics. However, this puts us face to face with the problem in what respect is practical realism actually realism and in what respect is it different from standard empiricism. The solution can be combining practical realism with the idea of adding the metaphysical assumptions to the approach. By this move, practical realism would obtain a necessary foundation that enables to understand why scientific research remains a systematic quest for truth and does not limit itself just to a special kind of practical activity. However, this way we would rather get aim-oriented empiricism than practical or any other kind of realism.

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EVIDENTIAL RELATIONS IN A TRADING ZONE

Session 6I

Congress section(s): B1

Computer Simulation has been treated as a type of thought experiment (Di Paolo et al. (2000), experiment (Humphreys 1990, 2004; Barberousse et al. 2009; Morrison 2009, 2015; Morgan 2003; Parker 2009), and argument (Bisbert 2012, Beisbert and Norton). Consequently, the epistemic status of CS is weighed according to this ontological classification. For instance, if CS is an argument, then the strength of that argument gives the measure of the epistemic weight of a CS. Such classifications presuppose a neat demarcation between theory and experiment in the practice. The consequences of this assumption are reflected in the treatment of evidential relations by at CS. Contrary to the neat ontological classifications of CS, Peter Galison (2011) has noted the untidy relations between theory and experiment in CS. He employed the idea of 'trading zones' to explain the epistemic activities with CSs. Following Galison, this paper attempts to do two things. First, it shows the epistemic consequences of the various ontological characterization of CS. For this purpose, I invoke the traditional distinctions among data, phenomena, and evidence. Employing this distinctions, I show that no ontology of CS alone can adequately account for employment of CS as evidentially relevant scientific practice. In the second part, I discuss Galisons' notion of trading zone and demonstrate the construction of evidence in a trading zone. This will eventually show that CS practice betrays philosopher's urge for neat categories like theory and experiment. It will also show the inadequacy of neat ontological categorization in explaining and epistemically vetting the various features of CS like epistemic opacity. I will discuss a CS, published by Agrawal and Babu (2018), about the movement of microorganism in a biomotility mixture as a test case.
The paper is structured as follows. In the first section, I review the various ontological characterization of CS and present the data, phenomena, and evidence distinction. The section two will present the case study and identify the data, phenomena, and evidence distinction in it. Third section will show the limitation of the ontological characterization by applying them to the present test-case and the evidential relation drawn in the case study. Section four will discuss the idea of trading zone and explain how evidential relations in CSs can be characterized employing this concept. Fifth session will discuss the epistemic strategies within a 'trading zone' by referring to the case study. This will be further used to demonstrate the inadequacies of the neat ontological characterization of CS to account for the epistemic practice. I will then conclude the paper by noting some possible objections as well as the limitations of the approach developed in the paper.
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A FORMALIZATION OF LOGIC AND PROOFS IN EUCLID'S GEOMETRY

Session 29C

Congress section(s): C1

According to Paul Bernays and other scholars (e.g. Ian Muller), Euclid's geometry is a theory of constructions, in the sense that the geometrical figures are considered to be constructed entities. Euclid's geometry thus opposes to contemporary axiomatic theories (e.g. Hilbert's geometry) which proceed from a system of objects fixed from the outset, and which just describe the relationships holding between these objects.
The aim of this talk is to provide a formal and logical analysis of Euclid's constructive practice as it emerges from the Book I of the Elements. In the first part of the talk we argue that this practice cannot be captured by the standard methods of intuitionistic logic, like the witness extraction for existential formulas, since in Euclid's Elements there is nothing like a fixed domain of quantification. On the contrary, it is the constructive activity itself that allows one to generate, step by step, the domain of the theory.
In order to give a formal and precise analysis of this point, in the second part of the talk we study the proof methods used in Euclid's Elements. We propose a reconstruction of these methods, according to which postulates correspond to production rules (acting on terms and) allowing one to introduce new objects starting from previously given ones. This is done by means of primitive functions corresponding to the actions of fixing a point, drawing a straight line, and drawing a circle, respectively. We argue that a combination of these production rules corresponds to a proof allowing one to solve a problem, i.e. to show that a certain construction is admissible from other primitive ones. The constructed objects are considered to be representable by diagrams, which in turn can be used to extract some information concerning the constructions (e.g. diagrams give evidence for the incidence between the two circles used to prove proposition I.1). Moreover, in order to demonstrate that the constructed objects possess certain specific properties, a method for keeping track of the relationships between the entities used during the construction process is proposed. This method consists in labelling proofs by means of relational atoms and by combinations of these relational atoms.
The language that we use for our formalization of Euclid's constructive practice is kept as minimal as possible. This marks some crucial differences with other already existent formal reconstructions of Euclid's geometry (e.g. the one by Michael Beeson or the one by Jeremy Avigad, Edward Dean and John Mumma). On the one hand, we claim that no identity relation is needed for points, lines, and circles. Identity is used instead only for abstract objects, like angles. On the other hand, we claim that no negation (as a propositional) operator is needed when reasoning on the properties of the constructed objects. The use of dual (incompatible) predicates is indeed already sufficient (e.g. "being strictly smaller in length" and "having the same length"). The logic of the Elements is thus taken to be weaker than usual standard logics, like intuitionistic or classical logic.

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FINDING A WAY BACK: PHILOSOPHY OF DATA SCIENCE ON ITS PRACTICE

Session 26A

Congress section(s): C6

Because of the bewildering proliferation of data science algorithms, it is difficult to assess the potential of individual techniques, beyond their obvious ability to solve the problems that have been tested on them, or to evaluate their relevance for specific datasets.
In response to these difficulties, an effective philosophy of data science should be able not only to describe and synthesize the methodological outline of this field, but also to project back on the practice of data science a discerning frame that can guide, as well as be guided by, the development of algorithmic methods. In this talk we attempt some first steps in this latter direction.
In particular, we will explore the appropriateness of data science methods for large classes of phenomena described by processes mirroring those found in developmental biology.
Our analysis will rely on our previous work [1,2,3] on the motifs of mathematization in data science: the principle of forcing, that emphasizes how large data sets allow mathematical structures to be used in solving problems, irrespective of any heuristic motivation for their usefulness; and Brandt's principle [3], that synthesizes the way forcing local optimization methods can be used in general to build effective data-driven algorithms.
We will then show how this methodological frame can provide useful broad indications on key questions of stability and accuracy for two of the most successful methods in data science, deep learning and boosting.

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LOGIC OF SCALES

Session 27L

Congress section(s): A2

We mean by "scale" a set of predicates so that:
1) All predicates of a same scale are contrary each to another.
2) If g does not belong to the scale S, then there is an f in S so that f and g are compatible.
3) The predicates of a scale, S, are complementary, namely, any would be an object, a, there is an element of S, for instance f, so that it takes place fa.
The scales, as systems of predicates, exists. For any predicate there is a negative predicate, namely, to the predicate f corresponds the predicate non-f, or f*. The set {f, f*} is a scale since its elements are contrary and complementary predicates, hence, the conditions 1-3 are fulfilled.
Let F = {f1, f2, ..., fn} be a set of contrary predicates. In this case, the set S ={f1, f2, ..., fn, O}, where O = (f1 v f2 v ... v fn)*, is a scale. Indeed, it is easy to prove that O is contrary relatively to any fi predicate. Moreover, if g is contrary to every fi

predicates, then g is compatible to O. On the other side, the elements of S are complementary. Any object satisfies a predicate from F or not. In this last case, it will satisfy the predicate O, therefore any object satisfies a predicate from S.
We call the predicate O the origin of the scale S and the predicate O*, namely, the predicate F = f1 v f2 v ... v fn, is the genus predicate of the scale S. We can easily notice that the set which includes only the predicates O and F is a scale. In this way, every scale can be represented using its origin and its genus. The genus and the origin of a scale are contradictory, namely, F = O*.
If {F, O} is a scale, and F = (f1 v f2 v ... v fn), then, for every predicate fi it takes place (x)(fx - Fx). Therefore, there must be a set of operators, xi, so that fi = xiF. We will call these operators numbers. It follows that a predicate can be analyzed using a genus and a number.
An object satisfies to a given moment a single predicate from a scale. If t1 and t2 are two different moments and f1 and f2 are predicates of the same scale, S, then it takes place: E = (f1, t1)a & (f2, t2)a.
We will introduce the following convention: E =not (f1, f2)(t1, t2)a. The ordered pair (f1, f2) represent a change relatively to the scale S. We have got the result that the elements of the Cartesian square S x S = S2 are all possible changes definable upon the elements of the scale S. On the other hand, the function h: S - S, which assigns to any element of S an element and only one from S, is a transformation inside of the scale S. We may now introduce new kinds of propositions, like the propositions of change and the propositions of transformation having a specific logic.

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VALUES IN SCIENCE AND VALUE CONFLICTS

Session 4G

Congress section(s): B5

In my presentation I will bring into focus a problem of recognizing values influencing science and propose that in the instances of value conflicts we are more acutely aware of the values influencing us. I will also present a possible guideline for value conflicts by hierarchy of values. Although, it is generally agreed that values have a role to play in science, the exact nature of this role or which values should be allowed in science is debatable. The value-free science ideal proposes that only epistemic values should be allowed to influence the core epistemic practices of science. Heather Douglas (2000) has made a case for the necessity of non-epistemic values in science. Douglas´ proposal stems from the argument from inductive risk. Inductive risk is the chance that a scientist might be mistaken in accepting or rejecting a hypothesis (Douglas, 2000). Douglas (2000) proposes that for assessing the possible harmful consequences of such a mistake non-epistemic values are necessary. Although Douglas (2009) distinguishes proper roles for the values in science, how to recognize these values remains unclear. She also seems to hold all values in equal standing, which presents a problem for resolving value conflicts. Others like Daniel Steel and Kyle Powys Whyte (2012), Ingo Brigandt (2015), Immaculada de Melo-Martin and Kristen Intemann (2016) have enhanced Douglas´ position. Steel and Whyte (2012), proposed a general values-in-science standard. It states, that non-epistemic values should not conflict with epistemic values in the design, interpretation, or dissemination of scientific research unless epistemic values fail to indicate a better option. Brigandt (2015) suggested that, besides epistemic considerations, social and environmental values may determine a scientific theory´s conditions of adequacy for an unbiased and complete theory. Melo-Martin and Intemann (2016) maintained that social, ethical, and political value judgements are legitimate in scientific decisions insofar as they promote democratically endorsed epistemological and social aims of the research. Nonetheless, the question of recognizing values seems to remain unanswered. The premise of my presentation is that values influencing science come more vividly forth in the context of different value conflicts. Therefore, it is important to analyze value conflicts in science. Related problem is resolving value conflicts in the context of equal values. As a solution, I propose a flexible hierarchy of values to help us resolve value conflicts in practical situations.
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A SIMPLE PROOF OF BARR'S THEOREM FOR INFINITARY GEOMETRIC LOGIC

Session 24C

Congress section(s): A1

Geometric logic has gained considerable interest in recent years: con- tributions and applications areas include structural proof theory, category theory, constructive mathematics, modal and non-classical logics, automated deduction.
Geometric logic is readily defined by stating the structure of its axioms. A coherent implication (also known in the literature as a “geometric axiom”, a “geometric sentence”, a “coherent axiom”, a “basic geometric sequent”, or a “coherent formula”) is a first-order sentence that is the universal clo- sure of an implication of formulas built up from atoms using conjunction, disjunction and existential quantification. The proper geometric theories are expressed in the language of infinitary logic and are defined in the same way as coherent theories except for allowing infinitary disjunctions in the antecedent and consequent.
Gentzen's systems of deduction, sequent calculus and natural deduction, have been considered an answer to Hilbert's 24th problem in providing the basis for a general theory of proof methods in mathematics that overcomes the limitations of axiomatic systems. They provide a transparent analysis of the structure of proofs that works to perfection for pure logic. When such systems of deduction are augmented with axioms for mathematical theories, much of the strong properties are lost. However, these properties can be regained through a transformation of axioms into rules of inference of a suitable form. Coherent theories are very well placed into this program, in fact, they can be translated as inference rules in a natural fashion: In the context of a sequent calculus such as G3c [4, 8], special coherent implications as axioms can be converted directly [2] to inference rules without affecting the admissibility of the structural rules; This is essential in the quest of applying the methods of structural proof theory to geometric logic.
Coherent implications I form sequents that give a Glivenko class [5, 3]. In this case, the result [2], known as the first-order Barr's Theorem (the general form of Barr's theorem [1, 9, 6] is higher-order and includes the axiom of choice) states that if each $I_i : 0 \leq i \leq n$ is a coherent implication and the sequent $I_1, \ldots, I_n \Rightarrow I_0$ is classically provable then it is intuition- istically provable. By these results, the proof-theoretic study of coherent gives a general twist to the problem of extracting the constructive content of mathematical proofs.
In this talk, proof analysis is extended to all such theories by augmenting an infinitary classical sequent calculus with a rule scheme for infinitary geometric implications. The calculus is designed in such a way as to have all the rules invertible and all the structural rules admissible.
An intuitionistic infinitary multisuccedent sequent calculus is also intro- duced and it is shown to enjoy the same structural properties as the classical calculus. Finally, it is shown that by bringing the classical and intuitionistic calculi close together, the infinitary Barr theorem becomes an immediate result.
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DSTIT MODALITIES THROUGH LABELLED SEQUENT CALCULUS

Session 27L

Congress section(s): A2

Dstit (deliberately seeing to it that) is an agentive modality usually semantically defined on a tree-like structure of successive moments. Any maximal sequence of moments forms a history, with individual moments parts of different histories, but all histories sharing some moment. The tree has forward branching time (BT), corresponding to indeterminacy of the future, but no backward branching, corresponding to uniqueness of the past, and is enriched by agent choice (AC). Choice is a function mapping an agent/moment pair to a partition of all histories passing through that moment (since an agent's choice determines what history comes about only to an extent).
In such (BT+AC) frames, formulas are evaluated at moments in histories. Specifically, an agent a deliberately seeing to it that A holds at the moment m of a history h holds iff (i) A holds in all histories choice-equivalent to h for the agent a, but (ii) doesn't hold in at least one history that the moment m is a part of. In simple terms, the agent sees to it that A if their choice brings about those histories where A holds, but nonetheless it could have been otherwise (i.e. an agent can't bring about something that would have happened anyway).
Stit modalities, including Dstit, received an extensive axiomatic treatment in [1], and the proof-theoretic approaches to it so far have been done via labelled tableaux [5, 6].
In contrast, in this paper we investigate Dstit modality by means of a sequent calculus. Following [2, 3], we employ the axioms-as-rules approach, and develop a G3-style labelled sequent calculus. This is shown to possess all the desired structural properties of a good proof system, including being contraction- and cut-free.
Moreover, we demonstrate multiple applications of the system. We prove the impossibility of delegation of tasks among independent agents, the interdefinability of Dstit with an agent-relative modality, cstit, and an agent-independent modality, settled true, as well as the treatment of refraining from [1] and [4]. Finally, we demonstrate the metatheoretical properties of our system, namely soundness, completeness and decidability via a bounded proof search.
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DEDUCTIVE SAVAGES: THE OXFORD NOETICS ON LOGIC AND SCIENTIFIC METHOD

Session 18L

Congress section(s): A4, B6

In 1832, William Whewell reminded his friend, the political economist Richard Jones, that "if any truth is to be got at, pluck it when it has grown ripe, and do not, like the deductive savages, cut down the tree to get at it." [1] He preferred a cautious ascent from particular facts to general propositions over the reckless anticipations of people like the Oxford Noetics. To Whewell, this was more than an epistemic preference; it was a moral one. [2] Progress in all fields should be slow and sure or it could potentially lead down atheistic, materialistic, even revolutionary paths.
A few major figures comprised the Noetics, including Edward Copleston, Nassau Senior, and Richard Whately. Despite Whewell's conflation of their methods with the more radical political economists like David Ricardo, Jeremy Bentham, and James Mill, the foundation of their own programme was to champion Anglican theology on the grounds of its rationality. As part of this programme, Copleston engaged with Scottish scholars who considered the Oxford curriculum backward; Senior accepted a position as the first Drummond Professor of Political Economy; and Whately published his inordinately popular Elements of Logic (1826) and accepted a position as the second Drummond Professor. Most significantly for my paper, they revitalized syllogistic logic and divorced political economy from its "subversive" connotations. [3]
The Noetics were influential in a number of aspects of Victorian logic, philosophy, theology, and science. Yet their programme has gone underappreciated. First of all, Whewell's depiction of them as "deductivists" is not exactly right. Christophe Depoórtere has already shown this in the case of Nassau Senior in the context of political economy, but I intend to do the same for the general programme. Second, their revitalization of syllogistic logic has been misinterpreted as a revitalization of scholastic logic despite their harsh criticisms of the scholastics. [4] Still other aspects of the their programme have been left virtually unexplored, like Whately's notion of induction, or the relationship between logical and physical discovery.
In this paper, I will provide a more sympathetic account of the Noetic movement in the context of its positions on logic and scientific method. I will focus mostly on Whately, though Copleston and Senior will have their important parts to play. Instead of interpreting their scientific method as deductivist, I will show that they did not believe there was a single method suitable for all sciences; rather, they believed that deduction and induction (in Whewell's sense) played variable roles in each according to their nature.
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TRANSCENDENTAL IN PHYSICAL THEORY

Session 3H

Congress section(s): B4

The structure of any physical theory includes representations about metaphysical essences. Examples of such existence are the absolute space and the absolute time in Newton's mechanics, the perpetual mobile in thermodynamics, absolute standards in theories of calibration fields of Hermann Weyl, the velocity of light in the special theory of relativity by A. Einstein, etc. These representations cannot be interpreted as real physical objects. At the same time they are necessary for interpretation of real physical phenomena.
In my report I analyze details of Hermann Weyl's theory of gauge fields which is used wildly in modern theories of elementary particles. The idea of gauge changes allows to give geometrical description of physical forces. The concept of gauge invariant entered by Hermann Weyl assumes existence in the gauge space the ideal standards of measurement. Weyl's ideal standards are absurd things. They cannot be defined as real physical objects. At the same time without the assumption of ontological status of a set of ideal standards identical each other it is impossible to speak about existence of gauge transformation of real physical quantities. Ideal standards give physical sense to gauge transformations and allow to prove gauge invariance of physical laws. Thus, we can speak about ideal standards only as super-physical (or metaphysical) essences.
I suggest to interpret these metaphysical essence as transcendental. I. Kant gives dual treatment of transcendental. According to Kant transcendental there is both a deep force into a subject, hidden for him, and a certain unconditional existence out of a subject, defining its individual conscious acts. Research of the nature of these transcendental existences which cannot be defined neither as something subjective nor as something objective is very perspective.
In my concept transcendental existence is a product of invention of symbolical object which corresponds logically "empty" concept, and it is characterized by sign and meaning coincidence. Such transcendental existences are an important metaphysical component of any fundamental physical theory.

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A NON-TRIVIAL EXTENSION FOR MS

Session 27K

Congress section(s): A1, A3

The method of possible-translation semantics was introduced by Walter Carnielli in [3]. The core idea of this method is to purport a logic with a multivalued semantics. It was applied to a variant of Cn calculi by the same author in [2] and later on to Cn itself by Jõao Marcos in [5].
One consequence of the application of this method is the recovery of truth-functionality for logics whose matrices are non-finitely determinate. This is particular important for the calculus C1. This property has been useful for the development of a tableaux system for C1 that we have called Ms
Our aim in this talk is to explore a possible non-trivial extension of our system for the hierarchy of calculi Cn. We will compare this system with those offered in [1], [4] and [5]. The comparision aims at showing the adventages of this system for considering it a viable alternative
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SCHRÖDINGER'S "WHAT IS LIFE?" 75 YEARS ON

Session 13H

Congress section(s): B2

2019 marks 75 years since Erwin Schrödinger, one of the most celebrated physicists of the twentieth century, turned his attention to biology and published a little book titled "What Is Life?". Much has been written on the book's instrumental role in marshalling an entire generation of physicists as well as biologists to enter the new field that came to be known as "molecular biology". Indeed, many founding figures of molecular biology have acknowledged their debt to it. Scientifically, the importance of "What Is Life?" is generally taken to lie in having introduced the idea that the hereditary material (at the time it hadn't yet been conclusively identified as DNA) contains a ‚code-script' that specifies the information necessary for the developmental construction of an organism. Although Schrödinger ascribed too much agency to this code-script, as he assumed that it directly determines the organism's phenotype, his insight that the genetic material contains a code that specifies the primary structure of the molecules responsible for most cellular functions has proven to be essentially correct. Similarly, Schrodinger's famous account of how organisms conform to the second law of thermodynamics, by feeding on "negative entropy" at the expense of increasing the entropy of their surroundings, is also quite correct (even if this idea was already well-known at the time). Consequently, most retrospective evaluations of "What Is Life?" (including the ones which have just appeared to commemorate its 75th anniversary) converge in praising the book for having exerted a highly positive influence on the development of molecular biology. In this paper I challenge this widely accepted interpretation by carefully dissecting the argument that Schrödinger sets out in "What Is Life?", which concerns the nature of biological order. Schrödinger clearly demarcates the kind of order found in the physical world, which is based on the statistical averaging of vast numbers of stochastically-acting molecules that collectively display regular, law-like patterns of behaviour, from the kind of order found in the living world, which has its basis in the chemical structure of a single molecule, the self-replicating chromosome, which he conceived as a solid-state ‚aperiodic crystal' in order to account for its remarkable stability in the face of stochastic perturbations. Schrödinger referred to the former, physical kind of order as "order-from-disorder" and the latter, biological kind of order as "order-from-order". As I will argue, this demarcation proved disastrous for molecular biology, for it granted molecular biologists the licence for over half a century to legitimately disregard the impact of stochasticity at the molecular scale (despite being inevitable from a physical point of view), encouraging them instead to develop a highly idealized, deterministic view of the molecular mechanisms underlying the cell, which are still today often misleadingly characterized as fixed, solid-state ‚circuits'. It has taken molecular biologists a disturbingly long time to "unlearn" Schrödinger's lessons regarding biological order and to start taking seriously the role of self-organization and stochasticity (or "noise"), and this, I claim, should be considered the real scientific legacy of "What Is Life?" 75 years on.

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APPROACHING PROBABILISTIC LAWS

Session 5D

Congress section(s): B2

In the general problem of verisimilitude, we try to define the distance of a statement from a target, which is an informative truth about some domain of investigation. For example, the target can be a state description, a structure description, or a constituent of a first-order language. In the problem of legisimilitude, the target is a deterministic or universal law, which can be expressed by a nomic constituent involving the operators of physical necessity and possibility. The special case of legisimilitude, where the target is a probabilistic law, has been discussed by Roger Rosenkrantz (Synthese, 1980) and Ilkka Niiniluoto (Truthlikeness, 1987, Ch. 11.5). The basic proposal is to measure the distance between two probabilistic laws by the Kullback-Leibler notion of divergence, which is a semimetric on the space of probability measures. This idea can be applied to probabilistic laws of coexistence and laws of succession, and the examples may involve discrete or continuous state spaces. These earlier studies should be elaborated in three directions. First, other measures of divergence could be considered. Secondly, if deterministic laws are limiting cases of probabilistic laws, then the legisimilitude of the latter should be reducible to that of the former. Thirdly, a solution should be sought for the epistemic problem of estimating degrees of probabilistic legisimilitude on the basis of empirical evidence.

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THE REHABILITATION OF KARL POPPER'S VIEWS OF EVOLUTIONARY BIOLOGY AND THE AGENCY OF ORGANISMS

Session 12C

Congress section(s): B6

In 1986 Karl Popper gave the Medawar Lecture at The Royal Society in London. He deeply shocked his audience, and subsequently entered into prolonged correspondence with Max Perutz over the question whether biology could be reduced to chemistry. Perutz was insistent that it could be, Popper was equally insistent that it could not be. The lecture was never published by The Royal Society but it has now been made public with the publication of Hans-Joachim Niemann's (2014) book. Popper contrasted what he called "passive Darwinism" (essentially the neo-Darwinist Modern Synthesis) with "active Darwinism" (based on the active agency of organisms). This was a classic clash between reductionist views of biology that exclude teleology and intentionality and those that see these features of the behaviour of organisms as central in what Patrick Bateson (2017) calls "the adaptability driver". In the process of investigating how organisms can harness stochasticity in generating functional responses to environmental challenges we developed a theory of choice that reconciles the unpredictability of a free choice with its subsequent rational explanation (Noble and D Noble 2018). Popper could not have known the full extent of the way in which organisms harness stochasticity nor how deeply this affects the theory of evolution (Noble & D. Noble, 2017), but in almost all other respects he arrived at essentially the same conclusions. Our paper will call for the rehabilitation of Popper's view of biology. Neo-Darwinists see genetic stochasticity as just the source of variation. We see it as the clay from which the active behaviour of organisms develops and therefore influences the direction of evolution.

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PREDICTION MARKETS AND EXTRAPOLATION

Session 18F

Congress section(s): B1, B3, C7

The extrapolator's circle states, roughly speaking, that in order to extrapolate a model or causal relation it is necessary to know that it applies in the relevant new domain, but that in order to establish the latter it is necessary in turn to examine that new domain – thus negating the main benefit of extrapolation, which is precisely that we can avoid having to examine the new domain (Steel 2008). The extrapolator's circle is an important problem. How might it be overcome? We argue for a previously unappreciated solution, namely the use of prediction markets (when they are available, which is currently mainly in social science cases).
Prediction markets are markets for placing bets on future or otherwise unknown events. The price signals arising in such markets, if interpreted as probability assignments, constitute implicit predictions. Prediction markets are attractive because they have a track record of predictive success (e.g. Ahlstrom-Vij 2016). They work well when there are at least some informed traders on the market – indeed, going by the current empirical literature, this seems close to a sufficient condition for them to predict successfully. If so, then the only thing you need to know as a market maker applying a prediction market to some new domain is that, somewhere in the pool of traders you attract, there will be some who are informed. Crucially, what you don't need to know is any particular theory about the new domain. Of course, individual traders on the market might

make any number of theoretical assumptions, and (lucky guesses aside) those assumptions will usefully inform the market's output only in so far as they lead to good predictions. But the market maker need presuppose almost no theory whatsoever. In effect, prediction markets are mechanisms that extrapolate easily because they require unusually minimal assumptions. In particular, they require only that there exist some informed traders, plus that there is sufficient market liquidity, available data, legal infrastructure, and so forth.

The above only concerns prediction of actual events. But extrapolation also often concerns conditional predictions, e.g. about the result of possible or counterfactual interventions. Hitherto, there has been no evidence that guidance about such conditional predictions can be given by prediction markets because by definition in conditional cases no actual event ever occurs that settles market participants' bets, at least not in the timeframe of interest. But recent experimental research now suggests that so-called self-resolving prediction markets, i.e. markets for non-actual events, may operate just as reliably as markets for actual events. We report on that research here. If it holds up, it will show that prediction markets can achieve all of the goals of extrapolation, namely prediction of both actual and non-actual events alike.

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PROBLEMATIC INTERDISCIPLINARITY OF THE COGNITIVE SCIENCE

Session 8F

Congress section(s): C5

In spite of the implicit agreement that cognitive science is by default interdisciplinary research project (as introductory chapters in any textbooks or companions on Cognitive Science attest), some authors submit that this interdisciplinarity [ITD] is not obvious at all, and we should consider whether there is any (Cohen-Cole, 2007; Graff, 2015). They argue, mainly based on historical data, that cognitive science tends to more and more divergence between disciplines, which are becoming more and more autonomous and self-sufficient (Graff, 2015). Is CS really turning away from its interdisciplinary beginnings or maybe it has never been fully interdisciplinary?

Starting from the distinction between object-, problem-, method- and theory-oriented ITD (Schmidt, 2011), I will focus on the tension between object- and problem-oriented ITD in Cognitive Science. More specifically, I will argue that: (1) interdisciplinary research is mostly problem-driven – there are INT-forcing problems, essential for lively interdisciplinary research; (2) although the object-oriented INT in Cognitive Science seems noncontroversial, most grand problems of Cognitive Science (the nature of mind or cognition) are not a INT-forcing problems, but there is a lot of smaller interdisciplinary problem forced by the issues related by integration between particular disciplines of Cognitive Science (e.g.: stabilization of theoretical constructs between psychology and neuroscience (Sullivan, 2017)). Therefore, following the classical work of Campbell (Campbell, 2014) and contemporary rich research on interdisciplinarity (e.g.: Grüne-Yanoff, 2016; Huutoniemi, Klein, Bruun, & Hukkinen, 2010; Klein, 2010; Koskinen & Mäki, 2016; MacLeod, 2016), I will argue that cognitive science strives to fish-scale model of plurality of interdisciplines, which is in accordance with contemporary pluralistic stance in Cognitive Science (Dale, 2008; Dale, Dietrich, & Chemero, 2009).

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VARIETIES OF PERMISSION FOR COMPLEX ACTIONS

Session 11B

Congress section(s): A2

The main problem in deontic logic based on propositional dynamic logic is how to define the normative status of complex actions based on the normative status of atomic actions, transitions and states. There are two main approaches to this problem in the literature: the first defines the normative status of an action in terms of the normative status of the possible outcome states of the action (Broersen, 2004; Meyer, 1988), while the second defines the normative status of an action in terms of the normative status of the transitions occurring in the possible executions of the action (van der Meyden, 1996). In this work, I focus on interpretations of permission concepts. In particular, I address what I take to be two shortcomings in the two main approaches to permission in dynamic logic.

First, when assessing an agent's behavior from a normative viewpoint, one must often take into account both the results brought about by the agent, and the means by which those results were brought about. Consequently, when deciding whether a complex action is to be permitted or not, one must, in many cases, take into account both the normative status of the possible outcome states of the action, and the normative status of the atomic actions that occur in the complex action: choosing one of the two is not enough.

Second, most existing accounts, with the exception of the work of Kulicki and Trypuz (2015), consider the permissibility of actions only relative to their complete executions, i.e. the possible executions where each step in the complex action is carried out. However, in the presence of non-determinism it may happen that some initial part of a complex sequential action leads to a state where the remaining part of the action cannot be executed. This possibility can lead to counterintuitive consequences when one considers strong forms of permission in combination with non-deterministic choice. Such cases show that also partial executions of complex actions are important from a normative viewpoint.

Taking both permitted states and permitted atomic actions as primitive allows for a wide variety of permission concepts for complex actions to be defined. Moreover, the distinction between complete and partial executions of complex actions offers further options for defining permission concepts. Based on these points, I define a variety of permission concepts and investigate their formal properties.

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CREDAL ACCURACY IN AN INDETERMINISTIC UNIVERSE

Session 6D

Congress section(s): B2

The truthlikeness program was originally focused on the explication of the notion of propositional accuracy. Its aim was to characterize an ordering of propositions with respect to closeness to a target proposition—the truth of some matter. The comparison class of propositions is determined by some question under investigation; each proposition in the comparison class is an answer to the question—either partial or complete—and is assumed to be true or false; candidates for the target are the complete answers. An ordering would yield a plausible ranking of the cognitive value of credal states. The simplest kind of credal state—belief in, or acceptance of a proposition—can be represented by the object of that belief. The cognitive value of accepting a proposition would be greater the closer that proposition is to the target. The epistemic utility program is after an ordering of probabilistic credal states with respect to a target state. In an earlier article I showed that these two programs, at least as currently formulated, make incompatible demands. A core desideratum in the truthlikeness program is a principle called proximity, while a core desideratum in the epistemic utility program is propriety. Proximity and propriety cannot be combined. For the most part the targets in the epistemic utility program are taken to be opinionated states, corresponding to complete propositional answers. But the truth that is the target of an inquiry may itself be probabilistic—for example, it may consist of objective propensities. Richard Pettigrew, placing constraints on Bregman divergences, has zeroed in on the Euclidean measure of distance between probability distributions as the solution in the general case. That measure yields the standard Brier measure in the deterministic or opinionated case. This measure, along with a host of others, ignore the metric structure of the logical space, and this is what lies at the root of the violation of proximity. In this paper I identify what I argue is the most promising measure of distance in the general probabilistic case—the Wasserstein, or earth-moving, measure—which in turn yields both the correct notion of credal accuracy in the opinionated case, and propositional accuracy, as special cases.

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HOW SHOULD WE MAKE INTELLIGIBLE THE COEXISTENCE OF THE DIFFERENT LOGICS? AN ATTEMPT BASED ON A MODAL SEMANTIC POINT OF VIEW

Session 14L

Congress section(s): C1

Recently, logicians and computer scientists increasingly tend to treat those different logics such as the classical, the intermediate, the intuitionistic and the still weaker logics (notably the linear logics) as the equally justifiable, legitimate objects of the study. It is too obvious that those research trends are to be welcomed in view of the amount of the theoretically fruitful results they bring in. We should admit, however, that we are still quite in the dark about how to make philosophically intelligible and justify the coexistence of those different logics, in particular, of the two representative logics, i.e. the classical logic (henceforth CL) and the intuitionistic logic (henceforth IL).
With good reasons, logicians and computer scientists usually prefer to avoid being involved in philosophical debates and are prone to take pragmatic attitudes to the problem. What about philosophers, then? They seem to be rather bigoted. At the one extreme, the ordinary analytic philosophers vehemently stick to CL (and the modal extensions thereof) and refuse to take the IL and the still weaker logics into serious philosophical consideration. At the other extreme, those few radical philosophers such as Michael Dummett baldly claim that CL should be abandoned on account of the unintelligibility of its fundamental semantic principle, i.e. the principle of bivalence, and that instead IL should be adopted as the uniquely justifiable genuine logic.
On one hand, I agree with Dummett that IL has at least one prominent virtue that CL definitely lacks: the constructive character of its inference principles, which, one might say, makes it theoretically more transparent and philosophically more coherent than CL, whose characteristic inference principles, i.e. the classical reductio, makes it irremediably non-constructive. On the other hand, however, it is too evident that CL plays a pivotal role in the development of the ordinary classical mathematics, in particular of its theoretical foundations, i.e. the set theory, and that those theoretical contributions of the classical logic should be, rather than just being neglected or denounced to be unintelligible, squarely made sense of and illuminatingly accounted for.
I propose to start by setting up a certain common "platform" on which to locate the different logics and to determine their respective properties and mutual relationships precisely. It is well-known that the translation introduced by Gödel in his [G1933] (henceforth G-translation) from the language of IL to that of the logic S4, a modal extension of CL, is sound and faithful: An IL formula φ is provable in IL if and only if its G-translation is provable in S4. Roughly speaking, one may understand the situation thus: IL has (something very akin to) its isomorphic image in (some sublanguage of) S4. And in this sense S4 is called "the modal companion" of IL. G-translation also yields a modal companion for each of the super-intuitionistic logics (i.e. the stronger logics than IL). For example, it assigns CL with the modal logic S5 as its modal companion. Moreover, various weaker logics can be assigned with their respective modal companion by the translation (or its slight variant). Thus, for the moment we can conclude that we have a "platform" (in the above sense of the word) for the different logics just in the world of the (classical) modal logics.
Note that this is not the end of the matter but the beginning. Why can the modal languages play such a prominent role? Undoubtedly the key to the answer lies in the notion of the modality (in particular that of necessity), but the mere notion of necessity is rather void of content and seem to provide hardly any sufficient explanation. At this point, the Kripke semantics (the relational structural semantics) of the modal languages is helpful in that it accounts for the modal notions by the non-modal relational terms. But then it becomes crucial how to conceive of the two key notions of the semantics: the notion of the possible state and that of the accessibility relation. I am going to propose a new account of these notions that is based on a proof theoretical viewpoint. .

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SOME PHILOSOPHICAL REMARKS ON THE CONCEPT OF STRUCTURE. CASE OF LADYMAN'S AND HELLER'S VIEW

Session 19H

Congress section(s): B4

In the perspective of historical development of science, especially physics, on the one hand can be noticed the essential role of the concept of structure (Landry 2011). On the other hand, however, it has never been proved that no theory never correctly described the nature of entities postulated by it or that such entities were never crucial for new predictions (Alai 2017). In todays' dispute about structural understanding of theories some philosophers (particular focus will be on philosophical ideas proposed by James Ladyman and Michał Heller) pointed out metaphysical and mathematical consequences of ongoing discussions (Ladyman 2002b; 2008). Viewing objects in the structural/relational way in ontology of physics and mathematics rather than in a classic one, shows that the concept of structure today remains crucial for philosophy of physics and either for the philosophy of science (Heller 2006; 2016a). In this context however arises the question about the type of dependence of structuralism in philosophy of physics not only on the structuralism in philosophy of mathematics, but on some metaphysical assumptions regarding the concept of structure. Therefore the main question to be clarified is why science and the contemporary philosophy of science need the philosophy of structure and what could be the correlation between metaphysical and epistemological engagement of so far proposed concept (Ladyman 2002a; 2007). According to Ladyman and Heller it is the mathematical structure of scientific theories that has the crucial role (e.g. the case of quantum mechanics and its different formulations) and, according to the latter, probably the category theory formalism

offers new perspectives and at the same time widens the problematic of structures and their relationships (Heller 2014; 2015). Subsequently, one can ask whether different formulations of scientific theories are representations of the same ideal mathematical structure or rather of physical structure of the world. In the first case, raises up the question if this ideal mathematical structure assumes implicitly some Platonic vision of mathematical objects or rather reveals some epistemic strategy of our cognition. In the second case, indeed contemporary philosophy of science is replete with debate which do not merely concern the characteristics of a well-defined scientific theory, but also what scientific theories can say about the world, i.e. do entities postulated by them really exist or are they just useful fictions. Therefore it can be asked if the structural realism grant the laws of nature an ontic status of some real elements of entitative structure of the world. Or maybe the concept of structure remains as the heuristic instrument, because the retention of mathematical formalism is merely a comfortable and cost-saving pragmatic tool used among the community of researchers. As emphasised by Heller, seems that the problem of rationality/intelligibility of the world (in its epistemic and ontic aspect) is connected to the structuralist view (Heller 2006; 2016b). Probably among some philosophers such a great commitment to the structuralist view reveals some hidden metaphysical presuppositions about the world and the science.

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STIT HEURISTICS AND THE CONSTRUCTION OF JUSTIFICATION STIT LOGIC

Session 12B

Congress section(s): A2

From its early days, stit logic was built around a set of heuristic principles that were typically phrased as recommendations to formalize certain ideas in a certain fashion. We have in mind the set of 6 stit theses advanced in [1, Ch. 1]. These theses mainly sought to guide the formalization of agentive sentences. However, it is often the case that one is interested in extending stit logic with new notions which are not necessarily confined to agentive phenomena; even in such cases one has to place the new notions in some relation to the existing stit conceptual machinery which often involves non-trivial formalization decisions which are completely outside the scope of the Belnapian stit theses.
The other issue is that the preferred stit operator of [1] is achievement stit, whereas in the more recent literature the focus is on different variants of either Chellas stit or deliberative stit operator.

In our talk we try to close these two gaps, by (1) reformulating some of the Belnapian theses for Chellas/deliberative stit operator, (2) developing heuristics for representing non-agentive sentences in stit logic, and (3) compensating for the absence of achievement stit operator by introducing the so-called “fulfillment perspective” on modalities in stit logic.
In doing so, we introduce a new set of heuristics, which, we argue, is still in harmony with the philosophy expressed in [1]. We then apply the new heuristic principles to analyze the ideas behind the family of justification stit logics recently introduced in [2] and [3].

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TOWARDS A BRIDGE OVER TWO APPROACHES IN CONNEXIVE LOGIC

Session 26F

Congress section(s): A2

One of the approaches in connexive logic suggested by Heinrich Wansing captures connexivity through a nonstandard falsity condition for the conditional. More specifically, Wansing suggests to take the condition of the form “if A is true then B is false” rather than the condition of the form “A is true and B is false” as the falsity condition for the conditional of the form “if A then B”, where truth and falsity are not necessarily exclusive. This simple idea was first formulated as a variant of Nelson’s logic N4. Some later developments observed that the central idea of Wansing does not rely on N4. Indeed, Wansing’s idea works in the context of a four-valued logic, a three-valued logic, and even in the context of weak relevant logics, namely the basic relevant logic BD of Graham Priest and Richard Sylvan, as well as in the context of conditional logics.
It should be noted that as a byproduct, connexive logics formulated a la Wansing will include the converse direction of Boethius’ theses as valid/derivable theses. Of course, these formulas are not required for connexive logics in general. In fact, these formulas are sometimes criticized. However, as Priest claims, Wansing’s system is most likely to be “one of the simplest and most natural.”
Another approach to connexivity in the literature is the one through experimental philosophy. Niki Pfeifer marked the first contribution towards this direction with a more general aim to “extend the domain of experimental philosophy to conditionals”. The particular focus is on Aristotle’s theses, and Pfeifer proposes an interpretation of Aristotle’s theses based on coherence based probability logic and offers a justification for Aristotle’s theses.
The present note focuses on another paper by Paul Egre and Guy Politzer who carried out an experiment related to the negation of indicative conditionals. In particular, they consider weak conjunctive and conditional formulas of the form “A and possibly not B” and “if A then possibly not B” respectively, beside the more well-discussed strong conjunctive and conditional formulas of the form “A and not B” and “if A then not B” respectively, as formulas equivalent to “not (if A then B)”. Many of the debates on the negation of conditionals focused on the strong forms and discussed whether the conjunctive formula is appropriate or the conditional formula is appropriate. However, Egre and Politzer challenge the debate by suggesting that we should also take into account of the weak forms, not only the strong forms.
Based on these backgrounds, the general aim behind this talk is to see if we can bridge the above approaches in connexive logics. The more specific aim is to observe that the formulas considered by Egre and Politzer can be formalized in a rather natural manner by following the idea of Wansing to consider falsity conditions of the conditional. To this end, we make use of modal logics that expand Nelson’s logic N4 developed by Sergei Odintsov and Heinrich Wansing, and offer some observations.

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COMMON SOLUTIONS TO SEVERAL PARADOXES. WHAT ARE THEY? WHEN SHOULD THEY BE EXPECTED?

Session 15L

Congress section(s): A2

In this paper I will examine what a common solution to more than one paradox is and why, in general, such a solution should be expected. In particular, I will explore why a common solution to the Liar and the Sorites should be expected. Traditionally, the Sorites and the Liar have been considered to be unrelated. Nevertheless, there have been several attempts to uniformly cope with them. I will discuss some of these attempts in the light of the previous discussion.

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SCIENCE AS CRITICAL DISCUSSION AND PROBLEM OF IMMUNIZATIONS

Session 15D

Congress section(s): B1

The value of ideal of a critical discussion is something that should be shared by scientists. It is because in the core of a critical discussion is an inter-subjective evaluation of given propositions, facts, evidence. I will argue that (A) the pursuit of this ideal can also be taken as a possible demarcation criterion for science, at least concerning its demarcation from pseudo-sciences.
Pseudo-sciences are characterized as something that wants to be or looks like science, but it is not. Uses of unfounded immunizations are one of the possible signs of pseudo-science (Derksen 1993). In general, immunizations (immunizing strategies or stratagems) prevent for a theory to be falsified or reasonably denied. This concept was initially introduced by Popper (1959/2005) as a conventionalist trick. Popper identified four types: an introduction of ad hoc hypotheses, a modification of ostensive (or explicit) definition, a skeptical attitude as to the reliability of the experimenter, and casting doubt on the acumen of the theoretician. Later, Boudry and Braeckman (2011) provided an overview of immunizing strategies identifying five different types: conceptual equivocations and moving targets, postdiction and feedback loops, conspiracy thinking, changing the rules of play, and invisible escape clauses. They also provided several examples to each type. But more importantly, they presented a definition of immunizing strategies: "[a]n immunizing strategy is an argument brought forward in support of a belief system, though independent from that belief system, which makes it more or less invulnerable to rational argumentation and/or empirical evidence."
Although I do consider immunizations as an indication of pseudo-science, I will argue that (B) immunizations are not arguments as Boudry and Braeckman proposed but rather (C) immunizations are violations of rules of a critical discussion. To support the first part of this claim (B), I will present an analysis of selected examples provided by Boudry and Braeckman using the Toulmin's model of argument (Toulmin 1958/2003). Regarding the second part (C), I will show that analyses of these examples as violations of rules of a critical discussion in pragma-dialectical theory (van Eemeren & Grootendorst 2004) is more suitable.
In conclusion, immunizations prevent a critical discussion, and therefore reasonable process where inter-subjective evaluation of claims plays a significant role. The evidence, facts, theories and similar are accepted in science by the scientific community, not by individuals. Thus, inter-subjectivity is characteristic for science, and lack of it is typical for pseudo-sciences. Therefore, (A) science can be characterized as an attempt of a critical discussion where the goal is to solve a difference of opinion by reasonable means.
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DOES RESEARCH WITH DEEP NEURAL NETWORKS PROVIDE A NEW INSIGHT TO THE AIM OF SCIENCE DEBATE?

Session 26H

Congress section(s): B1

Neural network, or so called "deep neural networks" (DNN) in particular, is one of the rapidly growing research areas, and now it is applied to many fields of science, including physics, biology, chemistry, medicine, and economics. One of the great features of DNN is that it can automatically learn features in the data set that are relevant to the given task (such as classification or prediction). On the other hand, it is not necessarily easy for humans to interpret the "reasoning" DNNs are performing in order to make correct predictions/classifications, a feature that Hooker and Hooker (2018) calls "naked prediction".
As such, the increasing use of this technology in various branches of science seems to pose an interesting question on the debate about "the aim of science". This debate originated from Bas van Fraassen's (1980) famous characterization of the scientific realism debate as opposing hypotheses about the aim of science. According to it, realists hold that science aim to obtain the true description of the world, while anti-realists (or constructive empiricists) hold that science aims at obtaining the empirically adequate theory, i.e. a theory that correctly describes the observable part of the world. Importantly, what is at stake here is not actual scientists' intention but the possibility of rational reconstruction of scientific practice as aiming at these goals. In other words, this is a debate about the accountability of scientific practice from realists' and constructive empiricists' perspectives.
Given the "naked" nature of DNNs, its use in scientific research seems to support constructive empiricism, for it seems inexplicable from realists' perspective. This is a remarkable implication because, when Van Fraassen (1980) demonstrated how various scientific practice and the reasoning behind them can be reconstructed from constructive empiricisits' point of view, his aim seemed at most creating underdetermination about the aim of science. But now, we seem to have evidence (i.e. a scientific practice) that supports constructive empiricism over realism. In this talk, however, I will point out the possibility that a certain type of research with DNN can provide evidence exclusively for scientific realism.
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DISCONTINUITY AND ROBUSTNESS AS HALLMARKS OF EMERGENCE

Session 11E

Congress section(s): B4, C9

In the last decades, the interest in the notion of emergence has steadily grown in philosophy and science, but no uncontroversial definitions have yet been articulated. Classical formulations generally focus on two features: irreducibility, and novelty. In the first case, an entity is emergent from another one if the properties of the former cannot be reduced to the properties of the latter. In the second case, a phenomenon is emergent if it exhibits novel properties not had by its component parts.

Despite describing significant aspects of emergent processes, both these definitions raise several problems. On the one hand, the widespread habit to identify emergent entities with entities that resist to reduction is nothing more than explaining an ambiguous concept through an equally puzzling notion. Just like emergence, in fact, reduction is not at all a clear, uncontroversial technical term. On the other hand, a feature such as qualitative novelty can easily appear to be an observer-relative property, rather than an indicator of the ontological structure of the world.

In view of the above, to provide a good model of emergence other features should be taken into consideration too, and the ones which I will focus on are discontinuity and robustness.

The declared incompatibility between emergence and reduction reflects the difference between the models of reality underlying them. While reductionism assigns to the structure of reality a mereological and nomological continuity, emergentism leaves room for discontinuity instead. The reductionist universe is composed of a small number of fundamental (micro)physical entities, and by a huge quantity of combinations of them. In this universe, the nature of the macroscopic entities depends upon that of the microscopic ones, and no physically independent property is admitted. Accepting the existence of genuine emergence, conversely, implies the claim that the structure of the world is discontinuous both metaphysically and nomologically. Matter is organized in different ways at different scales, and there are phenomena which are consequently scale-relative and have to be studied by different disciplines.

In this framework, emergence represents the specific trait had by macroscopic entities showing scale-relative properties which depend upon the organizational constraints of their components' relationships rather than upon their individual properties. While the laws of physics are still true and valid across many scales, other laws and regularities emerge with the development of new organizational structures whose behavior is often insensitive to microscopic constraints. And that's where the notion of robustness came into the picture. By robustness, it is intended the ability of a system to preserve its features despite fluctuations and perturbations in its microscopic components and environmental conditions. Emergent phenomena, therefore, rather than novel, are robust in their insensitivity to the lower level from which they emerge. Emergence, therefore, does not describe atypical processes in nature, nor the way in which we (cannot) explain reality. It suggests, by contrast, that the structure of the world is intrinsically differentiated, and at each scale and organizational layer correspond peculiar emergent and robust phenomena exhibiting features absent at lower or higher scales.

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CMW-REVOLUTION IN SOCIAL SCIENCES AS A TYPE OF "SCIENTIFIC REVOLUTION"

Session 6F

Congress section(s): C7

T. Kuhn defined "scientific revolution" as the transition from one paradigm to another paradigm. I.Lakatos revised this conception and discussed on the struggle of several "research programs" inside every scientific discipline, instead of full domination only of one program. But both these researchers proved their theories mainly on the basis of natural science. But how do we analyze the situation in social sciences concerning "scientific revolutions"? Russian scholar V.Stepin discussed on three paradigms in social sciences: 1) classical paradigm; 2) not-classical paradigm; 3) post-non-classical paradigm. Therefore, we have here two scientific revolutions: a "not-classical" revolution and a "post-non-classical" revolution. But we do not agree with his opinion. From our point of view, there were two paradigms in social sciences: "naturalistic" and "oecumenical", and they are divided with CMW-revolution. CMW should be decoded as "Comte – Marx – Weber". This revolution really took place in the second half of XIX and in the beginning of XX centuries. What was essence of this revolution?

Firstly, August Comte formulated a positivist paradigm; this paradigm became a basis for development of all social sciences in XIX-XX and beginning of XXI centuries. But, for instance, humanitarian sciences (cultural science, philology, literary criticism) have rejected this paradigm, and this became a ground for birth of humanitarian sciences.

Secondly, under this revolution social knowledge separate from humanitarian knowledge. At the beginning Neo-Kantianism gave a powerful impulse for this process, and Max Weber finally completed it.

And, at last, thirdly, social sciences utterly and finally have got ideologi-cal measurement; ideology intersperses in them as integral part, and finally this fact was proved by Karl Marx.

"Naturalistic" period in the development of social sciences, before CMW-revolution, could be characterized as absolute imitation of natural sci-ences (especially physics) in methodology and theory, indissolubility of social and humanitarian knowledge, absence of ideological measurement in the most of theoretical problems of social knowledge.

"Oecumenical" period in the development of social sciences, after CMW-revolution, lasts up to contemporary times. This period is character-ized with the next features: dissolubility of social and humanitarian knowledge, creation of its own "Universe" ("Oekumena") of social knowledge, different from "Universe" of knowledge of natural science, "idelogization" of any piece of social knowledge."

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DON'T BE A DEMARC-HATER: CORRECTING POPULAR MISCONCEPTIONS SURROUNDING POPPER'S SOLUTION TO THE DEMARCATION PROBLEM

Session 5G

Congress section(s): B6

Here are three common philosophic myths:

1. Falsifiability, Karl Popper's demarcation criterion, sets out the boundaries of the natural sciences from non-science or pseudo-science.
2. The criterion explicitly applies solely to singular theories that are universal in scope.
3. It is is his sole criterion of demarcation.

These three myths are each expressed in, for instance, Philosophy of Science: The Central Issues: ‚According to Popper, a theory is scientific if and only it is falsifiable' (Curd 2013, 1307). As Curd goes on to claim,

'Popper's simple idea does not work. ... [F]alsifiability is both too weak and too strong. It is too weak because it would allow as scientific any number of claims that are testable in principle but that are not, by any stretch of the imagination, scientific. It is too strong because it would rule out as unscientific many of the best theories in the history of science.' (Curd 2013, 68)

A number of objections against (1)--(3) share similar features. In brief, Popper's demarcation criteria are (so his many critics claim) too broad or too narrow in scope, thereby failing to include some (or all) paradigmatic 'scientific' theories or exclude some (or all) paradigmatic 'pseudo-scientific' theories.

As many of these philosophers of science see it, these objections directly lead to the downfall of the Popperian programme and later shifts in emphasis within the discipline. However, I argue philosophers of science and historians of philosophy of science have misrepresented and continue to misrepresent both Popper's problem of demarcation and proposed demarcation criteria.

By examining both the original German version and English translations of Logik der Forschung and the extensive oeuvre of Popper throughout his philosophic career, I show that these objections are spurious. In reality,

1*. Popper's demarcation problem is to determine if there are necessary and sufficient conditions for drawing borders between what is empirical and what is non-empirical (encompassing the domains of much of traditional metaphysics, analytic statements, normative statements, mathematics and logic).

2*. The criterion of falsifiability explicitly only applies to large sets of statements.

3*. Popper set forward a second--almost entirely neglected--criterion of demarcation that classifies individual statements as either empirical or non-empirical.

Consequently, many philosophers of science have dismissed a philosophical programme based on a mischaracterisation; they have been shadowboxing against a philosophical ghost.

That is to say, it is not disputed that many objections are effective against (1)-(3), for hardly anyone can dispute their effectiveness both as a matter of an analysis of the direction taken in history of philosophy of science away from the Popperian programme and the apparent deductive validity of many of these arguments. Nevertheless, none of the objections are sound, for these objections depend on accepting these myths and all three myths are demonstrably false. This result leads to a reevaluation of much of the Popperian programme, historical work on early to mid-20th century analytic engagement with the programme, as well as the early 20th-century debate on demarcation.

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A GAME-THEORETIC APPROACH TO EVIDENCE STANDARDS IN MEDICINE

Session 19F

Congress section(s): C1

Contributions in the philosophy of science pointing to the social character of the scientific enterprise (Goldman, 1999; Kitcher 2003; Solomon, 2001; Douglas, 2000, Steel and Whyte 2012) emphasize how the social dimensions of science are (intrinsically) intertwined with its standard epistemological goals. This is all the more evident in the health domain and the environment due to various implications for individual and public goods. In a society where science is also financed by public investments, and health and environment are constitutionally protected goods, individuals, interest groups, industry, as well as governmental agencies and institutions obviously create a complex network of dynamic, strategic interactions. This complex web may characterize each scientific domain distinctively. I focus in the present talk on medicine and the pharmaceutical industry and take the recent debates on the reproducibility crisis, the "AllTrials campaign" and various appeals to transparency and "Open Science" as a case in point, while presenting a game-theoretic approach to such phenomena.

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ON THE COMPLEXITY OF FORMULAS IN SEMANTIC PROGRAMMING

Session 27J

Congress section(s): A3

Abstract. We study the algorithmic complexity of hereditarily finite list extensions of structures [1]. The generalized computability theory based on Σ-definability, which has been developed by Yuri Ershov [2] and Jon Barwise [3], considers hereditarily finite extensions consisting of hereditarily finite sets. In the papers by Yuri Ershov, Sergei Goncharov, and Dmitry Sviridenko [4, 5] a theory of hereditarily finite extensions has been developed, which rests on the concept of Semantic Programming. In the paradigm of Semantic Programming, a program is specified by a Σ-formula in a suitable superstructure of finite lists. Two different types of implementation of logic programs on the basis of Σ-definability have been considered [6]. The first one is based on deciding the truth of Σ-formulas corresponding to the program in the constructed model. The second approach is based on the axiomatic definition of the theory of the list superstructure. Both of these approaches raise the natural question of how fast one can compute a program represented by Σ-formulas. In the recent papers [6, 7] Sergey Goncharov and Dmitry Sviridenko constructed conservative enrichment of language of bounded quantifiers by conditional and recursive terms and have put a hypotheses that in case the base model $\mathcal{M}$ is polynomially computable then deciding the truth of a given Δ_0-formula in this enriched language in a hereditarily finite list extension of $\mathcal{M}$ has polynomial complexity. Here we confirm these hypotheses and consider the complexity of this problem for a number of natural restrictions on Δ_0-formulas.

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BOLZANO, KANT AND THE EVOLUTION OF THE CONCEPT OF CONCEPT

Session 17B

Congress section(s): B6

My presentation shall discuss §120 of Bolzano's Wissenschaftslehre. Bolzano writes:

»Bin ich so glücklich hier einen Irrtum, der anderen unbemerkt geblieben war zu vermeiden, so will ich unverhohlen gestehen welchem Umstande ich es zu danken habe, nämlich nur der von Kant aufgestellten Unterscheidung von analytischen und synthetischen Urteilen, welche nicht stattfinden könnte, wenn alle Beschaffenheiten eines Gegenstandes Bestandteile seiner Vorstellung sein müssten« (Wissenschaftslehre § 120).
(„If I am so fortunate as to have avoided a mistake here which remained unnoticed by others, I will openly acknowledge what I have to thank for it, namely it is only the distinction Kant made between analytic and synthetic judgments, which could not be if all of the properties of an object had to be components of its representation" (Bolzano, WL, §120)).
Bolzano recognized Kant's insistence on the analytic/synthetic distinction as important and he drew a sharp a distinction between concept and object, like Kant. And on this distinction a new notion of the theoretical concept was crafted, because it has made both Kant as well as Bolzano, aware of the errors of the traditional notion of a concept as something established by abstraction. Kant's fundamentally significant distinction between analytic and synthetic judgments is necessarily bound to the further development of the concept beyond the traditional notion of the concepts of substance or abstraction. (Cassirer, E., 1910, Substanzbegriff und Funktionsbegriff, Berlin: Verlag Bruno Cassirer).
The whole edifice of rational knowledge therefore rested on the so-called Ontological Argument for the existence of God (Röd, W., 1992, Der Gott der reinen Vernunft, München: C.H. Beck).
The kernel of this argument is the claim that the notion of the non-existence of God is a contradiction; for God is perfect and existence is perfection. Leibniz added to this argumentation, saying "from this argument we can conclude only that if God is possible, we cannot conclude that he exists. For we cannot safely use definitions for drawing conclusions, unless we know .... that they include no contradictions" (Leibniz in: R. Ariew/D. Garber (Eds.), Leibniz, Philosophical Essays, Hackett Publ. Comp. Cambridge, p.25).
Kant emphasized that the principle of consistency only applies, if there is an object given. The statement that "a triangle has three angles", says Kant, "does not enounce that three angles necessary exist, but upon the condition that a triangle exists, three angles must necessarily exist in it" (Kant, Critique of Pure Reason, B 622). So Kant insisted on a distinction between characteristics of objects and parts of concepts. Bolzano has been the first to recognize this clearly and to understand the consequences.

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SOCIAL SCIENCES AND MORAL BIASES

Session 5F

Congress section(s): C7

Social sciences are often subject to moral biases. This phenomenon manifests itself at least in two ways. First, moral considerations can distort the research itself, by affecting the search for and the assessment of evidence, the acceptation of a thesis, the judgement about what is worth studying and the focus on a research program. As an example, Horowitz, Haynor and Kickham (2018) explain sociologists' aversion to cultural explanations by an ideological bias linked to the supposed sociology's social justice mission. Second, the moral assessment is also characteristic of the public perception (e.g., by politicians and the media) of social sciences. For instance, ex-French Prime Minister Manuel Valls, after the terrorist attack in Paris, used the expression of « culture of excuse » in order to denigrate and morally condemn sociologists who try to explain this kind of social phenomenon.
I propose first to distinguish, on the model of the distinction between normative theories in ethics, two kinds of moral biases which affect social sciences : deontological biases and consequentialist biases. The first consists in assessing a thesis in the light of its conformity with a moral norm. For example, it is often claimed, in the scientific or public debate, that such a sociological theory « disrepects » social agents by removing all sense of responsibility. In this sense, it would be morally wrong to accept this theory. The second morally assesses scientific theses by the states of affairs they bring about. A sociological theory is blamed, for instance, because it is supposed to stigmatize social agents and reinforce their behavior. In other words, it would be morally dangerous to accept this social theory.
After explaining when these kinds of assessment are really sophistical, I offer two kinds of reasons (internal and external) in order to explain why deontological and consequentialist biases affect specifically social sciences. Then, I argue, following Goldman's social epistemology way of thinking, that such assessements are epistemically deleterious for both science and society. I finally conclude by proposing some ways of neutralizing such moral biases and by defining a new « role responsibility » (Douglas, 2003) for social scientists. This responsibility is not, strictly speaking, epistemic (as is, for example, the requirement to follow the evidence wherever it leads) nor moral but « pragmatico-epistemic ». This kind of responsibility, which relates to the epistemic consequences of the social sciences and the requirement of neutralizing the moral way of problematizing social issues, should, among others, shape the way in which scientific results are presented.

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SENSORY PERCEPTION CONSTRUCTED IN TERMS OF CARNAP'S INDUCTIVE LOGIC: DEVELOPING PHILOSOPHY OF COMPUTATIONAL MODELING OF PERCEPTION

Session 30M

Congress section(s): A3, B2, C5

A central issue in contemporary analytic epistemology is whether there is a species of belief that is basic and whether there is a species of justification that is immediate. Pryor (2000) argues that (a) sensory perception has propositional content, that (b) the perceptual experience that has a propositional content P immediately justifies the content P, and that (c) the justification can be used to deny the skepticism of the external world. Afterward, Pryor also defends these claims from the arguments that perceptual experience is non-propositional and therefore it cannot be a justifier.
In this paper, I critically discuss Pryor's argument and defend the following two claims; (1) there is a species of infallible perceptual mental state that is basic and the perceptual mental state has a propositional content, and (2) the relation between a sensory perception of a particular (or demonstratives such as "this" and "that") and a belief is logical and the perceptual mental state immediately justifies its content. First of all, I will briefly introduce the problem and point out that the Pryor's argument has some problems which cannot be overlooked. Secondly, I defend the claim (1) by arguing that particulars are a certain kind of higher-order propositional items constructed from the basic mental state and that they have conceptual content. Thirdly, I defend the claim (2) by arguing that both the object of veridical perception and the object of illusion are a certain kind of higher-order propositional items constructed from identical infallible basic mental state. I attempt to give the construction of perception in terms of Carnap's inductive logic. The whole arguments are expected to defend foundationalism from the argument in Sellars (1997) and that in Davidson (1986).
Recent years, computational modeling of human perception is pursued in the discipline of natural science such as brain science and cognitive psychology. In such naturalistic pursuit, the problem of how different sense modalities such as visual and haptic are combined with is widely tackled by using the method of statistics. Lastly, in contrast to such naturalistic approach, I attempt to show how the conclusion of the paper will be helpful to those who are concerned with philosophical (utilizing traditional armchair methods of analysis) computational modeling of perception.

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OBJECTIVITY OF SCIENCE FROM THE PERSPECTIVE OF X-PHI

Session 5H

Congress section(s): B3

Objectivity as a prime scientific virtue features prominently among topics in the philosophy of science. It has been one of the defining elements of any inquiry into nature since early modern times, and gained even more significance in the later period, when it acquired its current meaning and became part and parcel of the scientific ethos. A number of authors made seminal contributions to the theoretical understanding of objectivity or created their own versions of the concept, often from diverging perspectives. As a result, the term itself diversified into a family of related concepts, and it also became a matter of contention in a number of debates.

We examine the objectivity problem yet from another perspective. Recently, a new approach was formed that uses empirical and experimental methods as a way of philosophical inquiry (see Knobe 2004, Sosa 2007, Knobe & Nichols 2008). Experimental philosophy or "x-phi" makes extensive use of methods adopted from empirical sciences, in contrast to mostly "speculative" way of traditional philosophical inquiry. In our research, we focus on concept of objectivity and its formation and understanding in the natural sciences. For this purpose, we employ tools taken from experimental philosophy, sociology and cognitive sciences, such as interviews, focus groups, questionnaires and laboratory experiments. Our research is interdisciplinary – we brought together philosophers and sociologists of science with active scientists. So, the philosophical insights are supplemented by expertise from diverse scientific backgrounds and the scientists provide us access to the scientific community.

The research is being carried out in several phases. We have already completed the qualitative empirical part of the study in which we conducted interviews and focus groups with 40+ scientists from various fields and subfields. This method has already brought valuable insights into how precisely objectivity is categorized and operationalized by the scientists themselves (as intersubjectivity, testability, approximation to the truth, precision, impartiality etc.) and also about the contemporary challenges and threats to objectivity (replicability, big data, new technologies and methods, trend-tracking, time pressures, publication overflow etc.). All this provided a better initial insight into the current scientific practices including various kinds of biases.

The next part (which is currently under way) consists from quantitative questionnaire and it will provide more detailed understanding of these topics and reveal the scope of the related issues. We use the conception of "decision vectors" (see Solomon 2001) as biasing factors (social, motivational, cognitive, ideological etc.) that have direct impact on scientific objectivity. However, following Solomon, we approach these factors as epistemically neutral, i.e. they influence the outcome (direction) of decisions in the science while this influence may or may not be conducive to scientific success. We also use experiments analogous to moral dilemmas (see Bonnefon, Sharriff, and Rahwan 2016) that are included in the questionnaire. At the Congress in Prague we would like to present the most important findings from the qualitative phase of the research, coupled with the preliminary results from the quantitative phase.

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THE INTENSIONAL AND CONCEPTUAL CONTENT OF CONCEPTS

Session 29K

Congress section(s): A4

A famous logic text, the Port Royal Logic, composed by two leaders of the Port Royal movement Antoine Arnauld and Pierre Nicole in 1662, made a distinction between the comprehension [compréhension] and the extension [étendue or extension] of an idea. The comprehension of an idea consists of "the attributes which it includes in itself, and which cannot be taken away from it without destroying it," (Arnauld and Nicole, 1996, I, 6; II, 17). The extension of an idea consists of "the subjects with which that idea agrees," or which contain it. Both the comprehensions of ideas and the extensions of ideas are used in the Port Royal Logic in justifying the basic rules of traditional logic, (ibid., II, 17-20) . However, nowadays there are at least two different ways to interpret "the comprehension of an idea", i.e. either as "the intension of a concept" or as "the conceptual content of a concept". These two things are to be distinguished as well, which will be shown in this paper below.

1. Limits of the Traditional Conceptual Content of a Concept

In traditional approach the conceptual content and the extension of a concept can be defined as follows:

I The conceptual content of a concept consists of all those attributes, i.e. concepts, which are contained in it.

II The extension of a concept consists of all those objects which fall under it.

From these two definitions the rule of inverse relation between the extension and the conceptual content of a concept follows:

#The lesser the extension of a concept, the greater is its conceptual content, and vice versa.

However, Bernard Bolzano in his Wissenschaftslehre (1837, §120) gives the following examples in order to show that the rule (#) is not always the case:

1. 'A man, who understands every European language',

and

2. 'a man, who understands every living European language'.

The conceptual content of the concept (1) is lesser than the conceptual content of the concept (2), for the concept (2) has in addition the concept of 'living' as its conceptual content. Also, the extension of the concept (1) is also lesser than the extension of the concept (2), for there are fewer people who understand every European language (including e.g. Latin) than who understand every living European language. Thus, according to Bolzano, the concepts (1) and (2) contradicts the rule (#).

3. The Intensional and Conceptual Content of a Concept

Given the Bolzano's 'counter-examples' (1) and (2), it is now possible to distinguish between the intensional and the conceptual content of concepts as well as the extension of concepts. These differences are illustrated by means of Bolzano's 'counter-examples' (1) and (2) as follows: Firstly, the intensional content of the concept (1) is greater than the intensional content of the concept (2), for the man, who understands every European language, understands also every living European language, whereas the man, who understands every living European language, does not necessarily understand every European language. Secondly, the conceptual content of the concept (1) is smaller than the conceptual content of the concept (2), for the concept (2) has in addition the concept of 'living' as its conceptual content. Thirdly, the extension of the concept (1) is smaller than the extension of the concept (2), for there are fewer people who understand every European language than who understand every living European language. Hence, it is now possible to distinguish between the intensional and the conceptual content of concepts as well as the extension of concepts.

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THE QUANTUM MEASUREMENT AS A PHYSICAL INTERACTION

Session 31K

Congress section(s): C2

The aim of the talk is to argue that there is ground for dealing with quantum measurement simply as a physical mind-independent interaction. Electrons, for example, manifest definite spin along certain axes due to physical facts or physical procedures that occur in nature; and it is these physical facts or procedures that guarantee definite results when we perform measurements. The fact that the dynamical equations of standard quantum mechanics cannot describe what exactly goes on during the measurement process does not justify reference to mind-dependence, especially when this does not solve or explain anything. The measurement problem is something that the working physicists have to face up to, and either retain standard quantum mechanics acknowledging that the world at the quantum level behaves as the theory describes or modify it or replace it with another theory.

However, what if the electron is in the |y+⟩e state and we want to measure its spin component along the x-axis? Which physical interaction forces it to adopt a value along the x-axis? Can we tell a coherent story about that kind of experiments when taking the measurement only as a physical interaction? I think we can and I will articulate one using the relevant well-known Stern-Gerlach experiment.

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COMPLEXITY OF MATHEMATICAL COGNITIVE TASKS

Session 6C

Congress section(s): C1

In studying mathematical practice, one area of great importance is identifying the cognitive processes involved in the different mathematical tasks that human agents engage in. A particularly interesting issue in this regard is the complexity of those processes. The prevalent paradigm of modelling the complexity of mathematical tasks relies on computational complexity theory, in which complexity is measured through the resources (time, space) taken by a Turing machine to carry out the task.
This reliance on computational complexity theory, however, can be a problematic fit with mathematical practice. The complexity measures used are asymptotic, meaning that they describe the complexity of functions as the arguments approach infinity (or a particular value). The mathematical tasks that human agents are involved with, however, always concern only finite, mostly relatively small inputs. In this talk, I will argue that starting from simple mathematical tasks, the human performance is not always accurately characterized by the asymptotic complexity measures. Based on empirical data on mental arithmetic, I will show that we need to rethink the complexity measures in terms of the different stages involved in tasks of mental arithmetic.
In addition, it is problematic that in computational complexity theory, the complexity of mathematical problems is characterized by optimal algorithms for solving them, i.e., Turing machines that take the least amount of time or space to reach the solution. In mathematical practice, there are many aspects which can make human algorithms for solving problems computationally suboptimal, yet still cognitively advantageous. In this talk, I will focus on two such aspects: constructing diagrams and the spatial manipulation of symbols. In terms of computational complexity, drawing a diagram can needlessly add to the complexity of the task. However, it can be an integral heuristic tool for human agents to grasp the solution to a mathematical problem. Similarly, the spatial manipulation of symbols can make an important difference for human agents while making the solution computationally suboptimal.
Why is this the case? Why do we use computationally suboptimal problem-solving strategies? I will argue that this depends on the way we are enculturated in mathematics. From our number systems to visual presentations and physical tools, the way we practice mathematics is determined in a crucial way by the mathematical culture we are located in. This, in turn, determines (at least partly) which problem-solving algorithms are optimal for us.
While computational complexity theory can make an important contribution to studying such humanly optimal algorithms, it is important to establish that there are many potential differences between computationally optimal and humanly optimal algorithms.

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FORMALISATION AND UNDERSTANDING IN MATHEMATICS

Session 28C

Congress section(s): C1

The aim of my talk should be that of introducing the FFIUM Symposium. It will, then, concern the main topic of this symposium: how formalisation (of a mathematical informal, or diversely formalised theory) can contribute to (mathematical) understanding.
I will distinguish three forms of understanding in mathematics.

i) Both the boundaries and the internal relations of conceptual dependence of informal mathematical theories are often blurred. An informal definition of natural numbers does not make it clear, for example, whether defining addition on these numbers is part of defining them, or if the latter is independent of the former, in the same way, in which informally proving a theorem within number theory may not make it clear whether the proof relies on the admission of a form of the Axiom of Choice or on predicative comprehension. Different kinds of formalizations can, then, fix both the frontiers and the conceptual dependences among concepts in different ways, thus contributing to our understanding of the content of the informal theory itself. We might call this form of understanding 'understanding by recasting'. The basic idea, here is initially due to Ken Manders. It is that mathematics provides understanding of other mathematics by conceptually recasting it. Recasting is intended here as a transformation of expressive means, and it is taken to provide understanding insofar as it enhances the grasp of the relevant content, without loss of precision or rigor. In other words, one way of providing mathematical understanding is by offering a new way to express a certain piece of mathematical content:, more focused on this content itself, and that is less open to errors caused by conceptual contamination.
ii) Once a formal theory is available, it also must be understood. An essential contribution to our understanding of a formal theory is made by our identification of it as a formalization of a particular corpus of informal mathematics. Formal (consistent) theories have models, and often these will not be isomorphic to one another. Investigating these models can be valuable for a variety of mathematical and philosophical purposes, for example proving independence theorems. But quite often, these theories are conceived to account for a previously intended model, or better a previously intend system of objects, relations and operation, intuitively fixed in a way that, in many case, depends on previous history. Establishing how the formal theory (be it categorical or not) account for this model or system, what is lost and gained in formalisation, what is related and what separated, in a second form of understanding. While understanding by recasting is understanding of an antecedent piece of mathematics (often an informal one), this new form of understanding is understanding of the very formal theory itself. One could call it 'understanding by interpretation'.
iii) In both understanding by recasting and understanding by interpretation, understanding could be seen as a process that allows to fix a content considered as invariant: a formal theory recasts the same, invariant content of a piece of antecedent mathematics; interpretation spread light on the way it does it, or can be taken to do it. Reflecting on the relations between different pieces of antecedent mathematics can, however, suggest ways for radically transforming a received content, and, thus, change the geography of mathematics itself. This provides a new form understanding. It might be called 'understating by innovation'. Formalisation can help this process by fixing a new content and also suggesting new informal ways to think at it.
After having distinguished these different forms of understanding, the talk will try to identify their possible interactions, and suggest how they might contribute to pursue epistemic economy: making as much as possible mathematical content dependent on as less as possible basic conceptual resources.

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COMPARATIVE INFINITE LOTTERY LOGIC

Session 28M

Congress section(s): A2, B2, C1, C2

Norton (2018) proposes an inductive logic for infinite lotteries and inflationary multiverse cosmologies that is radically different from Kolmogorov probability. In particular, both finite and countable additivity fail.
Norton calls the support values of this logic "chances" in order to distinguish from Kolmogorovian probabilities. The events to which he applies it are lottery outcomes (the ticket drawn is x, the ticket drawn is in set S, etc.), and random choices of pocket worlds (the chosen world is x, the chosen world is in set S, etc.). The cornerstone of his logic is the principle of Label Independence: Every fact about the chances of events remains true when the elementary outcomes (lottery tickets or pocket worlds) are relabelled by any permutation. From this Norton derives the failure of additivity. The intuitively attractive Containment Principle, which says that if a set of outcomes A is properly contained in a set of outcomes B then the chance of A is strictly less than the chance of B, is also shown to fail.
Norton argues that this logic is too weak to help us to confirm or disconfirm particular inflationary models. The Principle of Mediocracy says that we should assume that we live in a randomly chosen civilisation in the multiverse. If one model makes

it more likely that a randomly chosen world is like ours than another model does, then the former model is better confirmed in that respect. However, given Label Independence, all infinite, co-infinite sets of worlds are equally likely. Any reasonable eternal inflation model predicts infinitely many worlds like ours and infinitely many unlike ours, so on Norton's logic, each such model is equally confirmed.
However, these results depend on a reification of chance, consisting in the postulation of a chance function Ch from events to things called chances. Thus we can say, for example, Ch(ticket 7 is chosen) = C, so by Label Independence, this must remain true no matter which ticket is labelled '7'. If instead chances are purely comparative, consisting in relations of the form 'A is less likely than B', etc., then we can have an infinite lottery logic that satisfies comparative versions of Label Independence, additivity, Containment, and also regularity. (Regularity here is the property, which some find appealing, that any strictly possible event is more likely than a strictly impossible event.)
Unfortunately, even this comparative infinite lottery logic will not help us to confirm or disconfirm inflationary models. Given one inflationary model, our comparative logic may tell us that our world is more likely to be of one kind than another based on that model. However, this gives us no basis on which to say that our world is more likely to be as it is on one model than it is on another model, and thus no basis to say which model is better confirmed.
Norton, John D. (2018). Eternal Inflation: When Probabilities Fail. Synthese. https://doi.org/10.1007/s11229-018-1734-7

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ROBUSTNESS IN CONFIGURATIONAL CAUSAL MODELLING

Session 11E

Congress section(s): B1

We describe a notion of robustness for configurational causal models (CCMs, e.g. Baumgartner & Thiem (2015)), present simulation results to validate this notion, and compare it to notions of robustness in regression analytic methods (RAMs). Where RAMs relate variables to each other and quantify net effects across varying background conditions, CCMs search for dependencies between values of variables, and return models that satisfy the conditions of an INUS-theory of causation. A such, CCMs are tools for case-study research: a unit of observation is a single case that exhibits some configuration of values of measured variables. CCMs automate the process of recovering causally interpretable dependencies from data via cross-case comparisons. The basic idea is that causes make a difference to their effects, and causal structures can be uncovered by comparing otherwise homogeneous cases where some putative cause- and effect-factors vary suitably.
CCMs impose strong demands on the analysed data, that are often not met in real-life data. The most important of these is causal homogeneity – unlike RAMs, CCMs require the causal background of the observed cases to be homogeneous, as a sufficient condition for the validity of the results. This assumption is often violated. In addition, data may include random noise, and lack sufficient variation in measured variables. These deficiencies may prevent CCMs from finding any models at all. Thus, CCM methodologists have developed model-fit parameters that measure how well a model accounts for the observed data, that can be adjusted to find models that explain the data less than perfectly.
Lowering model fit requirements increases underdetermination of models by data, making model choice harder. We performed simulations to investigate the effects that lowering model-fit requirements has on the reliability of the results. These reveal that given noisy data, the models with best fit frequently include irrelevant components – a type of overfitting. In RAMs, overfitting is remedied by robustness testing: roughly, a robust model is insensitive to the influence of particular observations. This idea cannot be transported to CCM context, which assumes a case-study setting: one's conclusions ought to be sensitive to cross-case variation. But this also makes CCMs sensitive to noise. However, a notion of robustness as the concordance of results derived from different models (e.g. Wimsatt 2007) , can be implemented in CCMs.
We implement the notion of a robust model as one which agrees with many other models of same data, and does not disagree with many other models, in the causal ascriptions it makes. Simulation results demonstrate that this notion can be used as a reliable criterion of model choice given massive underdetermination of models by data. Lastly, we summarize the results with respect to what they reveal about the differences between CCMs and RAMs, and how they help to improve reliability of CCMs.
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TOWARDS A MODEL THEORY OF SYMMETRIC PROBABILISTIC STRUCTURES

Session 18A

Congress section(s): A1

Logic and probability bear a formal resemblance, and there is a long history of mathematical approaches to unifying them. One such approach is to assign probabilities to statements from some classical logic in a manner that respects logical structure.
Early twentieth century efforts in this direction include, as a partial list, work of Lukasiewicz, Keynes, Masukiewicz, Hosiasson and Los, all essentially attaching measures to certain algebras. Carnap goes somewhat further in his influential 1950 treatise "Logical foundations of probability", where he considers a limited monadic predicate logic and finite domains. The key model-theoretic formalisation is due to Gaifman, in work that was presented at the 1960 Congress of Logic, Methodology and Philosophy of Science held at Stanford — the first in the present conference series — and that appeared in his 1964 paper "Concerning measures in first order calculi". This work stipulates coherence conditions for assigning probabilities to formulas from a first order language that are instantiated from some fixed domain, and shows the existence of an assignment fulfilling these conditions for any first order language and any domain. Shortly thereafter, Scott and Krauss extended these results to an infinitary setting that provides a natural parallel to countable additivity.
In his 1964 paper Gaifman also introduced the notion of a symmetric probability assignment, where the measure given to a formula is invariant under finite permutations of the instantiating domain. When the domain is countable, such an assignment is an exchangeable structure, in the language of probability theory, and may be viewed as a symmetric probabilistic analogue of a countable model-theoretic structure. There is a rich body of work within probability theory on exchangeability — beginning with de Finetti in the 1930s and culminating in the representation theorems of Aldous, Hoover and Kallenberg — and this can be brought to bear on the study of such symmetric probabilistic structures.
A joint project of Nathanael Ackerman, Cameron Freer and myself, undertaken over the past ten years, investigates the model theory of these exchangeable structures. In this talk I will discuss the historical context for this project, and its current status.

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FREGE SEMANTICS OR WHY CAN WE TALK ABOUT DEFLATION OF FALSE?

Session 24M

Congress section(s): A2

The followers of the deflationary model of truth [1;3;4] believe that, having easily coped with the task of eliminating "truth", they finally can solve most difficult problem for them – the need to include truth as a value of propositions in scientific knowledge. Have done it, they are faced with the need of elimination the "false." But this leads to a paradox. To justify this let's remind Frege's statements:
(I). Every true sentence denotes the truth;
(II). Every false sentence denotes the false [2].
Now let's consider two types of sentences: (1) "True sentence," in Frege's sense, is a sentence that itself has that sign ("to be true"), which it designates as an object. The suggestion, that the "true sentence", which "denotes truth as an object," is itself not true leads us to the absurd. Now consider the second type of sentence - "false" (defined as negation of "true sentences (p)" : (2) A "false sentence" is not a true sentence, that is, ¬ p . Further will give more complete definition:

(A) The sentence that «true sentences have the feature of “being true”» is a sentence in which some term p (“ true sentence”) itself has that feature of X (“ to be true “), which it designates as an object.

Consequently, according to the definition of false sentences (2), it turns out that the sentence “every false sentence denotes a false” should look like this:

(B) “Any non-true sentence (¬ p) denotes non-truth Y” Further ask two questions: Question (α), «Can the proposition “Every true sentence which denotes the truth” be true?». Answer is “yes, it can”.

Question (β): «Can the proposition “Every false sentence which denotes the false” be false?». In this case we face a problem. To demonstrate it let’s understand the quoted statement (II) “every false sentence denotes false” - only the name of that false sentence, which denotes false. At the same time, the unquoted expression that every false sentence denotes false we will regard as an object judged by the sentence-name (quoted). Further let’s give our expression stricter form in accordance with (2) “ ¬ p.”

We get: The sentence “every false sentence denotes false” denotes every false sentence denoting false, if and only if, “every false sentence denoting false” is not a sentence denoting the truth.

Now, instead of expression “denoting the truth”, we substitute the definition of a sentence denoting the truth:

The sentence “every false sentence denotes false” denotes every false sentence denoting false, if and only if, “every false sentence denoting false” is not every true sentence denoting the truth.

As a result, we come to the paradox: “false sentences at the same time have the sign “to be false (not true)” (¬ p) and do not “have them (¬ ¬ p)”, or, given that (¬ ¬ p = p) in formal expression: ¬ p ↔ p

The final result is: it is impossible to agree with Frege in this matter - such object as “false” does not exist.

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ON CONDITIONS OF INFERENCE IN MANY-VALUED LOGIC SEMANTICS OF CL_2

Session 28M

Congress section(s): A2

The main aim of this paper is to find conditions of inference in many-valued logic semantics of two-valued CL_2. J-operators will be used due to J. Rosser and A. Turquette. Let’s have a n-valued logic L_n with one designated truth-value 1 and anti-designated truth-value 0. J_1-operator corresponds to 1. J_0-operator corresponds to 0. The set of L_n-formulae is L_n-*For*. If S is L_n-formula, then $J_1(S), J_0(S)$ are TF-formulae. If P_1, P_2 are TF-formulae, then $J_1P_1, J_0P_1, =P_1$ and $(P_1 \Rightarrow P_2)$ are TF-formulae, where $=-$ $-It\ is\ false\ that$,$\Rightarrow$ – if...then . P, P_1, denote meta-variables for TF-formulae. Set of TF-formulae is TF-*For*. It is asserted CL_2(TF-*For*, $=,\Rightarrow$). If A is L_n-formula or A is TF-formula, then A is formula. A, A_1, denote meta-variables for formulae. Set of formulae is *For*. Axioms for J-operators:

$(J_1P \Leftrightarrow P)$,

$(J_0P \Leftrightarrow =P)$,

$((J_kA \wedge J_mA) \Rightarrow (k = m))$. Adding rules of inference: $\dfrac{A}{J_1A}$,

$\dfrac{J_1A}{A}$ Definition 1. $(A_1 \supset A_2) =_{df} (J_1A_1 \Rightarrow J_1A_2)$, $\neg A =_{df} =J_1A$.

Then formatting rules, axioms and modus ponens of logic $CL_n(For, =,\Rightarrow)$ with n-valued (non-main by A. Church) interpretation are inferred. Theorem 1. 1.1. $(J_1A \vee J_0A) \Rightarrow (J_1A \Rightarrow J_0\neg A)$ 1.2. $(J_1A \vee J_0A) \Rightarrow (J_0A \Rightarrow J_1\neg A)$ 1.3. $((J_1A_1 \vee J_0A_1) \wedge (J_1A_2 \vee J_0A_2)) \Rightarrow ((J_0A_1 \vee J_1A_2) \Rightarrow J_1(A_1 \supset A_2))$ 1.4. $((J_1A_1 \vee J_0A_1) \wedge (J_1A_2 \vee J_0A_2)) \Rightarrow ((J_1A_1 \wedge J_0A_2) \Rightarrow J_0(A_1 \supset A_2))$ Note that right part of theorem 1 correspond to semantic rules of two-values (main) interpretation of CL_2. Let’s have a set Q2 which (Q2 $\subset$ *For*) and for all Q if ($Q \in$ Q2), then $(J_1Q \vee J_0Q)$. Theorem 2. *If exist* Q2, *then* CL_2(Q2, $\neg, \supset$) *is inferred*. Therefore conditions of inference in many-valued logic semantics of two-valued CL_2(Q2, $\neg, \supset$) are existence of Q2 which (Q2 $\subset$ *For*) and for all Q if ($Q \in$ Q2), then $(J_1Q \vee J_0Q)$.

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ON RANKS FOR FAMILIES OF THEORIES OF ABELIAN GROUPS

Session 14K

Congress section(s): A1

Abstract. We continue to study families of theories of abelian groups [1] characterizing *e*-minimal subfamilies [2] by Szmielew invariants $\alpha_{p,n}$, β_p, γ_p, ε [3, 4], where $p \in P$, P is the set of all prime numbers, $n \in \omega \setminus \{0\}$, as well as describing possibilities for the rank RS [2]. We denote by $\mathcal{T}_A$ the family of all theories of abelian groups.

Theorem 1. *An infinite family $\mathcal{T} \subseteq \mathcal{T}_A$ is e-minimal if and only if for any upper bound $\xi \geq m$ or lower bound $\xi \leq m$, for $m \in \omega$, of a Szmielew invariant*

$$\xi \in \{\alpha_{p,n} \mid p \in P, n \in \omega \setminus \{0\}\} \cup \{\beta_p \mid p \in P\} \cup \{\gamma_p \mid p \in P\},$$

there are finitely many theories in $\mathcal{T}$ satisfying this bound. Having finitely many theories with $\xi \geq m$, there are infinitely many theories in $\mathcal{T}$ with a fixed value $\alpha_{p,s} < m$, if $\xi = \alpha_{p,n}$, with a fixed value $\beta_p < m$, if $\xi = \beta_p$, and with a fixed value $\gamma_p < m$, if $\xi = \gamma_p$.

Theorem 2. *For any theory T of an abelian group A the following conditions are equivalent:* (1) *T is approximated by some family of theories;* (2) *T is approximated by some e-minimal family;* (3) *A is infinite.*

Let $\mathcal{T}$ be a family of first-order complete theories in a language Σ. For a set Φ of Σ-sentences we put $\mathcal{T}_\Phi = \{T \in \mathcal{T} \mid T \models \Phi\}$. A family of the form $\mathcal{T}_\Phi$ is called *d-definable* (in $\mathcal{T}$). If Φ is a singleton $\{\varphi\}$ then $\mathcal{T}_\varphi = \mathcal{T}_\Phi$ is called *s-definable*.

Theorem 3. *Let α be a countable ordinal, $n \in \omega \setminus \{0\}$. Then there is a d-definable subfamily $(\mathcal{T}_A)_\Phi$ such that $\mathrm{RS}((\mathcal{T}_A)_\Phi) = \alpha$ and $\mathrm{ds}((\mathcal{T}_A)_\Phi) = n$.*

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COMBINING TRUTH VALUES WITH PROVABILITY VALUES: A NON-DETERMINISTIC LOGIC OF INFORMAL PROVABILITY

Session 28M

Congress section(s): A2

Mathematicians prove theorems and are not committed to any particular formal system. They reason in a semi-formal setting using informal proofs. According to the proponents of the standard view (Enderton, 1977; Antonutti Marfori, 2010) informal proofs are just incomplete formal derivations — in principle, an informal proof can be associated with a formal proof in a fully-formal system, usually some version of set theory. There are quite a few reasons not to reduce informal provability to formal provability within some appropriate axiomatic theory (Antonutti Marfori, 2010; Leitgeb, 2009).

The main worry about identifying informal provability with formal provability starts with the intuition that whatever is informally provable is true. This means that when we do informal proofs, we are committed to all the instances of reflection schema: if φ is informally provable, then φ is true. However, this principle is not provable in any decent axiomatic theory of its own formal provability predicate. Moreover, no such theory can even be extended with the schema, provided some other non-controversial principles for formal provability are retained. One of the approach to regain the reflection is to treat informal provability as a partial notion.

Pawlowski and Urbaniak (2018) developed a framework in which informal provability is modeled by a non-deterministic three-valued logic CABAT. Semantics in CABAT is based on an intuitive partition of mathematical claims into provable (value 1), refutable (value 0), and neither (value n). The main reason to use non-deterministic semantics is that the value of a complex formula in deterministic logics is always a function of the values of its components. This fails to capture the fact that, for instance, some informally provable disjunctions of mathematical claims have informally provable disjuncts, while some other don't.

Alas, two main problems with the system they proposed appeared. First, the CABAT reading of the reflection schema does not connect informal provability with truth, since CABAT strictly speaking does not have truth-values. The other problem is related to the natural asymmetry between truth and informal provability. The latter implies the former but not the other way around. So, ideally, we want to have a difference between both: the reflection schema and provabilitation ($\varphi \to B\varphi$), and between strong NEC and CONEC. Unfortunately, CABAT cannot make all these distinctions.

In this talk, we propose a logic that follows the same motivations as CABAT, but whose semantics incorporates truth values along with provability values. We develop a four-valued non-deterministic logic T-BAT (provable and true, refutable and false, provable and neither, and refutable and neither), which does a better job as the logic of informal provability than CABAT, since it can prove the reflection schema, can distinguish between truth values and provability values, and preserves the intuitive asymmetries between truth and provability.

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CAN MACHINE LEARNING EXTEND BUREAUCRATIC DECISIONS?

Session 11F

Congress section(s): B5, C6

In the recent literature, there has been much discussion about the explainability of ML algorithms. This property of explainability, or lack thereof, is critical not only for scientific contexts, but for the potential use of those algorithms in public affairs. In this presentation, we focus on the explainability of bureaucratic procedures to the general public.The use of unexplainable black-boxes in administrative decisions would raise fundamental legal and political issues, as the public needs to understand bureaucratic decisions to adapt to them, and possibly exerts its right to contest them. In order to better understand the impact of ML algorithms on this question, we need a finer diagnosis of the problem, and understand what should make them particularly hard to explain. In order to tackle this issue, we turn the tables around and ask: what makes ordinary bureaucratic procedures explainable? A major part of such procedures are decision trees or scoring systems. We make the conjecture, which we test on several cases studies, that those procedures typically enjoy two remarkable properties. The first is compositionality: the decision is made of a composition of subdecisions. The second is elementarity: the analysis of the decision ends on easily understandable elementary decisions. The combination of those properties has a key consequence on explainability, which we call explainability by extracts: it becomes possible to explain the output of a given procedure, through a contextual selection of subdecisions, without the need to explain the entire procedure. This allows bureaucratic procedures to grow in size without compromising their explainability to the general public.
In the case of ML procedures, we show that the properties of compositionality and elementarity correspond to properties of the segmentation of the data space by the execution of the algorithm. Compositionality corresponds to the existence of well-defined segmentations, and elementarity corresponds to the definition of those segmentations by explicit, simple variables. But ML algorithms can loose either of those properties. Such is the case of opaque ML, as illustrated by deep learning neural networks, where both properties are actually lost.This entails an enhanced dependance of a given decision to the procedure as a whole, compromising explainability by extracts. If ML algorithms are to be used in bureaucratic decisions, it becomes necesary to find out if the properties of compositionality and elementarity can be recovered, or if the current opacity of some ML procedures is due to a fundamental scientific limitation.

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DUALITY AND INTERACTION: A COMMON DYNAMICS BEHIND LOGIC AND NATURAL LANGUAGE

Session 12H

Congress section(s): A1, C1, C7

The fact that some objects interact well together – say, a function with an argument in its domain of definition, whose interaction produce a result – define a notion of duality that has been central in the last-century mathematics. Not only does it provide a general framework for considering at the same time objects of interests and tests (or measures) on them, but it also provides a way to both enrich and restrict the objects considered, by studying a relaxed or strengthened notion of interaction.
A reconstruction of logic around the notion of interaction have been underway since the pi- oneering works of Krivine and Girard where (para-)proofs are seen as interacting by exchanging logical arguments, the interaction stopping successfully only if one the two gives up as it recognises to lack arguments. All the proofs interacting in a certain way – for instance, interacting correctly with the same proof – can then be seen as embodying a certain formula; and the possible operations on proofs translate into operations on formulæ.
In this work, we intend to show that, somewhat surprisingly, the same approach in terms of duality and interaction succeeds in grasping structural aspects of natural language as purely emergent properties. Starting from the unsupervised segmentation of an unannotated linguistic corpus, we observe that co-occurrence of linguistic segments at any level (character, word, phrase) can be considered as a successful interaction, defining a notion of duality between terms. We then proceed to represent those terms by the distribution of their duals within the corpus and define the type of the former through a relation of bi-diuality with respect to all the other terms of the corpus. The notion of type can then be refined by considering the interaction of a type with other types, thus creating the starting point of a variant of Lambek calculus.
This approach has several precursors, for instance Hjelmslev's glossematic algebra, and more generally, the structuralist theory of natural language (Saussure, Harris). The formal version we propose in this work reveals an original relation between those perspectives and one of the most promising trends in contemporary logic. We also include an implementation of the described algorithm for the analysis of natural language. Accordingly, our approach appears as a way of analyzing many efficient mechanized natural language processing methods. More generally, this approach opens new perspectives to reassess the relation between logic and natural language.
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REALISM ABOUT MOLECULAR STRUCTURES

Session 7L

Congress section(s): B4, C2

Since the beginning of modern chemistry in the 19th century, chemists have represented molecules with various figures and shapes. Towards the latter half of the 19th century, chemists moved from representing molecules as two dimensional planar objects, to three dimensional objects with a particular orientation in space. While there have been evolving models of the chemical bond, many chemists still think about molecular structures in realist terms. That is, many chemists think that molecular models as depicted by contemporary chemistry, are true or approximately true representations of molecular structures (Hendry, 2012). However, some argue that quantum field theory provides a powerful objection to realism about molecular structures. In our paper we set out to do two things.
First, we outline the basic contours of the standard chemical theory of molecular structures, or "structure theory." Structure theory is a basic framework for understanding molecules, their constituent parts, and the nature of the chemical bond, and provides a robust theoretical basis for making predictions and explaining disparate phenomena. We go on to explore some of the heavy lifting done by structure theory, including applications in molecular symmetry and molecular orbital theory. We conclude this section by arguing that structure theory and its applications provide a strong, empirically informed philosophical argument for "molecular realism" (i.e. realism about molecular structures).
Second, we consider an objection to molecular realism based on quantum theory. Quantum theory recognizes the wave-particle duality of matter and provides a framework to describe how particle energy is quantized--that is, how particles absorb and release energy in discrete amounts. The Schrodinger Equation in quantum theory describes the wavefunction of a system, including particles within the system. Chemists tend to utilize the Born Interpretation according to which the

amplitude of the wave function is interpreted as a probability distribution. Philosophers and scientists have considered the implications of quantum theory for realism. For example, Richard Dawid (2018) notes, “In a number of ways, high-energy physics has further eroded what had survived of the intuitive notion of an ontological object that was badly damaged already by quantum physics and quantum field theory.” Similarly, Hasok Chang (2016) observes that while “numerous experimental interventions rely on the concept of orbitals...[they] have no reality if we take quantum mechanics literally.” However, we argue that uncertainties about orbitals and the precise natures of subatomic particles does not undermine the plausibility of molecular realism. More specifically, we argue that our current understanding of molecular symmetry provides strong evidence for molecular realism that is able to withstand objections to realism based on quantum theory.

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ECUMENISM: A NEW PERSPECTIVE ON THE RELATION BETWEEN LOGICS

Session 25C

Congress section(s): A1, A2

A traditional way to compare and relate logics (and mathematical theories) is through the definition of translations/ interpretations /embeddings. In the late twenties and early thirties of last century, several such results were obtained concerning some relations between classical logic (CL), intuitionistic logic (IL) and minimal logic (ML), and between classical arithmetic (PA) and intutionistic arithmetic (HA). In 1925 Kolmogorov proved that classical propositional logic (CPL) could be translated into intuitionistic propositional logic (IPL). In 1929 Glivenko proved two important results relating (CPL) to (IPL). Glivenko’s first result shows that A is a theorem of CPL iff ¬¬A is a theorem of IPL. His second result establishes that we cannot distinguish CPL from IPL with respect to theorems of the form ¬A. In 1933 Gödel defined an interpretation of PA into HA, and in the same year Gentzen defined a new interpretation of PA into HA. These interpretations/translations/embeddings were de- fined as functions from the language of PA into some fragment of the language of the HA that preserve some important properties, like theoremhood. In 2015 Dag Prawitz (see [3]) proposed an ecumenical system, a codification where classical logic and the intuitionistic logic could coexist “in peace”. The main idea behind this codification is that the classical logic and the intuitionistic logic share the constants for conjunction, negation and the universal quantifier, but each has its own disjunction, implication and existential quantifier. Similar ideas are present in Dowek [1] and Krauss [2], but without Prawitz’ philosophical motivations. The aims of the present paper are: (1) to investigate the proof theory and the semantics for Prawitz’ Ecumenical system (with a particular emphasis on the role of negation), (2) to compare Prawitz’ system with other ecumenical approaches, and (3) to propose new ecumenical systems.

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PATHS OF ABSTRACTION: BETWEEN ONTOLOGY AND EPISTEMOLOGY IN MATHEMATICAL PRACTICE. THE ZILBER’S TRICHOTOMY THROUGH THE LENS OF LAUTMAN AND CAVAILLÈS

Session 3F

Congress section(s): C1

Boris Zilber’s famous tricotomy conjecture was stated around 1983 and was the result of several earlier results and observations. Its original formulation had the intention of providing a classification of an important class of “natural” structures in mathematics, namely strongly minimal structures. These correspond to a model theoretic formulation close to categoricity but with very specific classification tools, and includes important classical structures such as algebraically closed fields, vector spaces — two whole areas of mathematical practice are roughly anchored there (classical algebraic geometry and in a general sense, all “linear” structures). The original statement was shown to be false in 1990 by Ehud Hrushovski: he built a strongly minimal structure that does not fit the original tricotomy classification as originally envisioned by Zilber. Later, Zilber and Hrushovski proved a version of the tricotomy for a reduced class of structures, namely, “Zariski geometries”.

In their proof, when they establish the tricotomy, one of the three classes requires building bi-interpretations (basically, if a Zariski geometry is not modular then the corresponding model is bi-interpretable with some field). This works as intended, up to a finite cover. The situation could have been more direct, not involving any such finite cover, but the final result provides bi-interpretability up to a “finite-to-one map”. This state of affairs is taken further afield by Zilber in later work (Non Commutative Geometry, 2005 and Applications to the Model Theory of Physics, 2006).

This raises various questions that we consider worth studying under the light of the philosophy of mathematical practice. For instance; why Zilber’s insistence on further looking for (what he calls) “natural” (or even “a priori”) examples coming from classical mathematics, and corresponding to these finite covers? (The response is yet far from complete from the purely mathematical perspective, but several structures stemming from mathematical physics are now considered very natural candidates.) And why is there an apparent difference between Zilber’s insistence as opposed to Hrushovski’s more “pragmatic” attitude? Is this a mere psychological difference or else is there a deeper philosophical reason? We argue the latter in our presentation: Zilber and Hrushovski follow different kinds of generalization, different types of abstraction. We propose a case analysis of these types of abstraction.

We propose a difference between what we may call ontological abstraction as opposed to epistemological abstraction. The main difference, in philosophical terms, centers in the former’s emphasis on what are structures and where they “live”, versus the latter’s emphasis on how we get to know mathematical structures. Naturally, we do not claim that these two types of abstraction reside in an absolute sense in the mind of a specific mathematician; we do not even claim that the two forms of abstraction are mutually exclusive. But we do observe a certain prevalence of one form over the other in specific cases of mathematical abstraction, illustrated by the story told above.

In mathematical practice, one type of abstraction is able to capture relationships between mathematical objects that the other type of abstraction is not (necessarily). In our case study, Zilber appears to have a motivation of a stronger ontological character — and this enabled him to visualize the novelty of the solution beyond the original tricotomy technical solution, whereas the more epistemological character of Hrushovski’s kind of abstraction enabled him to see the original failure of tricotomy. In the end, the tension between these two types of abstraction results in an iterated process where further abstraction is “fueled” by the alternation of two contending modes. Zilber’s “stubbornness” and Hrushovski’s “pragmaticism” are a more contemporary version of epistemological abstraction (stemming from Bourbaki); however, in many ways Zilber’s insistence (stubbornness?) conceals a kind of abstraction that we perceive as quite novel compared with the predominant forms of abstraction of the past century.

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MECHANISTIC EXPLANATIONS AND COMPONENTS OF SOCIAL MECHANISMS

Session 24K

Congress section(s): C7

This paper addresses the question of what the components of social mechanisms in mechanistic explanations of social macro-phenomena must be (henceforth "the question of components"). The analytical sociology's initial position and new proposals by analytical sociologists are discussed. It is argued that all of them are faced with outstanding difficulties. Subsequently, a minimal requirement regarding the components of social mechanisms is introduced.
The two core ideas of analytical sociology are the principle of mechanism-based explanations and structural individualism (Hedström 2005). It is believed that, in social sciences, the principle of mechanism-based explanations implies structural individualism. The reason is that there are social mechanisms only at the individual level (Hedström 2005). The commitment with structural individualism leads to analytical sociology's initial position regarding the question of components: a social mechanism in an explanation of a social macro-phenomenon must be composed of individuals, their properties, actions, and relations. In response to that proposal, it has been argued that it is unlikely to be the case that all social mechanisms are at the individual level (Vromen 2010). Therefore, the principle of mechanism-based explanations does not imply structural individualism.
Given that critique against analytical sociology's initial position, different answers have been raised by analytical sociologists. Michael Schmid (2011), who maintains analytical sociology's initial position, introduces a new argument in support of structural individualism. He argues that mechanism-based explanations require laws and that, in social sciences, laws are available only at the individual level. The problem is that his argument is based on a very questionable premise. Mechanistic explanations do not require laws. In fact, they have been developed as an alternative to those explanations that require laws.
Unlike Schmid, Ylikoski (2012) partially gives up the analytical sociology's initial position. He distinguishes between causal and constitutive mechanism-based explanations, and maintains analytical sociology's initial position only regarding constitutive explanations. He considers that in constitutive mechanism-based explanations structural individualism must be fulfilled, because constitutive relevant entities, properties, activities, and relations are always at the individual level. Structural individualism, as it has been noted, leads to the analytical sociology's initial position. However, it is unlikely to be the case that constitutive relevant entities, properties, activities, and relations are always at the individual level. Consider properties of parliaments (e.g. being conservative). Properties of political parties, which are not at the level of individuals, are constitutive relevant for them.
There is not a fixed level to which components of social mechanisms must always belong. Neither in causal mechanism-based explanations nor in constitutive mechanism-based explanations, components of social mechanisms must be always at certain fixed level. Nevertheless, a minimal requirement can be raised: a component of a social mechanism in a mechanistic explanation of a social macro-phenomenon must not have the explanandum phenomenon as a part (proper or improper) of it. This requirement applies to both causal and constitutive explanations.
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MARGARET CAVENDISH ON CORPOREAL QUALITIES

Session 25G

Congress section(s): B6

Margaret Cavendish is insistent that matter and many of its qualities are ‚all one thing', and that there are no accidents, forms or modes in nature. But it is very hard to see what she means by such claims, especially given the great variety of qualities that they concern. In this paper, I discuss Cavendish's arguments that various qualities are ‚all one thing' with matter, and offer interpretations of this claim first, for all qualities but motion, and second, for motion. I consider passages that suggest that qualities are identical with matter that is distinct from the matter that bears them, but argue that ultimately, Cavendish thinks that all qualities besides motion are nothing over and above patterns in the matter that bears them. I close with an account of what Cavendish means when she writes that motion is identical with body. Cavendish's positions and arguments, I show, are situated interestingly among others in the history of philosophy, but they represent together a unique and radical approach to reductionism about accidents.

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SCIENTIFIC EXPLANATIONS AND PARTIAL UNDERSTANDING

Session 12K

Congress section(s): B1

Notions such as partial or approximate truth are often invoked by proponents of the factual accounts of understanding in order to address the problem of how flawed theories can provide understanding of their factual domain. A common problem of such arguments is that they merely do a lip service to such theories of truth instead of exploring them more fully. This is a perplexing fact, because a central feature of factual accounts is the so called veridical condition. The veridical condition itself appears as a result of a broadly inferential approach to understanding according to which only factually true claims can figure within an explanans capable of generating understanding of its explanandum.
Here I aim at amending this issue by exploring Da Costa's notion of partial truth and liking it with a factual analysis of understanding. As a result of pursuing such an account several interesting features of explanatory arguments and understanding will emerge.
Firstly, partial truth naturally links with the intuition that understanding comes in degrees. This appears straightforwardly from the fact that an explanation that contains partially true propositions can only provide partial explanatory information for its explanandum.
Secondly the distinction between theoretic and observational terms on which the notion of partial truth relies, permits us to be clear on the problem when will an explanation provide partial understanding. This can be the case only if the explanans has premises which contain theoretic concepts. Only such premises that relate theoretic and observational terms can be taken as partially true (premises that contain descriptive terms only can be simply assessed as true or false). As a result of such partiality the information transfer from premises to conclusion can be only partially factually accurate, which subsequently leads to partial understanding.
The resulting account of understanding then resolves the core problem that modest factual accounts face—namely, that if a partially factual account of understanding is accepted, then this account should also show by what means a partially true proposition figures centrally in an explanatory argument and explain how flawed theories can make a positive difference to understanding.
I will further support my case by a critical examination of predator-prey theory and the explanatory inferences it generates for 2 possible population states – the paradox of enrichment and the paradox of the pesticide. The paradox of the pesticide is the outcome of predator-prey dynamics according to which the introduction of a general pesticide can lead to the increase of the pest specie. The paradox of enrichment is an outcome of predator prey dynamics according to which the increase of resources for the prey species can lead to destabilization. Both of these outcomes depend on idealized conceptualization the functional response within predator-prey models. This idealization can be assessed as introducing a theoretic term. The

explanatory inferences using such a notion of functional response can then be judged only as approximately sound (paradox of the pesticide) or unsound (paradox of enrichment) and providing only partial understanding of predator-prey dynamics.

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BRIDGING ACROSS PHILOSOPHY OF SCIENCE AND SCIENTOMETRICS: TOWARDS AN EPISTEMOLOGICAL THEORY OF CITATIONS

Session 25I

Congress section(s): B3, B5

Citations are a crucial aspect of contemporary science (Biagioli 2018). Citation-based indicators such as the Journal Impact Factor are commonly employed by scientists to choose the publication venue for their articles, whereas indicators such as the h-index are used (and frequently misused) by university administrators to monitor and evaluate the research performance of academics. The implementation of performance-based research evaluation systems in many European and extra-European countries has further speeded up the proliferation of metrics in which citations are often a crucial component. Thus, scientometrics, the discipline that investigates the quantitative dynamics of citations in science, has risen from relative obscurity to play a major, and often much criticized, role within the science system (De Bellis 2014).
Unfortunately, citations have mostly escaped the attention of philosophers of science, maybe because they are relegated to the "context of discovery" of science (Leydesdorff 1998). Philosophers of science have not yet joined the discussion around a comprehensive theory of citations. This paper aims at beginning to close the gap between scientometrics and philosophy of science by advancing an epistemological theory of citations as a bridge between the two fields.
Firstly, I will present the two main competing theories of citation developed in the sociology of science: the normative theory and the socio-constructivist theory, grounded respectively in the normative and in the social constructivist approach in the sociology of science.
Secondly, I will show that these theories share the same explanandum as a target: they both assume that the key aim of a theory of citation is to uncover the motivations that scientists have for citing.
Thirdly, I will propose to shift the focus from the behavior of the scientists to the epistemological function of citations within scientific documents (Petrovich 2018). I will argue that citations can be considered as information channels between the citing document and the cited texts. In this way, the focus is no more on the motivations of scientists for citing (sociological perspective), but on the dynamic of scientific information that is made visible by citations (epistemological perspective).
Lastly, I will claim that the transformation of scientific information into scientific knowledge can be studied by analyzing the dynamics of the citation network of scientific documents. Drawing on the Khunian distinction of pre-paradigmatic, normal, and revolutionary science, I will argue that different citation structures characterize each of these phases. Thus, I will conclude that citation analysis is an important tool to investigate the epistemological development of scientific fields from the pre-paradigmatic to the normal period.
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HIGH-ENERGY PHYSICS CULTURES DURING THE COLD WAR: BETWEEN EXCHANGES AND TRANSLATIONS

Session 30G

Congress section(s): B3

We conduct a sociological and historical analysis of the collaboration between American and Soviet scientists during the chain of experiments in high energy physics that took place in the 1970s (Pronskikh 2016). We consider each of the laboratories of the 1970s as a culture in and of itself and we investigate processes of exchange of material objects and translations of values and interests between different types of individual actors (scientists, politicians, managers) as well as between cultures (Soviet and US high-energy physics laboratories). Bearing upon a case study of a collaboration between Joint Institute for Nuclear Research (JINR, Dubna) and Fermi National Accelerator Laboratory (Fermilab, USA) in 1970-1980 (the period of the Cold War often referred to as "détente"), we examine how the supersonic gas jet target manufactured in JINR and delivered to Fermilab for joint use influenced the scientific culture at Fermilab, contributed to the birth of long-standing traditions as well as contributed to changing scientific policy toward an alleviation of the bi-laterality requirement in scientific exchanges between the Eastern and the Western blocks. We also focus on how processes in scientific cultures that at the time arose from their interactions influenced some of the epistemic goals of the endeavor.
We examine the experiment that is premised on an international collaboration within three frameworks: as a trading zone (Galison 1997), where various types of objects and values (not only epistemological) are being exchanged through intermediate languages forming; as a network composed of heterogeneous actors and their interactions accompanied by translations of interests (Latour 1987); as a locus of exchanges, sharing, and circulations which make cultures temporary formations (Chemla 2017). The developments that took place within the experiment can be described as the translation of different types of interests, including political, business, private and public aims. Moreover, the same actors could pursue different types of interests at different times. That prevents us from drawing rigid boundaries between "content" and "context" in science. As a consequence, one more problem arises: if one does not acknowledge such a distinction a priori, how can one eventually identify scientific cultures that act in the course of these translations and distinguish them from other cultures? One possible answer lays in the investigation of the values that circulate in a particular culture or between two or more interacting cultures and shape them, which reveals their historical variability.
The reported study was funded by RFBR according to the research project № 18-011-00046
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ANALYSIS OF INCORRECT PROOFS

Session 27M

Congress section(s): A2

Analysis of proofs is an open and ongoing topic in general proof theory. The focus of interest is, however, often limited to correct proofs only. In this talk, we outline a general framework that can analyze both correct and incorrect proofs.

As a motivating example, consider, e.g., the derivation of “A” from “A implies A” and “A”. Judging by this information alone, it certainly seems as a correct derivation certified by the implication elimination rule (modus ponens). But what if this derivation was not carried out using the implication elimination rule, but the via the formal fallacy known as affirming the consequent, which has the general form: from “A implies B” and “B” is inferred “A”. The standard approaches to proof analysis based around the Curry-Howard isomorphism (where proofs, or rather, proof terms are understood as typed lambda terms; see e.g., Sørensen & Urzyczyn 2006, Negri & von Plato 2011) are not well equipped to adequately analyze this situation. Our goal will be to sketch the basis of a logic of incorrect proofs capable of analyzing scenarios such as the one described above. More specifically, we would like to treat even incorrect proofs as first-class objects of our theory: we should be able to reason about them, talk about them or use them as arguments for functions, etc. Rather than to try to modify the current approaches to include even incorrect proofs, we will develop a framework that expects the possibility of incorrect proofs from the beginning. Our background formalism of choice will be Transparent Intensional Logic (TIL; Tichý 1988, Duží et al. 2010, Raclavský et al. 2015). We choose it because it is a highly expressive system, built from ground up around partial functions and neo-Fregean procedural semantics, both of which will be instrumental for our analysis of incorrect proofs.

In this talk, we will: i) introduce and demonstrate TIL-based proof analysis (how does it compare to the current mainstream frameworks for proof analysis?), and ii) propose how to analyze incorrect proofs (what is the semantics of incorrect proofs?).

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ARE CONNEXIVE PRINCIPLES COHERENT?

Session 25F

Congress section(s): A2

While there is a growing interest in studying connexive principles in philosophical logic, only little is known about their behaviour under uncertainty. In particular: can connexive principles be validated in probability logic? Among various approaches to probability, I advocate the coherence approach, which traces back to Bruno de Finetti. In my talk, I discuss selected connexive principles from the viewpoints of coherence-based probability logic (CPL) and the theory of conditional random quantities (CRQ). Both CPL and CRQ are characterised by transmitting uncertainty coherently from the premises to the conclusions. Roughly speaking, the theory of CRQ generalises the notion of conditional probability to deal with nested conditionals (i.e., conditionals in antecedents or consequents) and conjoined conditionals (like disjunctions and conjunctions of conditionals) without running into Lewis‘ triviality results. Within the frameworks of CPL and of CRQ, I investigate which connexive principles follow from the empty premise set. Specifically, I explain why Boethius‘ theses, Abelard’s First Principle, and in particular Aristotle’s theses do hold under coherence and why Aristotle’s Second Thesis does not hold under coherence. Although most of these connexive principles have a high intuitive appeal, they are neither valid in classical logic nor in many other logics (including those which were custom-made for conditionals). In the CPL and CRQ frameworks for uncertain conditionals, however, intuitively plausible connexive principles can be validated. The psychological plausibility of the proposed approach to connexivity is further endorsed by experimental psychological studies: an overview on the empirical evidence will conclude my talk.

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LEVELS OF BEING: AN EGALITARIAN ONTOLOGY

Session 11E

Congress section(s): B4

This paper articulates and defends an egalitarian ontology of levels of being that solves a number of philosophical puzzles and suits the needs of the philosophy of science. I argue that neither wholes nor their parts are ontologically prior to one another. Neither higher-level properties nor lower-level properties are prior to one another. Neither is more fundamental; neither grounds the other.

Instead, whole objects are portions of reality considered in one of two ways. If they are considered with all of their structure at a given time, they are identical to their parts, and their higher-level properties are identical to their lower-level properties. For most purposes, we consider wholes in abstraction from most of their parts and most of their parts’ properties. When we do this, whole objects are subtractions of being from their parts—they are invariants under some part addition and subtraction.

The limits to what lower level changes are acceptable are established by the preservation of properties that individuate a given whole. When a change in parts preserves the properties that individuate a whole, the whole survives; when individuative properties are lost by a change in parts, the whole is destroyed. By the same token, higher-level properties are subtractions of being from lower-level properties—they are part of their realizers and are also invariant under some changes in their lower level realizers.

This account solves the puzzle of causal exclusion without making any property redundant. Higher-level properties produce effects, though not as many as their realizers. Lower-level properties also produce effects, though more than the properties they realize. For higher-level properties are parts of their realizers. There is no conflict and no redundancy between them causing the same effect.

As long as we focus on the right sorts of effects—effects for which higher-level properties are sufficient causes—to explain effects in terms of higher-level causes is more informative than in terms of lower level ones. For adding the lower-level details adds nonexplanatory information. In addition, tracking myriad lower level parts and their properties is often practically unfeasible. In many cases, we may not even know what the relevant parts are. That’s why special sciences are both necessary and useful: to find the sorts of abstractions that provide the best explanation of higher-level phenomena, whereas tracking the lower level details may be unfeasible, less informative, or both.

Given this egalitarian ontology, traditional reductionism fails because, for most scientific and everyday purposes, there is no identity between higher levels and lower levels. Traditional antireductionism also fails because higher levels are not wholly distinct from lower levels. Ontological hierarchy is rejected wholesale. Yet each scientific discipline and subdiscipline has a job to do—finding the explanations of phenomena at any given level—and no explanatory job is more important than any other because they are all getting at some objective aspect of reality.

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A CLASS OF LANGUAGES FOR PRAWITZ’S EPISTEMIC GROUNDING

Session 3L

Congress section(s): A1, A2, A4

With his theory of grounds (ToG), and by means of an innovative approach to the notion of (valid) inference, Prawitz aims to explain the epistemic compulsion exerted by proofs. ToG allows for some advancements with respect to Prawitz’s earlier proof-theoretic semantics (PTS). Also, it provides a framework for epistemic grounding.

The philosophical tenet is that evidence is to be explained in terms of a notion of ground inspired by BHK semantics. Grounds involve constructive functions, defined by equations reminding of procedures for full-evaluation of PTS valid arguments. Inferences are just applications of functions of this kind.

From a formal standpoint, Prawitz suggests instead that functions are to be described in kind of λ-languages. Because of the semantic role that these languages are to play, however, they must be conceived of as indefinitely open to the addition of new

functions. Deduction uses no fixed set of inferences and, more strongly, Gödel's incompleteness states that such a set cannot exist.
In our talk, we focus on the formal part of ToG, and sketch a class of languages and properties of such languages for first-order epistemic grounding.
First, we define a language of types, and the notion of atomic base over such types. Types are used to label terms in languages of grounding, whilst the atomic bases fix, through the atomic rules they involve, the "meaning" of the types.
Second, we single out a class of total constructive functions from and to grounds over a base. Also, we define composition of functions for grounds. Finally, we give clauses that determine the notion of ground over a base. These are the inhabitants of our "semantic universe", and basically involve generalized elimination rules up to order 2.
Third, we define a class of languages of grounding over an atomic base, as well as the notion of expansion of these languages. We then define denotation from languages of grounding to the universe of functions and grounds. We prove a denotation theorem, showing that each term in a language of grounding denotes a function or a ground.
We finally classify languages of grounding by pinpointing the following properties:
- closure under canonical form, i.e., each term is denotationally equal to one, in the same language, which is in canonical form – we show, in two different ways, depending on whether denotation is fixed or variant, that each language of grounding can be expanded to one closed under canonical form;
- primitiveness, depending on whether the language expands a given one by adding new non primitive or primitive operations;
- conservativity, depending on whether the language expands a given one by enlarging or not the class of denoted functions and grounds – we show that re-writability of function symbols implies conservativity, and that primitiveness and conservativity are not equivalent notions;
- universal denotation, i.e. a syntactic structure is admissible in a language of grounding over any base – we provide necessary and sufficient conditions for universal denotation, and its limits with respect to the "semantic universe".

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ABSTRACT SEMANTIC CONDITIONS AND THE INCOMPLETENESS OF INTUITIONISTIC PROPOSITIONAL LOGIC WITH RESPECT TO PROOF-THEORETIC SEMANTICS

Session 18M

Congress section(s): A2

In [1] it was shown that intuitionistic propositional logic is semantically incomplete for certain notions of proof-theoretic validity. This questioned a claim by Prawitz, who was the first to propose a proof-theoretic notion of validity, and claimed completeness for it [3, 4]. In this talk we put these and related results into a more general context [2]. We consider the calculus of intuitionistic propositional logic (IPC) and formulate five abstract semantic conditions for proof-theoretic validity which every proof-theoretic semantics is supposed to satisfy. We then consider several more specific conditions under which IPC turns out to be semantically incomplete.
In validity-based proof-theoretic semantics, one normally considers the validity of atomic formulas to be determined by an atomic system S. This atomic system corresponds to what in truth-theoretic semantics is a structure. Via semantical clauses for the connectives, an atomic base then inductively determines the validity of formulas with respect to S, called ‚S-validity' for short, as well as a consequence relation between sets of formulas and single formulas. We completely leave open the nature of S and just assume that a nonempty finite or infinite set of entities called ‚bases' is given, to which S belongs. We furthermore assume that for each base S in such a set a consequence relation is given. The relation of universal or logical consequence is, as usual, understood as transmitting S-validity from the antecedents to the consequent. We propose abstract semantic conditions which are so general that they cover most semantic approaches, even classical truth-theoretic semantics. We then show that if in addition certain more special conditions are assumed, IPC fails to be complete. Here a crucial role is played by the generalized disjunction principle. Several concrete notions of proof-theoretic validity are considered, and it is shown which of the conditions rendering IPC incomplete they meet.
From the point of view of proof-theoretic semantics, intuitionistic logic has always been considered the main alternative to classical logic. However, in view of the results to be discussed in this talk, intuitionistic logic does not capture basic ideas of proof-theoretic semantics. Given the fact that a semantics should be primary over a syntactic specification of a logic, we observe that intuitionistic logic falls short of what is valid according to proof-theoretic semantics. The incompleteness of intuitionistic logic with respect to such a semantics therefore raises the question of whether there is an intermediate logic between intuitionistic and classical logic which is complete with respect to it.
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KARL R. POPPER: LOGICAL WRITINGS

Session 15C

Congress section(s): B6

Karl Popper developed a theory of deductive logic in a series of papers in the late 1940s. In his approach, logic is a metalinguistic theory of deducibility relations that are based on certain purely structural rules. Logical constants are then characterized in terms of deducibility relations. Characterizations of this kind are also called inferential definitions by Popper. His works on logic anticipate several later developments and discussions in philosophical logic, and are thus interesting from both historical and systematic points of view [1]:
- Anticipating the discussion of connectives like Prior's "tonk", Popper considered a tonk-like connective called the "opponent" of a statement, which leads, if it is present in a logical system, to the triviality of that system.
- He suggested to develop a system of dual-intuitionistic logic, which was then first formulated and investigated by Kalman Joseph Cohen in 1953.
- He already discussed (non-)conservative language extensions. He recognized, for example, that the addition of classical negation to a system containing implication can change the set of deducible statements containing only implications, and he gave a definition of implication with the help of Peirce's rule that together with intuitionistic negation yields classical logic.
- He also considered the addition of classical negation to a language containing intuitionistic as well as dual-intuitionistic negation, whereby all three negations become synonymous. This is an example of a non-conservative extension where classical laws also hold for the weaker negations.
- Popper was probably the first to present a system that contains an intuitionistic negation as well as a dual-intuitionistic negation. By proving that in the system so obtained these two kinds of negation do not become synonymous, he gave the first formal account of a bi-intuitionistic logic.
- He provided an analysis of logicality, in which certain negations that are weaker than classical or intuitionistic negation turn out not to be logical constants.
A critical edition of Popper's logical writings is currently prepared by David Binder, Peter Schroeder-Heister and myself [2], which comprises Popper's published works on the subject as well as unpublished material and Popper's logic-related correspondence, together with our introductions and comments on his writings.

In this talk I will introduce this edition in order to provide an overview of Popper's logical writings, and I will highlight the central aspects of Popper's approach to logic.

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"BILANCIE GIUSTE A POSTA PER CHIARIRE QUESTA VERITÀ". THE IMPORTANCE OF INSTRUMENT IN GUIDOBALDO DAL MONTE'S LE MECHANICHE

Session 24G

Congress section(s): B6

It is conventionally used to identify the beginning of the modern science with the scientific activity of Galileo Galilei. Nevertheless, as is known thanks to copious studies about the Mathematics of the Renaissance, lots of intuitions of the Pisan 'scientist' were consequence of a lively scientific debate and a cultural milieu that marked the Sixteenth Century.
Among characteristics of modern science, surely the employ of the instrument to prove a theory was one of the most important. However the protagonists of Sixteenth Century had already gained a certain awareness about the useful of instrument to do science and as a good argument to defend their own thesis.
In this paper, I would like to show how into the controversy about the equilibrium conditions of a scale, a debate that involved the main mathematicians of the time, Guidobaldo dal Monte, the patron of Galileo, often used experiments and instruments to prove the indifferent equilibrium. This approach is really evident in Le mechaniche dell'illustriss. sig. Guido Ubaldo de' Marchesi del Monte: Tradotte in volgare dal sig. Filippo Pigafetta (1581), namely the Italian translation of Mechanicorum Liber (1577), the first printed text entirely dedicated to mechanics.

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ON THE EPISTEMOLOGY OF DATA SCIENCE – THE RISE OF A NEW INDUCTIVISM

Session 26A

Congress section(s): C6

Data science, here understood as the application of machine learning methods to large data sets, is an inductivist approach, which starts from the facts to infer predictions and general laws. This basic assessment is illustrated by a case study of successful scientific practice from the field of machine translation and also by a brief analysis of recent developments in statistics, in particular the shift from so-called data modeling to algorithmic modeling as described by the statistician Leo Breiman. The inductivist nature of data science is then explored by discussing a number of interrelated theses. First, data science leads to the increasing predictability of complex phenomena, especially to more reliable short-term predictions. This essentially follows from the improved ways of storing and processing data by means of modern information technology in combination with the inductive methodology provided by machine learning algorithms. Second, the nature of modeling changes from heavily theory-laden approaches with little data to simple models using a lot of data. This change in modeling can be observed in the mentioned shift from data to algorithmic models. The latter are in general not reducible to a relatively small number of theoretical assumptions and must therefore be developed or trained with a lot of data. Third, there are strong analogies between exploratory experimentation, as characterized by Friedrich Steinle and Richard Burian, and data science. Most importantly, a substantial theory-independence characterizes both scientific practices. They also share a common aim, namely to infer causal relationships by a method of variable variation as will be elaborated in more detail in the following theses. Fourth, causality is the central concept for understanding why data-intensive approaches can be scientifically relevant, in particular why they can establish reliable predictions or allow for effective interventions. This thesis states the complete opposite of the popular conception that with big data correlation replaces causation. In a nutshell, the argument for the fourth thesis is contained in Nancy Cartwright's point that causation is needed to ground the distinction between effective strategies and ineffective ones. Because data science aims at effectively manipulating or reliably predicting phenomena, correlations are not sufficient but rather causal connections must be established. Sixth, the conceptual core of causality in data science consists in difference-making rather than constant conjunction. In other words, variations of circumstances are much more important than mere regularities of events. This is corroborated by an analysis of a wide range of machine learning algorithms, from random trees or forests to deep neural networks. Seventh, the fundamental epistemological problem of data science as defined above is the justification of inductivism. This is remarkable, since inductivism is by many considered a failed methodology. However, the epistemological argument against inductivism is in stark contrast to the various success stories of the inductivist practice of data science, so a reevaluation of inductivism may be needed in view of data science.

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CONCEPTS AND REPLACEMENT: WHAT SHOULD THE CARNAPIAN MODEL OF CONCEPTUAL RE-ENGINEERING BE?

Session 18C

Congress section(s): B1

Many concepts, it seems, are deficient. One response to conceptual deficiency is to, in some sense, refine the problematic concept. This is called ‚conceptual re-engineering'.
We can think of Carnapian explication as a model of conceptual re-engineering. On this approach, conceptual re-engineering consists of the replacement of one concept, the ‚explicandum', with another concept, the ‚explicatum'. One advantage of the approach is that Carnapian explication promises us something approaching a step-by-step guide for conceptual re-engineering.
For such a model to be helpful, however, we need an account of 'conceptual replacement'. For modelling purposes, then: How should we understand 'concepts'? And what should we understand to be involved in their 'replacement'?

I will consider and reject two answers, before recommending an alternative.
1. The naïve view
Concepts are word meanings, and have a definitional structure (or are constituted by rules). Replacement involves changing the meaning of a word from the explicandum to the explicatum, or using a word whose meaning is the explicatum in place of a word whose meaning is the explicandum.
Initial objection. This theory of concepts is problematic. (E.g. Williamsonian worries about analyticity.)
Principal objection. Conceptual re-engineering is a theory-neutral methodology, and so we want a model that is as theory-neutral as possible. Given the initial objection, the naïve view fails to meet this desideratum.
2. A Cappelen-inspired view
Talk of concepts is problematic. Think instead in terms of intensions and extensions. In particular: replacement involves changing a word's intension; the explicandum is the old intension; the explicatum is the new intension.
Advantage 1. Intensions/extensions theoretically much less weighty and controversial than many theories of 'concepts'.
Advantage 2. That being said, intensions are perhaps a plausible model of concepts anyway.
Objection. Changing a word's intension/extension is hard – it doesn't appear to be achievable by simply stipulating a new definition/rules of use for a term. So this view doesn't seem a good model of conceptual re-engineering.
3. The speaker-meaning view
Distinguish between speaker-meaning and semantic-meaning. Speaker-meaning is closely tied to intentions – very roughly, one speaker-means that which one intends to convey. Semantic-meaning is tied to linguistic conventions. Model both speaker-meaning and semantic-meaning using intensions/extensions. Then (for w1, w2 not necessarily distinct):
The explicandum is the semantic-intension of w1. The explicatum is a speaker-intension of w2. Replacement consists of using w2 to speaker-mean the explicatum, instead of using w1 to speaker-mean the explicandum.
Advantages 1 and 2 as above.
Advantage 3. We are in control over speaker-meaning (because we are in control over our intentions).
Advantage 4. Makes sense of why conceptual re-engineers typically specify explicata by definitions: they are displaying their communicative intentions. (Challenge: what about rules of use?)
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THE PROBLEM OF SCIENTIFIC-EPISTEMOLOGICAL RACISM AND THE CONTRIBUTIONS OF SOUTHERN GLOBAL EPISTEMOLOGIES IN THE CONSTRUCTION OF PARADIGMATIC TRANSFORMATIONS OF THE PHILOSOPHY OF SCIENCE

Session 7G

Congress section(s): B2, B5, B6

This paper addresses the issue of scientific racism and the impacts of epistemological boundaries in the construction of research. The objective is to understand the relevance of the epistemologies of the Global South to the paradigmatic transformations in the positivist models of the scientific method that was in force until the second half of the 20th century. This role is motivated by the fact that there are demands that force the field of scientific production to revise certain concepts in favor of solutions to the current contradictions. In a socio-political and economic scenario that divides the world into epistemological boundaries between the global North and South that contribute significantly to the maintenance of scientific segregations that have repercussed until nowadays in the methodologies used in the process of construction of scientific research. The methodology is bibliographical and seeks to problematize the distance between the representation and the objective reality of the fact researched, from this point we understand scientific racism as a product of this distance. The paper seeks contributions in the thinking of Boaventura de Souza Santos, Maria Paula Meneses to understand the scenario of the new demands that contemporary society is bringing and the challenges that science assumes in these issues. In addition to these ideas, the contribution of Fritjof Capra and Walter Mignolo's thinking is sought to understand the changes that are occurring in the processes of scientific knowledge construction. The results elucidate that contemporary society faces serious challenges for the formation of a sustainable future that is indispensable for the survival of the human species. Faced with the challenges of a complex society, where factors and phenomena are interconnected, new demands for the survival of the human being emerge and the old Cartesian and positivist models do not sustain themselves and fail to give effective answers to this question. Based on these questions, the research seeks alternatives in the knowledge of the peoples of the Global South who have different understandings about the relationship between the human being and nature that can contribute significantly to cross the epistemological and scientific frontiers in favor of the construction of a science that has an active role in shaping a sustainable society. As it deals with the theme of the influence of social transformations and their impacts on the philosophy of science, it deals with a theme relevant to the historical aspects of the philosophy of science in the contemporary world.
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TABLEAUX PROCEDURES FOR LOGICS OF CONSEQUENTIAL IMPLICATION

Session 27F

Congress section(s): A2

Logics of Consequential Implication belong to the family of connexive logics, but Consequential Implication (CI) differ from standard connexive implication for some remarkable properties :
1) The Factor Law A CI B --> (A & C CI B & C), which is commonly accepted in connexive logic, does not hold or holds in weakened form
2) Boethius' Thesis belongs to the set of valid formulas in the weak form
(A CI B) --> ¬(A CI ¬ B) but not in the strong form (A CI B) CI ¬(A CI ¬ B)
3) A clear distinction is drawn between analytic and synthetic (i.e. context-depending) conditionals, which is normally neglected in connexive logic
4) The operators for analytical consequential implication are intertranslatable with operators of monadic modal logic.
The point 4) makes it clear that, if a system X of analitical consequential implication is one-one translatable into a normal modal system X*, X is decidable iff X* is decidable. If X* is tableaux - decidable, X is also tableaux decidable thanks to the mentioned translation. A noteworthy fact is that monadic modal operators may be translatable into two or more operators of consequential implication. The two main definitions which may be provided are the following:
1) A --> B =df []($A \to B$) & ([]B -> []A) & (<>B -> <>A)
2) A => B = df []($A \to B$) & (<>B -> <>A)

Consequently, the decision procedure for → -->-systems and => - systems are divergent in the reduction of the wffs to modal language, even if they may be coincident for the tableaux technique. The more interesting problem concerns the tableaux procedure for systems of syntethic consequential implication (a special case of it being counterfactual implication). Such implication may be defined in terms of th e analytic one in various different ways, e.g.:

A > B =df *A => B

A >> B =df *A => B & ([]B ->[]A)

The operator * for “ceteris paribus” is submitted to independent axioms and is in turn translatable into a simpler operator in this way:

*A =df w(A) & A ,

where w is a modal operator submitted to the simple axiom

(w<>) <>A -> <>(w(A)& A)

The paper outlines the axiomatization of systems for analytic and synthetic CI-implication and shows how to define a tableaux procedure suitable to wffs containing > and >>.

The final result shows how the tableaux procedure for the axiomatic system of CI-implication can be converted into a completeness proof for it.

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CONCEPTUAL ENGINEERING AND SEMANTIC CONTROL

Session 17C

Congress section(s): B1

Cappelen (2018) proposes a radically externalist framework (the ‘Austerity Framework’) for conceptual engineering. This approach embraces the following two theses (amongst others). Firstly, the mechanisms that underlie conceptual engineering are inscrutable: they are too complex, unstable and non-systematic for us to grasp. Secondly, the process of conceptual engineering is largely beyond our control. One might think that these two commitments – ‘Inscrutability’ and ‘Lack of Control’ – are peculiar to the Austerity Framework or, at least, to externalist approaches more generally. And, indeed, this sort of thought has been suggested in the literature. Burgess and Plunkett (2013), for example, claim that internalists are better able to accommodate the idea that we have the semantic control required for deliberate engineering of concepts and expressions. However, Cappelen argues that neither Inscrutability nor Lack of Control are unique to his approach. Rather, they must be accepted by externalist and internalist views of meaning and content alike.

Cappelen argues as follows. Internalists claim that meaning supervenes on the internal properties of the individual. But this does not give us a direct route to semantic control or scrutability. The fact that the contents of an individual’s thoughts, or the meanings of her expressions, supervene on her internal properties does not entail that they supervene in any stable, systematic, or surveyable way. Cappelen argues that, for the internalist to avoid commitment to Inscrutability and Lack of Control, she must provide arguments for 3 claims: (a) There are inner states that are scrutable and within our control; (b) concepts supervene on these inner states; and (c) the determination relation from supervenience base to content is itself scrutable and within our control.

In this talk, I will consider how internalist metasemantic views might meet Cappelen’s 3 challenges. With regard to (a), I will argue that it is plausible that we have a weak sort of control over some of our inner states, some of the time. E.g., We have mental control as contrasted with mind-wandering (Metzinger 2013). With regard to (b), I will argue that it is reasonable to treat concepts as supervening on these states, as the resultant view is largely in keeping with widely accepted desiderata on a theory of concepts. With regard to (c), I will argue that we should appeal, not to mere supervenience, but to alternative relations such as identity or realization in order secure the result that the relation from determination base to content is both scrutable and within our control. For example, concepts might be identical with locations in a semantic network, or they may be the realizers of conceptual roles. Thus, internalists may offer the resources needed to understand conceptual engineering as an actionable method for improving concepts.

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ABSTRACTION IN SCIENTIFIC MODELING

Session 15I

Congress section(s): B1

Abstraction is ubiquitous in scientific model construction. It is generally understood to be synonymous to omission of features of target systems, which means that something is left out from a description and something else is retained. Such an operation could be interpreted so as to involve the act of subtracting something and keeping what is left, but it could also be interpreted so as to involve the act of extracting something and discarding the remainder. The first interpretation entails that modelers act as if they possess a list containing all the features of a particular physical system and begin to subtract in the sense of scratching off items from the list. Let us call this the omission-as-subtraction view. According to the second interpretation, a particular set of features of a physical system is chosen and conceptually removed from the totality of features the actual physical system may have. Let us call the latter the omission-as-extraction view.

If abstraction consists in the cognitive act of omission-as-subtraction this would entail that scientists know what has been subtracted from the model description and thus would know what should be added back into the model in order to turn it into a more realistic description of its target. This idea, most of the time, conflicts with actual scientific modeling, where a significant amount of labor and inventiveness is put into discovering what should be added back into a model. In other words, the practice of science provides evidence that scientists, more often than not, operate without any such knowledge. One, thus, is justified in questioning whether scientists actually know what they are subtracting in the first case. Since it is hard to visualize how modelers can abstract, in the sense of omission-as-subtraction, without knowing what they are subtracting, one is justified in questioning whether a process of omission-as-subtraction is at work.

In this paper we particularly focus on theory-driven models and phenomenological models in order to show that for different modeling practices what is involved in the model-building process is the act of extracting certain features of physical systems, conceptually isolating and focusing on them. This is the sense of omission-as-extraction, that we argue is more suitable for understanding how scientific model-building takes place before the scientist moves on to the question of how to make the required adjustments to the model in order to meet the representational goals of the task at hand. Furthermore, we show that abstraction-as-extraction can be understood as a form of selective attention and as such could be distinguished from idealization.

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THE AXIOMATIC APPROACH TO GENIDENTITY ACCORDING TO Z. AUGUSTYNEK

Session 20K

Congress section(s): A2

The identity of objects through time can be described using the notion of genetic identity (genidentity). This term was introduced to the language of science by Kurt Lewin (1890-1949) in 1922. Polish philosopher Zdzisław Augustynek (1925-2001) devoted a number of his works to this issue. He tried to specify the notion of genidentity using axiomatic definitions. He presented three sets of specific axioms. They delimit sets of theses, which are called systems by him. These systems are marked as AS1, AS2, and AS3. In particular axioms, besides the term genidentity (G), also the following terms are used: logical identity (I), quasi-simultaneity (R), quasi-collocation (L), and causality (H), as well as the complements of these relations: genetic difference (G‘), logical difference (I‘), time separation (R‘), space separation (L‘), and the complement of

relations H (H‘). They represent binary relations whose field is the set of events S. Augustynek analyzed some consequences of these axioms that are important, in his opinion, from the philosophical point of view. His results can be supplemented or even corrected in some places. This fact motivated us to analyze systems AS1, AS2, and AS3 once again. The first aim of this paper is to present the set-theoretic approach to the analysis of Augustynek’s systems. This approach, in our opinion, facilitates the analysis of the sets of specific axioms. The next step is formulating and justifying theses concerning, first, the relationships among systems AS1, AS2, and AS3, and second, supplementary axioms that cause a mutual equivalence of the axioms when added to systems AS1, AS2, and AS3. In this way we will correct some conclusions concerning systems AS1, AS2, and AS3 from Augustynek’s works. We will show a method of creating alternative axioms for systems AS1, AS2, and AS3, and suggest methods for further modifications of axioms of these systems. Our last aim is to analyze the problem of reducing the above-mentioned axiomatic definitions to conditional definitions containing the necessary condition and the sufficient condition of a selected notion from Augustynek’s systems. We are going to prove that these axiomatics can be reduced to certain conditional definitions of genidentity.

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WHAT IS AN HYPOTHESIS?

Session 16H

Congress section(s): B6

I document usage and meaning of the phrase ‚hypothesis‘ with original quotes from the earliest origins to contemporary sources. My study is based on some 100 authors from all fields of knowledge and academic cultures, including first hand reflections by scientists, from which I shall present a selection. I focus on explicit methodological statements by these authors and do not investigate concrete hypotheses.
The interpretations of the term hypothesis developed over time. Conflict and disagreement often was the result from not speaking a common language. Philologically no single definition captures all its meaning, and usage in fact often is contradictory. The purposes of this exercise are several: (1) To give meaning to expressions such as more-than-hypothesis, or pejoratives like mere-hypothesis. (2) To provide a lexical overview of the term. (3) To elaborate on the different kinds of epistemes. (4) To classify hypotheses (phenomenological postulates, models, instruments, and imaginary cases). (5) To trace the origins of evidence-based hypothetico-deductive epistemology (Bellarmine - Du Châtelet - Whewell - Peirce - Einstein – Popper). (6) To demarcate the term from several related ones (theory, thesis, principle, fact). Notwithstanding personal preferences, “hypothesis” shall remain a term with multiple even mutually exclusive connotations, what counts is giving exemplars of use (Kuhn!).
For purposes of illustration let me quote from a table of my finished manuscript with the principal interpretations of the term hypothetical: not demonstrated, unproven but credible, capable of proof if asked for one, presumption; an element of a theory, système, a subordinate thesis, proposal, assumption, paradigm, presupposition; a kind of syllogism (if – then), conditional certitude; a statement expressing diverse degrees of probability (morally certain, probable, fallible, falsifiable, reformable, tentative, provisional, unconsolidated, subjective, speculative, fictitious, imaginary, illegitimate assumption, unspecified); pejorative uses, diminutive; ex suppositione – to safe the phenomena (instrumentalism), mathematical contrivances, ex hypothesi – why?, a model, mutual base of a discourse, reconciles reason with experience; suppositio – postulate, rule, prediction, evidence-based hypothetico-deductive, that what is abducted, guess; a third category besides ideas and reality, a blueprint for change, free inventions of the mind. Hypothesis, supposition and conjecture are roughly synonyms.
A full length manuscript on the subject of my conference presentation is available for inspection by those interested.

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THE PROBLEM OF THE VARIABLE IN QUINE’S LINGUA FRANCA OF THE SCIENCES

Session 10H

Congress section(s): A2, B1, B2, B4, C1

This paper discusses the problematic use of the variable as sole denoting position in Quine’s canonical notation, specifically of singular reference. The notation is the formal theory Quine developed which was intended to be a perspicuous lingua franca for the sciences. The paper shows how singular objects that are necessarily determined in the notation are nonetheless necessarily denoted indeterminately, creating a formal paradox.
Quine’s notation is an adaptation of the formal logic of Russell’s theory of denotation. The paper begins by explaining how Quine’s wider ontological views led him to make reductions to Russell’s original theory in two ways. Firstly he held the sparse ontological view that being is strictly existence. Consequently, that only objects that exist at a particular point in space-time should be expressible within the lingua franca i.e., what Quine considered to be the empirical objects of the sciences. He thereby excluded universal objects from his lingua franca.
Secondly, due to the specific form of empirical linguistic relativism that Quine held, he only allowed objects in his lingua franca to be determined through categorisation into sets. This Quine achieved formally by solely allowing objects to be determined through predication, not reference. To do so, Quine disallowed any form of constant in reference-position. The only way to denote an object in Quine’s lingua franca therefore is through a variable quantified over existentially.1 As Quine famously put it, “to be, is purely and simply, to be the value of a variable.”2
The paper proceeds to explain the problem with said reductions. Briefly put, in the first-order propositional logic of a formal theory of reference such as that which Quine adopts, the variable occupies referential-position to represent indeterminate reference. In the case of singular reference, the variable indeterminately denotes one value, i.e., one object. But the variable is preliminary to determination. That is, once the necessary and sufficient predication has been given, the denoted object has been fully determined. At this point the variable is necessarily replaceable by a constant, as what is denoted is no longer indeterminate. But in Quine’s lingua franca, the variable can never be replaced by a constant. It would seem that in all such cases of singular denotation, the variable, per its very use in formal logic, indeterminately denotes, yet at the same time the object denoted is necessarily determined.
Formally speaking, such a paradox is damaging to the very purpose of the lingua franca: to express objects clearly and consistently across the sciences. Additionally it becomes difficult to reconcile form with content. That is, singular, determinate objects that exist at a precise point in space-time are exclusively expressed by means of a formal paradox. The paper concludes that, due to Quine’s ontological commitments, objects as expressed in his lingua franca cannot successfully be reduced purely and simply to the value of a variable without paradoxical results.
1. See W.V.O Quine, Word and Object, 179.
2. W.V.O Quine, “On What There Is”, (Philosophy Education Society Inc. The Review of Metaphysics: Vol. 2, No. 5: Sep., 1948:31).

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ARE IN-DEPTH INTERVIEWS A MUST FOR ETHNOGRAPHY OF HEP LABS?

Session 6H

Congress section(s): B3, C2

We discuss preliminary results of an empirical, ethnographic study of an international high-energy physics (HEP) laboratory, the Joint Institute for Nuclear Research in Dubna, Russia, conducted in summer 2018. The overall aim of the project was a comparative study of answers explicitly given by the interviewees in response to survey questions and answers implied to same questions the in-depth interviews. We deployed two contrasting research methods: structured surveys, using the set of OPTIMIST group questions [1], and in-depth interviews, drawing on a broad questionnaire. In most cases, the in-depth interviews were executed subsequent upon and complementary to survey questions, while in a few scenarios, interviews with informants were organized independently as well. In the survey stage, interviewees were asked questions regarding their freedoms and opportunities at work, for example, how they used their judgment to problem solving. The interviewees were requested to rank their answers on the Likert scale (from 1 to 7).
In the second stage of interviews, scientists were asked general questions regarding their role and professional identity in the project, the structure of their working day, the hierarchies in the group and division of labor. Depending on their answers, follow-up questions were asked regarding particularities of their duties and attitudes in relation to others in the project. Each interview was conducted by a philosophy student to avoid biases stemming from the interviewers' professional identity. Scientific staff and visitors to JINR from Russia, Belarus, Poland, Romania, Bulgaria, Cuba, and Chile have been surveyed and interviewed, with 15 interviews recorded and analyzed in all.
We found in our analysis that the two strategies, when used separately, lead to contradictory results since informants usually do not reflect upon survey questions deeply. In addition, they face considerable difficulty in responding to "philosophical" questions in general. An example can help here. On the survey question pertaining to the amount of freedom they can exercise, most participants chose the highest grade. However, their implied grade for the same question elicited from the in-depth interviews, in which some of the scientists admitted that there were budgetary constraints, limited freedom in the choice and use of equipment as well as that approaches to scientific work were in general conventional and repetitive, was much lower.
We propose to discuss these findings in a comparative perspective, and suggest that in an ethnographic study of a laboratory, mixed-methods which combine quantitative and qualitative approach are more efficient, i.e. "philosophical" questions have to be posed and elicited by experts in the course of face-to-face interviews with scientists, rather than obtaining their views in written surveys.
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IS SEMANTIC STRUCTURALISM NECESSARILY "SET-THEORETICAL" STRUCTURALISM? A CASE OF ONTIC STRUCTURAL REALISM

Session 20G

Congress section(s): B2

Standard approaches to scientific structuralism combine issues relevant to debates in metaphysics, epistemology, philosophy of science, and scientific methodology. Nowadays, one of the most frequently discussed and developed is the ontic structural realism, whose main thesis states that only the structure exists (French & Ladyman 2003). The emergence and development of various forms of structuralism eventually gave rise to a discussion on various methods of representing relevant structures that we should be realists about: from typically syntactic ones, such as Ramsey's sentence, to set- (or group-) theoretical and categorical semantic interpretations. The most popular version of the semantic approach to structural realism, attempting to define the concept of a „shared structure" between models in terms of relevant functions determined between the analyzed structures, is undoubtedly one that assumes a structure of sets as its formal framework (Suppes 1967, French 2000). The main advantage to such an approach is certainly the fact that it was thought to provide a unified formal framework for formulating explanations on such issues as the structure of scientific theories, the problem of applicability of mathematics in natural sciences, and the philosophical account of Structural Realist's commitment to the structure shared by successive theories. However, as it soon has been made clear by the critics, even if such framework can be quite useful in, for example, analyzing and comparing the structures of scientific theories, it is also very problematic when it comes to extracting the Structural Realist's ontological commitments, since we inevitably run into theoretical troubles while trying to talk about the set-structure made of relations without objects.
In recent years, voices saying that the set-theoretical approach - with all its advantages and drawbacks - does not have to be the „only right way" were raised. Mathematician Elaine Landry (2007) has joined the discussion noting that, from a mathematical point of view, there is no rational (devoid of dogmatism) reason to take for granted the claim about the fundamental character of set theory. What's more, in cooperation with Jean-Pierre Marquis (2005), she formulates an alternative vision of scientific structuralism, whose formal framework is provided not by - highly popular in this context - set theory, but by means of category theory. Reflections around alternative methods of representing relevant structures within scientific structuralism eventually lead Landry to conclude that the proposed categorical framework, even if historically and genealogically rooted in set theory, to achieve an adequate level of accuracy in the analysis of the notion of „model" or „shared structure", does not depend in any way on the previous support or embedding in set theory.
The aim of this talk is a comparative analysis of the philosophical foundations for applications of different structural frameworks discussed in the context of the ontic version of structural realism, with particular emphasis on set theory and category theory. These considerations will then be confronted with a hypothesis stating that we should make use of some kind of trade-off between the comparative and relevant powers of different representational methods, proposing a pluralistic (in opposition to unificatory) view on the role of structural representation within OSR.
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SOME SIXTY OR MORE PRIMORDIAL MATTERS: CHEMICAL ONTOLOGY AND THE PERIODICITY OF THE CHEMICAL ELEMENTS

Session 6A

Congress section(s): B1

Accounts on the periodic system often draw attention to how two of its main discoverers had contrasting views on the nature of chemical elements. Where Julius Lothar Meyer saw it likely that the elements were comprised of the same primordial matter (Meyer, 1870, 358; 1888, 133), Dmitrii Ivanovich Mendeleev opposed to this view. Instead, Mendeleev argued that each element was its distinct, individual, autonomous entity, and he discouraged from making representations of periodicity that suggested otherwise (1905, 22-24).

Following Andrea Woody's rich article on the law of periodicity as a theoretical practice (2014), this paper explores how Meyer's and Mendeleev's ontological views on primordial matter shaped their ideas on how to represent periodicity. I start by showing how Meyer's views on the nature of the elements were not an endorsement of the truth of the hypothesis on the primordial matter. Instead, for Meyer, taking the view on board was needed for conducting further investigations on the relationship between atomic weight and other properties of elements. With respect to Mendeleev, I show how his metaphysical views on nature of elements influenced his evaluation of other investigators' representations of periodicity. I argue that especially Mendeleev's rejection of graphs (Bensuade-Vincent, 2001) and equations for representing periodicity is in part explained by his views on the nature of the elements. From the many attempts of rendering periodicity to more mathematical language, I especially focus on the equations created by the Russian political philosopher and lawyer Boris N. Chicherin. After doing so, I show that Mendeleev's ontological views influenced his rejection of Chicherin's equations.

The examples of Meyer and Mendeleev show that their ontological commitments directed both their own representations of periodicity and their evaluations of other investigators' representations. Even though we are warned not to confuse means of representation with what is being represented (French, 2010), the case of Meyer and Mendeleev suggests that ontological views on the nature of elements influenced representing periodicity.

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VALUES IN SCIENCE AND EARLY PERIODIC TABLES

Session 27G

Congress section(s): B6, C2

As a philosopher, I am especially interested in the role of values in science. By values, I broadly mean features that people find good, useful, or desirable. Although values are more familiar to us from political and moral discussions, I suggest that values are also very much present in science, too. For example, scientists might find something valuable in the design of some experiment; in the goals of research; or they might value some features of theories so much that they act as reason for choosing between competing theories. For someone interested in values, the priority dispute on the discovery of the periodic system gives especially fertile working grounds. If we compare the tables of J.A.R. Newlands, Julius Lothar Meyer, and Dmitrii Mendeleev, they appear simultaneously similar and different. In my research, I argue that at least some of these differences are explained by chemists emphasising different values during the development of their periodic systems. This raises a number of questions: did the values influence the differing reception of the systems? Did they guide the further uses of the systems? In short, investigating the periodic systems through the lens of values allows us a telling snapshot of the many ways in which values may guide science.

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REDUCING VAGUENESS IN LINGUISTIC EXPRESSION

Session 11D

Congress section(s): B7

Students in professional programs or graduate research programs tend to use an excess of vague pronouns in their writing. Reducing vagueness in writing could improve written communication skills, which is a goal of many professional programs. Moreover, instructor effectiveness in reducing vagueness in students' writing could improve teaching for learning. Bertrand Russell (1923) argued that all knowledge is vague. This research provides evidence that vagueness in writing is mitigated with instructor feedback.

An empirical study tested the hypothesis that providing feedback on vague pronouns would increase clarity in students' writing over an academic semester. Vague terms such as "this", "it", "there", "those", "what", and "these" followed by a verb were highlighted, and a written comment drew students' attention to vague terms: "Rewrite all sentences with highlighted vague terms throughout your paper for greater clarity in professional writing."

Writing with "what", "it", and other vague pronouns allows students to apply course concepts or describe contexts without understanding either. A collaboration between instructor and student could improve clarity of information communicated by helping students explain their understanding of ideas or principles taught (Faust & Puncochar, 2016).

Eighty-six pre-service teachers and 36 education master's candidates participated in this research. All participants wrote at a proficient level, as determined by passing scores on a Professional Readiness Examination Test in writing. The instructor and a trained assistant highlighted and counted vague pronouns over six drafts of each participant's document. Inter-rater reliability using Cohen's kappa was 0.923 ($p < .001$). Frequency of vague pronouns decreased noticeably with each successive draft. A repeated measures ANOVA on use of vague pronouns in a final free-write essay compared to use of vague pronouns in an initial free-write essay achieved a statistic of $F(1,40) = 3.963$ ($p = 0.055$).

Ninety percent of participants identified an increased awareness of the importance of eliminating vague pronouns to improve writing clarity on end-of-semester self-evaluations. As an example, "While I write now, I find myself using a vague term, but I stop and ask myself, "How can I eliminate this vague term to make my paper sound better?" This type of self-reflection I have never done before and I see a big improvement in the tone of my writing."

This research provided information on effects of instructor feedback to reduce vague pronouns and thereby improve clarity of students' writing. As Russell (1923) said, "I shall be as little vague as I know how to be ..." (p. 84).

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ALGEBRAIC SEMANTICS FOR INQUISITIVE LOGICS

Session 29D

Congress section(s): A2

Inquisitive semantics is a currently intensively developing approach to the logical analysis of questions (see Ciardelli, Groenendijk, Roelofsen 2018). The basic idea of inquisitive semantics is that, to be able to provide a semantical analysis of questions, one has to go beyond the standard approach that identifies sentential meaning with truth conditions. This approach is applicable only to declarative sentences but not to questions which have no truth conditions. The concept of "truth in a world" is replaced by a concept of "support in an information state". Support is suitable for a uniform logical analyses of both statements a questions.
Inquisitive semantics, as we just described it, is a relational semantics – it is based on a relation of support between information states and sentences. Although this semantics has been explored thoroughly in the last decade, so far not much attention has been payed to the algebraic aspects of inquisitive logic (with the exceptions of Frittella et al. 2016 and Roelofsen 2013). But it is evident that the models of inquisitive semantics generate interesting algebras of propositions and it would be desirable to understand better the nature of these algebraic structures.
This paper is intended as a contribution to the algebraic study of inquisitive propositions. We will not focus solely on the algebras related to the basic inquisitive logic, i.e. the logic generated by standard inquisitive semantics. Our perspective will be more general and we will define a class of algebras that are suitable for a broad (in fact, uncountably large) class of propositional superintuitionistic inquisitive logics that were introduced in (Author, 2016). (Standard inquisitive logic is the strongest logic in this class having similar role as classical logic has among superintuitionistic logics.) We will call such algebras "inquisitive Heyting algebras". We will explain how questions are represented in these structures (prime elements represent declarative propositions, non-prime elements represent questions, join is a question-forming operation) and provide several alternative characterizations of inquisitive algebras. For example: A Heyting algebra is inquisitive iff it is isomorphic to the algebra of finite antichains of a bounded implicative meet semi-lattice iff it is join-generated by its prime elements and the set of prime elements is closed under meet and relative pseudo-complement iff its prime filters and filters generated by prime elements coincide and prime elements are closed under relative pseudo-complement. We will also explain how exactly are inquisitive algebras related to inquisitive logics.
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SOLIDARITY AND REGULATORY FRAMEWORKS IN (MEDICAL) BIG DATA

Session 30D

Congress section(s): B5, C4

The use we make of digital technologies produces a huge amount of data, known as big data, whose management is becoming increasingly more difficult. One of the problems in this regard is the possibility of data controllers abusing their situation of power and using the available information against the data subject. This abuse can have several faces. Prainsack (2015) identifies at least three types of abuse: hypercollection, harm, and humiliation. Hypercollection means that just because institutions can collect information about customers or citizens for purposes other than the ones for which it was collected in the first place, they do so. Harm occurs when the information obtained is used against the interests or rights of the data subject. This damage is accompanied by humiliating effects when making people partake in their own surveillance. In the face of this new reality, the question of how to govern data use has become more important than ever. The traditional way of governing the use of data is through data protection. Recently, the European Union has published a new General Data Protection Regulation (GDPR) that follows this approach. However, authors such as Prainsack and Buyx (2016) rightly point out that the strictly regulation approach is insufficient for dealing with all abuses related to the use of big data. On the one hand, excessive control can curb the opportunities and benefits of digital technologies for users and society as a whole. And on the other, control and regulation may be insufficient in controlling all risks associated with the use of big data.
In opposition to the strict regulation approach, Prainsack and Buyx propose a new one, based on solidarity. The solidarity approach entails the acceptance of the impossibility of eliminating the risks of modern data usage. The authors base their proposal on a solidarity formula whose objective is to compensate those affected by possible abuses: harm mitigation funds. Such funds would help to ensure that people who accept those risks and are harmed as a result have appropriate support.
The paper does not question the adequacy of harm mitigation funds, but rather the conception of solidarity that Prainsack and Buyx choose to justify them. I would argue that this conception of solidarity, based on psychology and moral sociology, has less normative force than exists in the strict regulation approach, which is based on the defence of fundamental rights. If we want the policy of harm mitigation funds to have a normative force similar to that of the strict regulation approach, then we must choose a conception of solidarity based on respect for fundamental rights.
In the paper, I first present the context in which it makes sense to oppose a solidarity-based perspective to a strictly regulation one. Then I review what I believe are the weak points in Prainsack and Buyx's ideas regarding solidarity. And finally, I introduce an alternative conception of solidarity that normatively better justifies any public solidarity policy addressing the risks of big data, including harm mitigation funds."

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INTERGENERATIONAL JUSTICE ISSUES IN GERMLINE GENOME EDITING

Session 8I

Congress section(s): C4

The birth of the twins whose embryo was subject to genome editing has shocked the participants at the Second International Summit Meeting on Genome Editing as well as the world. The Chinese scientist Dr. Jiankuai He was blamed for violating Chinese relevant regulations, international well accepted ethical norms as well as for irresponsible intervention which will bring about great health harms to the twins and their offspring, or future generations due to his invalid research design including they may be at greater than average risk of dying of some other infections, including flu by disabling their CCR5 gene. This constitutes an obvious example in which an action taken by present generation may directly cause harms to future generations. Some philosophers deny that the possibility of future people having rights vis-à-vis us based on the fact that future people will live in the future, our epistemic situation does not allow us to relate to future people as identifiable individuals, and they cannot claim these rights against them, i.e. cannot impose sanctions on currently living people for non-fulfillment of their corresponding duties. The article will argue that when we edit the embryo's gnome of the parent with genetic condition the future people are definitely be bearers of rights, that the rights they have will be determined by the interests (health, wellbeing) they have then, and that our present actions and policies can affect their interests. If we can so severely frustrate such interests of future people, and commit to violate the intergenerational justice, we can violate their future rights. The twins, Lulu and Nana are genetically identifiable people living now, but their offspring will be non-identifiable future people who may be at greater than average risk of dying of some other infections as the twins, our obligations to these non-identifiable future people are no less than to the twins because they are human beings too that require us to relate morally to them as fellow humans. Based on this ethical inquiry we suggest that the provision on safeguarding the rights of future people and adhering intergenerational justice should be included in the regulations on human genome editing.

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SITUATED COUNTING

Session 28L

Congress section(s): C5, C6

We present a model of how counting is learned based on three knowledge components: number as a property of collections; ordered sequence of numerals; and one-to-one mappings between collections. In the literature, the so-called cardinality principle has been in focus when studying the development of counting. We argue that identifying the ability to count with the ability to perform by fulfilling the cardinality principle is not sufficient, and that counting should be analyzed as the ability to perform a series of tasks. The tasks require knowledge of the three components. Some of these tasks may be supported by the external organization of the counting situation. Using the methods of situated cognition, we analyze how the balance between external and internal representations will imply different loads on the working memory and attention of the counting individual. This analysis will show that even if the counter can competently use the cardinality principle, counting will be more or less vary in difficulty depending on which kind of collection is to be counted and on its physical properties. The upshot is that a number of situated factors will influence the counting performance. This will determine the difficulty of the different tasks of counting.

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TYPE THEORY, REDUCIBILITY AND EPISTEMIC PARADOXES

Session 8M

Congress section(s): A2

Abstract. Variants of *simple type theory* (STT) (equipped with typed λ-calculus as language), pioneered by Church 1940, are among leading logical systems employed within computer science (functional programming, proof assistants, ...; dependent or intuitionistic STTs are often used), and, being systems of higher-order logic, they are also employed for post-Montaguean formalisation of language (type-theoretic, and even proof-theoretic semantics). Yet Church (e.g. 1976) repeatedly investigated a variant of Russell's (1908, 1910–13) *ramified TT* (RTT), which can be imagined as STT whose some types are split into proper *types* (i.e. sets of entities) of different *order*. Recent RTT's include Tichý 1988, Kamareddine, Laan and Nederpelt 2010 (computer scientists). Church describes a motivation for such an enterprise:

> If, following early Russell, we hold that the object of assertion or belief is a proposition and then impose on propositions the strong conditions of identity which it requires, while at the same time undertaking to formulate a logic that will suffice for classical mathematics, we therefore find no alternative except for ramified type theory with axioms of reducibility. (Church 1984, p. 521)

One assumes here a proof-theoretic approach to belief attitudes (whose objects are 'structured propositions', not possible-world propositions), not the model-theoretic one (for which see e.g. the handbook of epistemic logic by van Ditmarsch, Halpern, van der Hoek, Kooi 2015). (Within Tichý's RTT, which I use in the talk, one has model-theoretic level, so we can compute semantic values of terms and also correctness of rules; see my 2018.) My talk contributes to this research by investigating RTT's capability to treat belief attitudes, while epistemic paradoxes provide the test case. Here are some known facts in the background of the talk:

a. *RTT introduces restrictions*: ramification splits knowledge (belief, assertion ...) operator into its order variants which each has a restricted scope of applicability (e.g., K^k only applies to k-order propositions)

b. *reducibility releases restrictions*: in its general and well defensible form, the *reducibility principle* (RP) affirms the existence of lower-order 'propositional functions' that are congruent to (impredicative) higher-order ones

Thanks to the restrictions, RTT solves many epistemic and semantic paradoxes, but I show that RP brings paradoxes back (and so an additional method for their solution has to be used). Though this fact has been known for the case of the Liar and Grelling's paradoxes (Myhill 1979, Tichý 1988, Giaretta 1998, Martino 2001), it is novel for the case of epistemic paradoxes such as Church-Fitch's knowability paradox (FP) or the Knower paradox (KP). As regards FP, the 'typing approach' to FP (Williamson 2001, Linsky 2008) was criticised by various fallacious arguments (see my 2018-forthcoming for analysis); yet my RP-based version of FP seems to be a genuine one. As regards (my RTT version of) KP, the situation is more alarming, for 'typing' (Anderson 1983) has been its accepted solution. However, my analysis shows that it is the Knowledge principle that should be restricted (I draw here an analogue of Myhill's and Giaretta's response to the recurrence of semantic paradoxes).

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THE TARSKI EQUIPOLLENCE OF AXIOM SYSTEMS

Session 26L

Congress section(s): A1

In the present paper, we examine the notion of the equipollence of axiom systems provided by Alfred Tarski.
According to Tarski, two axiom systems are equipollent, if 'each term of the first system can be defined by means of the terms of the second together with terms taken from preceding theories, and vice versa' [6, p. 122]. Consequently, such axiom systems are interchangeable, i.e., as Tarski writes, one can replace the first axiom system with the second one, and vice versa [6, p. 121].
However, we demonstrate that two axiom systems for the classical propositional calculus which are equipollent in the sense of Tarski do not share the same meta-logical properties, and, consequently, are not interchangeable.
We take into consideration the well-known axiom system for the classical propositional calculus attributed to Jan Łukasiewicz. The Łukasiewicz axiom system (hereinafter AL) contains with the rule of detachment the following axiom schemata: 1. A→(B→A), 2. (A→(B→C))→((A→B)→(A→C)), 3. (~A→~B)→(B→A) (cf. [2, p. 461; 5, p. 6]). It is proved that AL is semantically complete [1, pp. 109-110; 3, pp. 96-116; 4, p. 42].
Then, we examine an axiom system (hereinafter AL') that is constructed out of negation, implication and disjunction, and which is, by mutual definitions of propositional connectives, equipollent in the sense of Tarski to AL. AL' contains with the rule of detachment the following axiom-schemata: 1'. A→(~B∨A), 2'. (~A∨~B∨C)→(~(~A∨B)∨~A∨C), 3'. (~~A∨~B)→(~B∨A).
Hence, if AL is complete, and if AL is interchangeable with the introduced AL', then AL' is supposed to be complete as well. However, we prove both by means of a proof-theoretic and a model-theoretic independence proof (cf. [3, pp. 122-125]) that there is a tautological schema which is independent from {1', 2', 3'}. Therefore, we demonstrate that not all formulae of the form of the examined schema are provable in AL.' Consequently, AL' is incomplete.
Therefore, either AL – in contrary to the common point of view – is incomplete, or two equipollent in the sense of Tarski axiom systems do not share the same meta-logical properties – the first axiom system is complete, and the second one is incomplete – and consequently, they are not interchangeable.
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DELIBERATION, SINGLE-PEAKEDNESS AND VOTING CYCLES

Session 14B

Congress section(s): A2

A persistent theme in defense of deliberation as a process of collective decision making is the claim that voting cycles, and more generally Arrowian impossibility results can be avoided by public deliberation prior to aggregation [2,4]. The argument is based on two observations. First is the mathematical fact that pairwise majority voting always outputs a Condorcet winner when the input preference profile is single-peaked. With its domain restricted to single-peaked profiles pairwise majority voting satisfies, alongside the other Arrowian conditions, rationality when the number of voters is odd [1]. In particular, it does not generate voting cycles. Second are the conceptual arguments [4, 2] and the empirical evidence that deliberation fosters the creation of single-peaked preferences [3], which is often explained through the claim that group deliberation helps creating meta-agreements [2]. These are agreements regarding the relevant dimensions along which the problem at hand should be conceptualized, as opposed to a full consensus on how to rank the alternatives, i.e. a substantive agreement. However, as List [2] observes, single-peakedness is only a formal structural condition on individual preferences. Although single-peaked preferences do entail the existence of a structuring dimension, this does not mean that the participant explicitly agree on what that dimension is. As such single-peakedness does not reflect any joint conceptualization, which is necessary for meta-agreement. Achieving meta-agreement usually requires the participants to agree on the relevant normative or evaluative dimension for the problem at hand. This dimensions will typically reflect a thick concept intertwining factual with normative and evaluative questions, for instance health, well-being, sustainability, freedom or autonomy, to name a few. It seems rather unlikely that deliberation will lead the participants to agree on the meaning of such contested notions. Of course, deliberative democrats have long observed that public deliberation puts rational pressure on the participants to argue in terms of the common good [4], which might be conducive of agreement on a shared dimension. But when it comes to such thick concepts this agreement might only be a superficial one, involving political catchwords, leaving the participants using their own, possibly mutually incompatible understanding of them [5]. All of this does not exclude the fact that deliberation might make it more likely, in comparison with other democratic procedures, to generate single-peaked preferences from meta-agreements. The point is rather that by starting from the latter one puts the bar very high, especially if there appear to be other ways to reach single-peaked preferences or to avoid cycles altogether. In view of this two questions arise regarding the claim that deliberation helps avoiding cycles:
Q1: Can cycles be avoided by pre-voting deliberation in cases where they are comparatively more likely to arise, namely in impartial cultures i.e. where a voter picked at random is equally likely to have any of the possible strict preference orderings on the alternatives?
Q2: If yes, are meta-agreements or the creation of single-peaked preferences necessary or even helpful for that?
In this work we investigate these questions more closely. We show that, except in case where the participants are extremely biased towards their own opinion, deliberation indeed helps to avoid cycles. It does so even in rather unfavourable conditions, i.e. starting from an impartial culture and with participants rather strongly biased towards themselves. Deliberation also creates single-peaked preferences. Interestingly enough, however, this does not appear particularly important for avoiding cycles. Most if not all voting cycles are eliminated, but not by reaching single-peaked preferences. We show this in a minimalistic model of group deliberation in which the participants repeatedly exchange, and rationally update their opinions. Since this model completely abstracts away from the notion of meta agreement, it provides an alternative, less demanding explanation as to how pre-voting deliberation can avoid cyclic social preferences, one that shifts the focus from the creation of single-peaked preferences to rational preference change and openness to change one's mind upon learning the opinion of others.
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IS BIASED INFORMATION EVER USEFUL (IN THE PHILOSOPHY OF SCIENCE)?

Session 6I

Congress section(s): B1

In this paper, we introduce a two-stage approach for picking out biased, yet useful information. On the first stage, the information is categorized by taking into consideration how biased the information is. The categorization is done in terms of confirmation bias, a generic label that groups a number of vices that include, but are not limited to omission, distortion, and lack of independence of the theory informing the interpretation and the theory seeking confirmation. This allows the user to build a negative filter, one that, for instance, will rule out everything but those pieces of information that exhibit at most one specific bias from the list above.
On the second stage, the information goes through a second positive filter. This filter is also built from a set of criteria, except this time, the items are epistemic virtues, and the goal is to use the filter to find the information worth keeping (despite its being somewhat biased). The approach is motivated by a couple of claims recently put forward by specific strands of virtue epistemology (VE):
That "knowledge needn't manifest virtue but instead needs only to arise from the sort of motivated inquiry that a virtuous person would engage in."
"[T]hat VE should focus less on achieving virtue and more on avoiding vice." (Turri, Alfano, and Greco 2018, §9)
We argue that the strategy is quite flexible and thus, powerful. Since the sets of criteria that each filter is built on are customizable, and both filters are largely independent of each other, the general account can be fine tuned to give rise to a very wide range of positions regarding what, if anything, counts as biased but useful information. Furthermore, we think that this generality will allow for a large number of applications.
After spelling out the details of the general strategy, we focus on its applications in the problem of relating the history and the philosophy of science. In particular, we use it to point out that the difference between top-down and bottom-up approaches to the integration of history and philosophy of science is not that the former are bias-ridden while the latter are not. Rather, the appeal of bottom-up approaches comes down to the fact that they implicitly include strategies for either minimizing bias, or making sure that whatever biased information goes through is there because it is also virtuous in some way. This immediately raises the question of whether a strategy like ours can even the field out by providing the tools that proponents of top-down approaches can use in order to minimize the risk of letting dangerously biased information through.

Turri, John, Mark Alfano, and John Greco. 2018. "Virtue Epistemology." In The Stanford Encyclopedia of Philosophy, edited by Edward N. Zalta, Summer 2018. Metaphysics Research Lab, Stanford University. https://plato.stanford.edu/archives/sum2018/entries/epistemology-virtue/.

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PROOF THEORY OF INFINITE GEOMETRIC THEORIES

Session 24C

Congress section(s): A1

A famous theorem of Barr's yields that geometric implications deduced in classical (infinitary) geometric theories also have intuitionistic proofs. Barr's theorem is of a category-theoretic (or topos-theoretic) nature. In the literature one finds mysterious comments about the involvement of the axiom of choice. In the talk I'd like to speak about the proof-theoretic side of Barr's theorem and aim to shed some light on the AC part.

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DEDEKIND, NUMBER THEORY, AND METHODOLOGICAL STRUCTURALISM: A MATTER OF STYLE?

Session 18D

Congress section(s): C1

In describing the contributions of Richard Dedekind to nineteenth-century number theory, the mathematician and historian of mathematics H.M. Edwards has attributed a distinctive "style" of doing mathematics to him, one that stands in contrast to that of his contemporary Kronecker and had a strong influence on twentieth-century mathematics. Other historians and philosophers of mathematics have credited Dedekind with contributing to a characteristically "conceptual" approach to mathematics, as inaugurated by his teachers and mentors Gauss, Dirichlet, and Riemann, as opposed to a more "computational" alternative, again often identified with Kronecker and his followers. And more recently, some authors have talked about a "structuralist style" as exemplified by Dedekind's work, as related to but also distinguishable from a "structuralist view of mathematical objects" one can find in that work as well.
The present talk has three main goals: The first is to describe Dedekind's contributions to number theory briefly so as to make its "structural" nature evident, primarily in a methodological but secondarily also in a metaphysical sense. This will involve contrasting his approach to mathematics with that of his contemporary Kronecker, as well as exploring his influence on twentieth-century set theory, especially via Zermelo's work; and it will lead to his impact on later "structuralist" mathematicians, such as Noether, Artin, and Bourbaki. Second, the question will be addressed in which sense one can, and should, talk about a "style" here, i.e., how exactly that notion should be understood in this context. More particularly, can it be spelled out in a philosophically significant way, and especially, in an epistemological sense? The argument will be that it can, although the notion of "style" is in need of clarification, which will also be provided to some degree.
The third main goal, which builds on the first two, will be to explore a dynamic change in this connection, involving several stages: from the "conceptual style" exemplified by Dedekind's teachers, especially Dirichlet and Riemann, which Dedekind adopted early on as well; to Dedekind's own "structuralist style", with its added set-theoretic and category-theoretic elements; through a more maturely "structuralist style" in figures such as Noether, Artin, and Bourbaki; and finally, leading to the "category-theoretic style" of Mac Lane, Grothendieck, and others. An added outcome of this part of the talk will be that the notion of "style" is particularly helpful if one views mathematics not as a static, finished system of theorems and proofs, but as a developing practice, so that center stage is taken by ways in which novel research is undertaken, mathematical ideas and results are re-organized, and the field is pushed in new directions.
This talk is meant to be part of a symposium, entitled "Styles in Mathematics" (acronym: SIM, co-organized by E. Reck & G. Schiemer), in which some of the themes central to it will be explored in more general ways.

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FAKE NEWS, PSEUDOSCIENCE, AND PUBLIC ENGAGEMENT

Session 5B

Congress section(s): B1, B5

In this presentation I plan to consider issues that lie at the intersection of fake news and pseudoscience, and their implications for the science community and broader society.
The term "fake news" is understood in this context to refer to information that is deliberately fabricated, and often distributed in ways that mimic the formats of news media, thus lending it, at least superficially, a semblance of credibility. Internet platforms make it possible for fake news items to be spread rapidly, with numbers of recipients or readers increased by orders of magnitude.
The dissemination of pseudoscientific arguments might be regarded as a subset of fake news. They are characterized by a lack of supporting evidence, erroneous arguments, and a general incompatibility with the scientific method.

A key question confronting scientific organizations, among others, concerns the development of interventions aimed at combating activities that undermine the scientific consensus. These are most effectively multi-dimensional, as is evident from a consideration of the relevant constituencies: from major social media platforms, to the scientific community, to the general public. Relevant aspects, which would vary according to the constituency, range from the urgency in building trust between scientists and the public; and the development of critical reading skills from the school level up. More broadly, the presentation will reexamine, in the context described, the role of and approaches to public engagement with science.

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CLASICAL LOGIC AND SCHIZOPHRENIA: FOR A NEUTRAL GAME SEMANTICS

Session 26M

Congress section(s): A2

In this paper we draw a proposal to develop a logic of fictions in the game-theoretical approach of dialogical pragmatism. From one of the main criticisms that point to classical logic: the structural schizophrenia of its semantics (Lambert, 2004: 142-143; 160), we review the ontological commitments of the two main traditions of logic (Aristotle and Frege) to highlight their limits concerning the analysis of fictional discourse, and the overcoming from a pragmatic game perspective.
In specialized literature, we can often find objections against the presumed explanatory power of logic and formal languages in relation to fictional discourse. Generally, the aim of such critics is the rol that the notion of reference fulfills in the logic analysis of fiction; according to our view, the most ideal approach would be a more pragmatic account. We respond to this objection affirming that, if we elaborate a context of adequate analysis, a properly pragmatic treatment of fiction in logic is possible without the restrictions imposed by the notion of reference. Dialogic logic, which considers the arguments as a interactive chain of questions and answers, offers an ideal analysis context for a better pragmatic approach.
In this sense we believe in the richness of the perspective of the dialogic logic that comprises existence through the interactional concept of choice, whose semantics is based on the concept of use and can be called pragmatic semantics.
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REALISM AND REPRESENTATION IN MODEL-BASED EXPLANATION

Session 20J

Congress section(s): B1

There has been a growing interest in the study of scientific representation. One of the motivations given for studying representation is that representational capacity is taken to be a requirement for successful explanation, one of the main aims of scientific inquiry. This is of special importance when accounting for the explanatory power of scientific models, amounting to what is sometimes called the ‘representationalist’ view according to which, in order to explain, a model has to represent its target phenomenon in a suitable manner. Interestingly, this assumed connection between scientific explanation and representation is most explicitly spelled out when it is actively criticized by authors who point towards the role of essential, false components in models employed for explanatory purposes in order to motivate accounts of explanation that do not rely on successful representation. In close neighborhood to this idea about representation one often finds a general preference in favor of realism in lots of the most prominent accounts of scientific explanation according to which the principles employed in our explanations should be approximately true. This is often, at least implicitly so, formulated in terms of representation. In this way, a representational match serves realism. Hence, the realist presupposition can be criticized with reference to much of the same problem cases as the representationalist view of explanation.
In this contribution, I will argue that it serves to look at what specific accounts of representation can do for the explanation debate and that the link between realist explanation and representationally successful explanation can be loosened in the process. There are multiple different accounts of representation that have been proposed in recent years and not all of them serve realist tendencies in model-based explanation equally. The view of representation that often seems to be presupposed by realist as well as by explicitly non representationalist accounts of explanation is one of representation as an objective two-place relationship between model and target phenomenon in terms of similarity or isomorphism. While this is in line with realist requirements on scientific models, it carries much of the same problems as well. Furthermore, pragmatic accounts that take into account the role of agents interpreting and drawing inferences from models have emerged as an alternative. These are not conducive to realism about explanatory models in the same way. They have found little to no application to accounts of model-based explanation. I argue that it is beneficial to connect them more closely. The result is a view of model-based explanation that is representational but not realist, and that avoids some of the drawbacks of the realist version.

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THREE WAYS TO UNDERSTAND THE INDUCTIVE THOUGHTS OF WHEWELL

Session 17L

Congress section(s): A4, B6

The inductive thinking of Whewell has been underestimated for a long time, and it seems that there is trend which simply treat his theory as a typical version of hypothetico -deductivism. However, our research found that, besides the classical hypothetico -deductivism interpretation, Whewell’s thoughts also can be interpreted as coherence theory, or the best explanatory inference. As a result, these three ways of interpretations are mutually intertwined. In our opinion, existing many ways to understand Whewell’s inductive thoughts shows that, on the one hand, his ideas are very complicated and profound, therefore deserved to study in-depth; on the other hand, it shows that our understanding of the nature of the

induction is still rather poor, and we must take into account that the coherence standard is indispensable for inductive logic system.

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PEIRCE ON THE LOGIC OF SCIENCE – INDUCTION AND HYPOTHESIS

Session 16H

Congress section(s): B6

In his Harvard Lectures on the Logic of Science from 1865 Peirce for the first time presented his logical theory of induction and hypothesis as the two fundamental forms of scientific reasoning. His study of the logic of science seems to have been initiated by the claim of William Hamilton and Henry L. Mansel that "material inferences", which Peirce calls a posteriori and inductive inferences, are to be considered as "extralogical". In consequence they regarded the principles of science, which Kant maintained to be valid a priori, to be as axioms "not logically proved". In opposition to this view Peirce in his Harvard Lectures seeks to establish first, that deduction, induction and hypothesis are three irreducible forms of reasoning which can be analysed with reference to Aristotle's three figures of syllogism as the inference of a result, the inference of a rule and the inference of a case respectively and second, with reference to Kant's doctrine that a synthetic "inference is involved in every cognition" and Whewell's distinction of fact and theory, that "every elementary conception implies hypothesis and every judgment induction" and that therefore we can never compare theory with facts but only one theory with another one and that consequently the universal principles of science, for instance the principle of causation, as conditions of every experience, understood as a theory inferred from facts, can never be falsified and are therefore valid a priori. Peirce develops his position examining the theories of induction of Aristotle, Bacon, Kant, Whewell, Comte, Mill and Boole. The paper will first reconstruct the main points of Peirce's discussion of the theories of these authors and give a critical account of his arguments and motives and second analyse his syllogistic and transcendental solution of the problem of a logical theory of scientific reasoning. This second part will be supplemented by an account of the significance of later revisions of the logic of science by Peirce in his Lowell Lectures (1866), the American Academy Series (1867), the Cognition Series (1868/69), the Illustrations of the Logic of Science (1877/78) and How to Reason (1893). The main focus of the paper will be Peirce's reformulation of Kant's conception of transcendental logic as a logic of science and therefore of synthetic reasoning with reference to a reinterpretation of Aristotle's Syllogistic.

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DYNAMIC TERM-MODAL LOGIC

Session 15B

Congress section(s): A2

Term-modal logics are first-order modal logics where the operators doubles as first-order predicates: in e.g. an epistemic knowledge operator K_a, the 'a' here is not merely an index, but a first-order term. Term-modal syntax considers e.g. "exists.x K_x phi(x)" a well-formed formula. Semantically, the knowing agents are elements in a proper domain of quantification, rather than mere indices as is the case in ordinary epistemic logic. Term-modal logic thus treat agents as first-class citizens, both syntactically and semantically. This has the effect that variants of the Descartes' Cogito may be expressed, e.g. by the valid "(not K_a phi) implies (K_a exists.x K_x phi)". Likewise, term-modal logics may in a natural way be used to expresses agents' (higher-order) knowledge about their relation R to others in a social network, e.g. by "K_a (R(a,b) and not K_b R(a,b))".

The above are inherently term-modal expressions. Though e.g. the latter may be emulated using ordinary propositional epistemic logic with operators K_a, K_b and atomic proposition r_{ab}, such solutions are ad hoc as they do not logically link the occurrences of the indices of the operators and the indices of the atom. Inherently term-modal expressions have been considered as early as von Wright's 1951 "An Essay in Modal Logic", with Hintikka's syntax in (1962, "Knowledge and Belief") in fact being term-modal. Despite their natural syntax, term-modal logics have received relatively little attention in the literature and no standard implementation exists. Often, combinations of well-known difficulties from first-order modal logic mixed with oddities introduced by the term-modal operators result in tri-valent or non-normal systems, systems with non-general completeness proofs, or systems satisfying oddities like "P(x) and forall.y not P(y)".

In this paper, we present a simple, well-behaved instance of a term-modal system. The system is bivalent, normal, with close-to-standard axioms (compared to similar first-order modal logics), and allows for a canonical model theorem approach to proving completeness for a variety of different frame classes. The "static" system may be extended dynamically with a suitable adaption of so-called "action models" from dynamic epistemic logic; for the extension, reduction axioms allow showing completeness. Throughout, the system is illustrated by showing its application to social network dynamics, a recent topic of several epistemic and/or dynamic logics."

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DOES ANALOGICAL REASONING IMPLY ANTHROPOMORPHISM?

Session 5H

Congress section(s): B3, C5

The goal of my talk is to present the part of the big empirical research on the correlation between logical reasoning and cognitive biases I'm working on.

For this presentation, I decided to focus only on analogical thinking to be sure, that I will have time to do the correct results scoring of my hypothesis. I assume that people who are better in analogical reasoning may also be more prone to anthropomorphism. In my view, people who are good at finding logical analogies are more liable to see the features of the living creatures in the inanimate things because both of these tendencies require some imagination and creative thinking.

To check this theory I am working on a survey that will be launched in February.

To test the level of anthropomorphism I will use three different questionnaires that affect different areas of perception. Two of which were created by myself. They focus on visual and auditory perception and do not require deep thinking. While the third one, I have taken from Waytz's, Cadoppo's, and Epley's study (2010), ask some reflections. I have chosen this scale because of its high reliability that was confirmed by different studies before (Cronbach's Alpha: 0.86).

To verify the logical abilities of our subjects I will ask them to finish the analogical arguments making a conclusion from the two premises. In addition, they will also pass the short verbal and geometrical analogy tests of form A:B:C:?.

As the platform for my survey, I will use the Lime Survey, because it is easy to use as well as comfortable to analyze the data at the end. Besides, in my opinion, it is more cheering for people to answer their questions privately when nobody confuses them with unwelcome attention. This way, I expect to get more honest answers.

The scoring will be done in SPSS.

The control group will be the over 100 students from 16 to 25 years old from my alma mater. I will divide that in two group (50x50): those who have and who have not learned logic or critical thinking before. I know that this decision narrows down the sample. Yet, I think that it will be better to rate the different age groups separately. The objective reasons are that I need to be sure that the subjects are about the same level of education and do not have any mental issues, which could be caused by age, like, for example, a memory loss.

Although the survey will be in Ukrainian, I will translate some example in English for the presentation.

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EXPRESSIVE POWER AND INTENSIONAL OPERATORS

Session 3J

Congress section(s): A2, C1

In his book Entities and Indices, M. J. Cresswell advocated the view that modal discourse in natural language requires a semantics whose power amounts to explicit quantification over possible worlds. His argument for this view had two parts. First, he argued that an operator-based intensional language can only reach the expressive power of English if it has a semantics with infinite world indices and employs infinite actuallyn/Refn-operators which store and retrieve such indices. (See Cresswell 1990, ch. 3, esp. pp. 40–46. Cresswell's actuallyn/Refn-operators are modal correlates of the Kn/Rn-operators introduced in the appendix of Vlach 1973 (pp. 183–185).)
Second, he gave a formal proof that an operator-based language of this kind is as powerful as a language of predicate logic with full quantification over worlds (see Cresswell 1990, ch. 4). Cresswell also suggested that possible worlds are semantically on a par with times and argued that temporal discourse in natural language has the power of full quantification over times.
In a recent paper, I. Yanovich (2015) argues that a first-order modal language equipped with standard modal operators and Cresswell's actuallyn/Refn-operators—following Saarinen (1978), Yanovich calls the latter backwards-looking operators—is less powerful than an extensional first-order language with full quantification over worlds or times. Yanovich suggests that the widespread belief to the contrary among philosophers and linguists is the result of a misinterpretation of Cresswell's formal proof. He observes that Cresswell's proof assumes that the modal language with backwards-looking operators also employs an operator of universal modality □, which adds extra expressive power to the language.
One important drawback of Cresswell's and Yanovich's discussions is they do not offer a precise definition of the notion of expressive power. Without such a definition, it is not thoroughly clear what criterion they are adopting in order to compare the power of different logical systems. In this paper, we provide a precise definition of expressive power—based on Ebbinghauss's notion of logical strength (see Ebbinghauss 1985, sec. 3.1)—that can be applied to the formal languages discussed by Cresswell in Entities and Indices. Armed with this definition, we address the question of whether a modal language equipped with Cresswell's actuallyn/Refn-operators and an operator of universal modality is as powerful as a first-order language with full quantification over worlds or times.
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HOW DO EVOLUTIONARY EXPLANATIONS EXPLAIN?

Session 29I

Congress section(s): B4, C3

In this talk I address the explanatory scope of evolutionary theory and the structure of evolutionary explanations. These issues are in need of clarification because of (at least) two factors. First, evolutionary theory has been undergoing continuous, profound change from Darwin's formulation via late nineteenth-century Neo-Darwinism to the mid-twentieth century Modern Synthesis, to a possible Extended Synthesis that is being considered today. These changes, which are likely to continue after the current debate on a possible Extended Synthesis has been settled, affect both what evolutionary theory is thought to explain and the ways in which it can perform explanatory work. Second, investigators in areas outside biology, such as evolutionary economics and the various evolutionary programs in the social sciences (e.g., Aldrich et al., 2008; Hodgson & Knudsen, 2010), are increasingly attempting to apply evolutionary theory to construct scientific explanations of non-biological phenomena, using different kinds of evolutionary models in different domains of research. This raises a number of questions: Exactly how much can be explained by applying an evolutionary framework to non-biological systems that differ widely from biological ones? Can applications of evolutionary theory outside biology can achieve a similar explanatory force as when applied to cases in biology? What – if any – basic explanatory structure unifies the different evolutionary models used in biology and the different areas of the social sciences? And so on.
I will try to achieve more clarity on these questions by treating them as a set of questions about the ontology of evolutionary phenomena. My claim is that practices of applying evolutionary thinking in non-biological areas of work can be understood as what I call "ontology-fitting" practices. For an explanation of a particular phenomenon to be a genuinely evolutionary explanation, the explanandum's ontology must match the fundamental ontology of evolutionary phenomena in the biological realm. This raises the question what elements this latter ontology consists of. However, there is no unequivocal answer to this question. There is ongoing discussion about the question what the basic elements in the ontology of biological evolutionary phenomena (such as the units of selection, the units of replication, etc.) are and how these are to be conceived of. Therefore, practitioners from non-biological areas of work cannot simply take a ready-for-use ontological framework from the biological sciences to fit their phenomena into. Rather, they pick elements from the biological evolutionary framework that seem to fit their phenomena, disregard other elements, and construct a framework that is specific to the phenomena under study. By examining cases of such "ontology fitting" we can achieve more clarity about the requirements for using evolutionary thinking to explain non-biological phenomena, as well as about the question how evolutionary explanations explain.
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FREQUENCY INTERPRETATION OF CONDITIONS FOR THE APPLICATION OF PROBABILITY THEORY ACCORDING TO KOLMOGOROV

Session 11L

Congress section(s): C1

In the well-known book by Kolmogorov, devoted to the axiomatic theory of probability, the requirements for probabilities were formulated in the context of their applications [1]. Why, in that publication, A.N. Kolmogorov turned to the problem of applying mathematics? The answer was given in a later work by Kolmogorov; he noted that successes in substantiating mathematics overshadowed an independent problem: "Why is mathematics applicable for describing reality" [2].

Kolmogorov formulated the following requirements for probabilities in the context of applications:
«A. One can practically be sure that if the set of conditions S is repeated a large number of times n and if m denotes the number of cases in which the event A occurred, then the ratio m/n will differ little from P(A).
B. If P(A) is very small, then one can practically be sure that, under a single realization of conditions S, event A will not take place» [1, P. 4].
The first requirement is an informal version of von Mises' asymptotic definition of probability. The second condition describes Cournot's principle in a strong form. These requirements are bridges that connect probability theory and mathematical statistics with reality. However, in the known contemporary literature, the question on compatibility of Kolmogorov's requirements has not been studied till the works of Shafer and Vovk [3]. As Shafer and Vovk noted, Borel, Levi and Frechet criticized the redundancy of condition A, since they believed that its formal description is the conclusion of Bernoulli's theorem. The report considers the frequency interpretation of condition A, since Kolmogorov noted that in the context of applications he follows Mises, the founder of the frequency interpretation.
The main thesis of the report is that condition A in the frequency interpretation is not the conclusion of Bernoulli's theorem. As is known, the conclusion of the theorem asserts that the frequency of a certain event A and the probability of event A, the probability is a constant, are close in probability. In the report I prove that, in the frequency interpretation, condition A is interpreted geometrically. I present the following arguments in defense of the thesis.
First, in the frequency interpretation, the probabilities of events do not exist a priori, they are representatives of the frequencies of these events. It is natural to consider that a constant probability of an event exists if the frequency characteristics of this event turn out to be stable, for example, they occupy a small interval. Secondly, the geometrical explication of condition A is quite consistent with the definition of probability in von Mises' frequency interpretation, since Mises defines probability on the basis of convergence of frequencies defined in mathematical analysis. Thirdly, our thesis gets support on the basis of the principle of measure concentration proposed by V. Milman. According to this principle, the functions of very many variables, for example, on a multidimensional sphere and other objects turn out to be almost constant. In accordance with this principle, functions of a large number of observations that calculate frequencies turn out to be almost constant. Thus, condition A does not depend on Bernoulli's theorem, but, on the contrary, turns out to be a precondition for the application of this theorem.

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ON HOWSON'S BAYESIAN APPROACH TO THE OLD EVIDENCE PROBLEM

Session 25K

Congress section(s): B2

The old evidence problem has been regarded as a typical intractable problem despite many challenges to it in Bayesian confirmation theory since 1980. Recently, Colin Howson (2017) proposes a new solution to the problem, which is considered as a part of the minimalist version of objective Bayesianism in that it focuses on priors and likelihood ratio.
Howson's new solution has two pints. (a) Classical objective Bayesian approach has no difficulty in accommodating the historical fact that Mercury's anomalous advance of perihelion confirmed GTR in a simple Bayes's Theorem computation. (b) Counterfactual strategy is an attempt to graft objective probability valuations onto a subjective stock, and apart from being rather obviously ad hoc it does not work.
Howson's new solution can work well in Bayesian framework, if we ignore the fact that he is a well-known subjective Bayesian and he suggested a solution, the counterfactual solution to the old evidence problem. However, when we consider it, his new solution appears as a very obscure idea. In this paper, I raise two problems to his new solution. (c) Generality problem: The new solution can work in solving the old evidence problem, but it cannot be applied to other problems such as Ravens paradox, Duhem's problem, and the thesis of underdetermination of scientific theory by evidence? (d) Identity problem: It is not clear whether his position belongs to subjective Bayesianism or objective Bayesianism, or even likelihoodism. Howson's new solution introduces objective factor to subjective Bayesianism, which leads ultimately to a hybrid Bayesianism, not to subjective Bayesianism.
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DEFINITE TRUTH

Session 6M

Congress section(s): A2

Any attempt of giving a formal representation of truth is challenged by sentences like the Liar ("What I am saying is not truth"), and the Truth-teller ("What I am saying is true"). Both sentences are felt to be problematic, but in different ways. While the Liar looks paradoxical, meaning that either assuming it to be true or assuming it to be false seems to contradict our intuitions about truth, the Truth-teller looks arbitrary, in the sense that both assume that it is true and assume that it is false seem to be equally defensible theses, according to our intuitions about truth.
Most formal representations of truth validate sets of sentences which exclude both Liars and Truth-tellers and, more general, aim to validate only sentences which are intuitively definite, in the sense of being neither paradoxical nor arbitrary. Clearly, even though most formal representations agree on assessing the Liar as paradoxical and the Truth-teller as arbitrary, the technical notions intended to capture the intuitive concepts of paradoxical, arbitrary and definite differ from one proposal to another. However, many formal representations can be recast to show a common pattern that we are going to illustrate.
Some formal accounts identify a family of truth assignments which are "foreseeable", in the sense of respecting some provisos: for instance, to be a fixed-point of some monotone evaluation scheme, or to arise as the set of stabilities of some process of revision, or to be a model of an axiomatic theory. What is common to all these approaches is that, usually, the Liar already is excluded from the domains of the foreseeable truth assignments, while the Truth-teller does belong to the domain of some foreseeable truth assignment and it is only filtered out by applying the following (implicit) meta-norm of "definiteness": whenever a formal account of truth chooses a designated truth assignment, this one is the only admissible way of assigning classical truth values to the sentences in its domain.
In my talk I will contrast, both from a theoretical and from a philosophical point of view, three different ways of formalising the above intuitive notion of "definiteness" of a given set of sentences, namely, (1) determinacy (= there exists only one foreseeable truth assignment whose domain is the given set); (2) intrinsicity (= there exists a foreseeable truth assignment for the given set which is compatible with any other foreseeable truth assignment); and (3) definiteness properly named (= there exists only one truth assignment for the given set which is compatible with any other foreseeable truth assignment), and I will investigate their different philosophical motivations and their theoretical pros and cons.
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A BRIDGE FOR REASONING: LOGICAL CONSEQUENCE AS NORMATIVE

Session 8L

Congress section(s): A2

How do we reason? How should we reason? Intuitively, the question of how we conduct our everyday inferential practice is central to living in a certain community, as well as being central within the philosophy of logic.
In this essay, I am going to argue that the form of logical consequence accepted by a community dictates how an agent in that community should reason, by imposing a normative constraint on the agent's reasoning. I will also argue that this normative constraint is an important part of the nature of logical consequence. I will take reasoning, after Harman, to be defined as 'reasoned change in view '. I will also be using an interpretationalist, model-theoretic account of consequence.

I will claim that the exact normative relation between a form of logical consequence and reasoning can be specified by a bridge principle. A bridge principle is a conditional which links a form of logical consequence to a normative claim about reasoning. For instance, MacFarlane proposes the following as possibilities for a bridge principle:
(Cr+) If A,B⊨C, then if you believe A and you believe B then you have reason to believe C
(Co+) If A,B⊨C, then if you believe A and you believe B then you ought to believe C
I will propose an adaptation to one of Macfarlane's bridge principles as being the most plausible candidate for describing the normative relation between logical consequence and reasoning and assess how it fares in response to certain objections and paradoxes. This adapted bridge principle will be called (Co+d):
(Co+d) If A,B⊨C [is accepted by your reasoning community], then if you believe A and you believe B, you ought to have a disposition to believe C
The structure of the paper is as follows. First, in (S1) I outline what it is to say that logical consequence is normative and how this works within an interpretationalist account of model-theoretic consequence. I put forward what I consider to be MacFarlane's two most promising bridge principles, Cr+ and Co+. Then in (S2) I will raise three definitive objections to these bridge principles. In (S3) as a response to these objections, I propose another more plausible bridge principle, Co+d, and assess how it responds to these objections. Lastly, in (S4) I further assess the bridge principle in relation to the paradox of the preface and the issue of priority through comparison to the bridge principle that MacFarlane considers to be the most successful (FBP).
I conclude that the normativity of logical consequence can be demonstrated through the use of an appropriate bridge principle, linking logical consequence to reasoning and that the most plausible candidate for this bridge principle is Co+d.
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BASIC QUASI-BOOLEAN EXPANSIONS OF RELEVANT LOGICS WITH A NEGATION OF INTUITIONISTIC KIND

Session 17M

Congress section(s): A2

Abstract. Let L be a logic including the positive fragment of Anderson and Belnap's First Degree Entailment Logic, the weakest relevant logic (cf. [1]). In [5] (cf. also [3] and [4]), it is shown that Boolean negation (B-negation) can be introduced in L by adding to it a strong version of the "conjunctive contraposition" $((A \wedge B) \rightarrow \neg C \Rightarrow (A \wedge C) \rightarrow \neg B$, in particular) and the axiom of double negation introduction (i.e., $\neg\neg A \rightarrow A$). Nevertheless, it is not difficult to prove that B-negation can equivalently be axiomatized by adding to L the "Ex contradictione quodlibet axiom" (ECQ, i.e., $(A \wedge \neg A) \rightarrow B$) and the "Conditioned Principle of Excluded Middle axiom" (Conditioned PEM, i.e., $B \rightarrow (A \vee \neg A)$). From the point of view of possible-worlds semantics, the ECQ-axiom can be interpreted as expressing the thesis that all possible worlds are consistent (no possible world contains a proposition and its negation). The conditioned PEM, in its turn, would express that all possible worlds are complete (no possible world lacks a proposition and its negation). Thus, the ECQ-axiom and the conditioned PEM-axiom are the two pillars upon which B-negation can be built in weak positive logics such as the positive fragment of Anderson and Belnap's First Degree Entailment logic. This way of introducing B-negation in relevant logics suggests the definition of two families of quasi-Boolean negation expansions (QB-expansions) of relevant logics. One of them, intuitionistic in character, has the ECQ-axiom but not the conditioned PEM-axiom, the other one, dual intuitionistic in nature, has the conditioned PEM-axiom, but not the ECQ-axiom. The aim of this paper is to define and study the basic QB-expansions of relevant logics built up by using the former type of negation, the one of intuitionistic sort. We shall provide an unreduced Routley-Meyer type semantics (cf. [2] and [5]) for each one of these basic QB-expansions. B-negation extensions or expansions of relevant logics are of both logical and philosophical interest (cf. [5], pp. 376, ff.). For example, the logic classical R, KR, the result of extending relevant logic R with the ECQ-axiom, plays a central role in the undecidability proofs for relevant logics by Urquhart (cf. [6]). It is to be expected that quasi-Boolean negation expansions of relevant logics (not considered in the literature, as far as we know) will have a similar logical and philosophical interest.

ACKNOWLEDGEMENTS: Work supported by research project FFI2017-82878-P of the Spanish Ministry of Economy, Industry and Competitiveness.

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FORMAL PROOF-VERIFICATION AND MATHEMATICAL INTUITION: THE CASE OF UNIVALENT FOUNDATIONS

Session 4F

Congress section(s): C1

The idea of formal proof verification in mathematics and elsewhere, which dates back to Leibniz, made important advances since the beginning of the last century when David Hilbert launched the research program called today after his name. In the beginning of the present century an original contribution into this area of research has been made by Vladimir Voevodsky (b. 1966 Moscow - 2017 Princeton) who proposed a novel foundations of mathematics that he called Univalent Foundations (UF). UF involves an interpretation of Constructive Type theory with dependent types due to Per Martin-Löf by means of Homotopy theory (called Homotopy Type theory or HoTT for short) and is designed to support an automated, viz. computer-assisted, verification of non-trivial mathematical proofs [1].
The present paper analyses the epistemic role of mathematical intuition in UF. The received view on the role of intuition in formalized mathematical theories stems from Hilbert. Hilbert stresses the importance of human capacity to distinguish between different symbol types, identify different tokens of the same symbol type and manipulate with symbols in various ways. He qualifies this cognitive capacity as a form of intuition and claims that it plays a major epistemic role in formal logic and formalized mathematical theories. All other forms of mathematical intuition, in Hilbert's mature view, have significant heuristic and pedagogical values but play no role in the formal representation and justification of ready-made mathematical theories (see [2], section 3.4 and further references therein).
Unlike Hilbert Voevodsky didn't write a philosophical prose but he expressed his vision of mathematics and explained motivations of his projects in many public talks, which are now available in record via his memorial web page maintained by Daniel Grayson in Princeton Institute of Advanced Studies [3]. In a number of these talks Voevodsky stresses the importance of preserving in the framework of formalized computer-assisted mathematics an "intimate connection between mathematics and the world of human intuition" . Using a simple example of classical theorem in the Algebraic Topology formalized in the UF setting [4], I show how a spatial intuition related to Homotopy theory serves here as an effective interface between the human mind and the computer, and argue that it plays in UF not only heuristic but also a justificatory role.
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RATIONALITY PRINCIPLES IN PURE COORDINATION GAMES

Session 16B

Congress section(s): A2

We analyse so-called pure win-lose coordination games (WLC games) in which all players receive the same playoff, either 1 ("win") or 0 ("lose"), after every round. We assume that the players cannot communicate with each other and thus, in order to reach their common goal, they must make their choices based on rational reasoning only.
We study various principles of rationality that can be applied in these games. We say that a WLC game G is solvable with a principle P if winning G is guaranteed when all players follow P. We observe that there are many natural WLC games which are not unsolvable in a single round by any principle of rationality, but which become solvable in the repeated setting when the game can be played several times until the coordination succeeds.
Based on our analysis on WLC games, we argue that it is very hard to characterize which principles are "purely rational" - in the sense that all rational players should follow such principles in every every WLC game.

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TECHNOSCIENCE AND PHILOSCIENCE

Session 26K

Congress section(s): B1, C8

According to the traditional views science and technology are definitely different from each other: while the former deals with facts, the latter deals with artifacts. Because of the radical changes in the recent forms of technological and scientific practices the validity of this traditional position has became uncertain, and a new view has emerged - to rethink the science-technology relations using the concept of technoscience.
The term "technoscience" became an essential component of discussions on the science- technology-society complex following the appearance of Latour's Science in action (Latour 1987). In the last decades number of studies were published on the history, philosophy and sociology of technoscience e.g. by Latour, Ihde, Barnes, Klein, Pickstone, Nordmann, Bensaude-Vincent and others (see the references below).
In this lecture I try to contribute to this discussion introducing the concept of philoscience as an alternative concept to technoscience. While the concept of technoscience expresses the entanglement of the traditional forms of science and technology in a given socio-historical environment, the concept of philoscience expresses the entanglement of science and philosophy in a given socio-historical environment.
On the account proposed in this lecture, all science is technoscience in its any historical forms; there is no science without technological components. On the other hand, at the same time all science is philoscience in its any historical forms as well; there is no science which would not include philosophical components.
When we speak about "science", unqualified, this inner structure of scientific knowledge remains obscured. It is always a fusion of technological and philosophical components that results in the formation of a "scientific matter", i.e., a concrete socio-historical form of science. The relative weight of technological and philosophical components in the mixture, and the level of their integration are challenges to be taken up by the history and philosophy of science and technology, and by further studies on the interrelatedness of technology, science, and philosophy.
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ACCESSIBLE CATEGORIES AND MODEL THEORY

Session 18A

Congress section(s): A1

Accessible categories were introduced by M. Makkai and R. Paré as a framework for infinitary model theory. They have turned out to be important in algebra, homotopy theory, higher category theory and theoretical computer science.
I will survey their conections with abstract elementary classes and discuss how model theory of abstract elementary classes can be extended to that of accessible categories. In particular, I will present a hierarchy beginning with finitely accessible categories and ending with accessible category hsving directed colimits.

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LOGICAL EMPIRICISM IN EXILE. HANS REICHENBACH'S RESEARCH AND TEACHING ACTIVITIES AT ISTANBUL UNIVERSITY (1933–1938)

Session 18L

Congress section(s): B3, B6, B7, C5, C7

The purpose of this paper is to shed new light on a less well-known stage in the development of Hans Reichenbach's thought, namely his research, output and teaching activities at Istanbul University (1933–1938). During his exile in Turkey, Reichenbach was able to continue his efforts to popularize and extend the program of "scientific philosophy," not only through the restructuring of the Department of Philosophy at Istanbul University, but also through different academic exchanges with European countries. Between the beginning and end of Reichenbach's exile in Istanbul, the Turkish reception of logical empiricism and scientific philosophy is characterized by a shift from a mere external interest in these fields to an effective implementation of the principles and methods that characterize Reichenbach's philosophical approach. The aim of this paper will be to show that Reichenbach's impact was not limited to an unilateral transfer of knowledge but that it has led to an active participation of the Turkish side in establishing a link between philosophy and particular scientific disciplines (Einzelwissenschaften) at Istanbul University. Therefore, the consideration of Reichenbach's output between 1933 and 1938 must necessarily be complemented with the study of the courses he gave at Istanbul University as well as the study of the work of his students, most of which were only completed after Reichenbach's departure to the United States. His students' under-researched contribution to the development of this scientific philosophy may seem to have quickly faded away at the philosophy department of Istanbul University, but one hypothesis I will examine is whether its more durable impact did not in fact happen in other fields, for example the development of experimental psychology in the same university.

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EMPIRICAL INVESTIGATION OF THE LIAR PARADOX. HUMAN BRAIN PERCEIVES THE LIAR SENTENCE TO BE FALSE.

Session 5H

Congress section(s): A2, B3, C5

Paradoxes are the unwanted children of philosophy. Whenever a paradoxes are discovered, philosophers immediately start working to identify their cause and make them disappear. For the Liar paradox (commonly formulated as: "This sentence is false"), that work started 2500 years ago and continues to this day. In our work we present an empirical investigation of one of the most popular solutions to this paradox.
In XV-th century Jean Buridan proposed that every sentence in natural language implicitly asserts its own truth (i.e. the virtual entailment principle). Adopting this principle means that the Liar sentence is not paradoxical but false, because its content is contradictory to what is virtually implied. From that, Jean Buridan follows that humans should perceive the Liar sentence the same way as any other false sentence. This solution to the Liar paradox received criticism for making ad hoc claims about natural language. There is no apparent reason to assume that every sentence asserts its own truth, other than to get rid of the paradox. However, thanks to modern advancements in psychophysiology, it became possible to empirically verify if human brain really perceives the Liar sentence like a false sentence.
We designed and conducted an electroencephalographic experiment to examine brain activity during the comprehension of the Liar sentence. We compared it to brain activity during comprehension of true sentences, false sentences and Truthteller sentences ("This sentence is true"). The experiment was conducted according to all the modern standards of neuroimaging. In our paper we minutely describe all the details of experimental procedure, brain activity recording and statistical analysis of the results. Our results show that the human brain processes the Liar sentence identically to false sentences, and that the Truthteller sentence is processed identically to true sentences. This provides evidence for the Buridan's hypothesis and the virtual entailment principle. These results have several implications for contemporary theories of truth and logic.
We demonstrate that it is possible to investigate the inherent "theory of truth" embedded in the human brain and that a theory of truth can have predictive power with regard to physical phenomena. However, we do not demonstrate that the Liar sentence is false. We merely demonstrate that humans perceive the Liar sentence like if it was false. This places our experiment within the relativistic view on truth, where adjectives pertaining to truth-value are assessment-sensitive and an agent is required to perceive them to assess them. Finally, finding empirical evidence for the virtual entailment principle supports the idea that humans think with the logic of truth (a logic for which the truth is a designated value of its adequate semantics). This demonstrates that the conclusions of non-Fregean logics regarding the Liar paradox coincide with human understanding of language.

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MODAL NOTIONS AND THE COUNTERFACTUAL EPISTEMOLOGY OF MODALITY

Session 12D

Congress section(s): B4, C7

The paper discusses a conceptual tension that arises in Williamson's counterfactual epistemology of modality: that between accepting minimal requirements for understanding, on the one hand, and providing a substantial account of modal notions, on the other. While Williamson's theory may have the resources to respond to this criticism, at least prima facie or according to a charitable interpretation, we submit that this difficulty is an instance of a deeper problem that should be addressed by various types of realist theories of metaphysical modality. That is, how much of the content of metaphysical modal notions can be informed through everyday/naturalistic cognitive and linguistic practices? If there is a gap between these practices and the content of our metaphysical modal assertions, as we believe there is, it appears that the (counterfactual) account needs to be supplemented by various principles, rules, tenets, etc. This reflects on the nature and content of philosophical notions, as it seems that one may not be able to endorse an extreme externalist account of philosophical expressions and concepts, of the kind Williamson favours, and at the same time draw out a substantial epistemology of these notions, as a robust interpretation of metaphysical modal truth seems to require.

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A DYNAMIC NEO-REALISM AS AN ACTIVE EPISTEMOLOGY FOR SCIENCE

Session 12I

Congress section(s): B1

In this talk I defend a dynamic epistemic neo-realism (DEN). One important difference between DEN and the traditional no-miracles account of realism is that the determining factors for epistemic commitment to science (i.e. belief in empirical and theoretical knowledge) lie in the active nature of the processes of science as opposed to NMR's focus on the logical properties of science's products.
I will focus on two factors of DEN in my discussion. First, I propose an explosion of the dichotomy between realism and anti-realism ruling the current realist debate. (See critiques of this dichotomy in various forms in e.g. McMulllin (1984), Stein (1989), and Kukla (1994).) The explosion I suggest results in a continuum of (neo-) realist stances towards the epistemic content of theories which rests on two motivations: (1) Depicting epistemic commitment to science in terms of a dichotomy between anti-realism and realism is inadequate, as, given the trial-and-error nature of science, most of science happens on a continuum between these stances. (2) Epistemic commitment to science need not (primarily) depend on the (metaphysical) truth of science's ontological claims, but is better determined on pluralist, functional, and pragmatic grounds. This position is not the same as Arthur Fine's (1984) natural ontological attitude. I advocate a continuum of epistemic ('neo-realist') stances as opposed to one 'core' one. Rather than imploding the realist/anti-realist dichotomy, I differentiate and refine it into a continuum of epistemic commitment.
Secondly – and the main focus of this talk – I offer a critical reconsideration of the three traditional - metaphysical, semantic and epistemic - tenets of traditional scientific realism from the perspective of DEN. Specifically the traditional versions of the semantic and epistemic tenets have to be re-interpreted in the light of the suggested continuum of neo-realist stances. Reference has to be re-conceptualised as an epistemic tracking device and not only (or at all, perhaps) as an indicator of ontological existence, while the concept of truth has to be 'functionalised' in Peirce's (1955) sense of truth-as-method with some adjustments to a traditional convergent view of science.
In conclusion, the account of neo-realism defended here is a fallibilist epistemology that is pragmatist in its deployment of truth and reference and pluralist in its method of evaluation. In its naturalised tracing of science, it explains the progress of science as the result of intensive time-and-context-indexed science-world interaction.

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ŁUKASIEWICZ'S CONCEPT OF ANTI-PSYCHOLOGISM

Session 12M

Congress section(s): A2, A4

Although Łukasiewicz was the first proponent of Husserl's anti-psychologism in the Lvov-Warsaw School, his later concept of anti-psychologism has some features that are incompatible with Husserl's concept. In his famous book, Husserl declared anti-psychologism as the view that laws of logic are not laws of psychology and consequently logic is not a part of psychology. The distinction between logic and psychology is based on the difference between axiomatic and empirical sciences. Logic is an axiomatic science and its laws are settled but psychology is an empirical science and its laws derive from experience. The laws of logic are settled in an ideal world and as such are independent of experience and apodictic. Łukasiewicz supported Husserl's views in a short paper "Teza Husserla o stosunku logiki do psychologii" that appeared in 1904 and later also in his famous paper "Logic and Psychology" which was published in 1910. At the same time, Łukasiewicz, however, started to question the fact that the laws of logic are settled, which was an essential part of anti-psychologism for Husserl. Łukasiewicz questioned the law of contradiction in his book On Aristotle's Law of Contradiction and finished his denial of the unchangeability of laws of logic by an introduction of his systems of many-valued logic. His first system, the three-valued logic, is clearly based on the denial of the law of bivalence.
In his later works, Łukasiewicz questioned also the distinction between axiomatic and empirical sciences and the truthfulness of apodictic statements, which was another important component of Husserl's anti-psychologistic argumentation. Nonetheless, he still claimed that psychologism is undesirable in logic in his later works. As he did not hold certain features of anti-psychologism that were essential for the concept of Husserl that he adopted at first, it seems that his own concept of anti-psychologism differed. Frege is the most prominent representative of anti-psychologism of that time, who was also appreciated by Łukasiewicz. It seems, however, that Łukasiewicz was also inspired by other logicians from the history of logic as Aristotle and certain medieval logicians. The aim of my talk is to provide the definition of Łukasiewicz's anti-psychologism.
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DECLARATIVE AND PROCEDURAL KNOWLEDGE: A RECENT MUTATION OF THE THEORY/PRACTICE DUALITY AND ITS SIGNIFICANCE IN THE ERA OF COMPUTATIONAL SCIENCE

Session 8D

Congress section(s): C8

The distinction between procedural and declarative knowledge takes shape in the early 70's (cf. Winograd 1975), in the context of the nascent cognitive sciences and artificial intelligence programs. While it certainly has some likeliness with Ryle's distinction between know-how and know-that (Fantl 2017) it also displaces and attenuates the opposition between the two traditional poles of knowledge. Both declarative and procedural knowledge are explicit and formal; the tacit, pre-linguistic, corporeal dimension of 'know-how' dear to Ryle having almost disappeared, they are seen as complementary: one cannot proceed without data, and data is meaningless without any guidance on its correct interpretation. (Simon 1984) concludes to their interchangeability: their mutual balance is set by pure computational efficiency considerations: having few procedures and lots of data may make sense in some contexts, while the reverse may be true in others.
We would like in this communication to trace back the history, significance and posterity of this relatively recent mutation of the theory / practice opposition. Grounded in a computational paradigm of knowledge, it tends to consider this opposition as technical rather than philosophical. Concepts are viewed as coding procedures of information, mediating inputs and outputs (e.g. (Dretske [1981] 1999) and representations as results of data compression. In the context of computational science (Humphreys [2004] 2007) and big data, this clearly tends to blur the distinction between theoretical science and technology: computerized procedures such as simulations are increasingly becoming the bread and butter of science, and some view the function of scientific theories as simple means to efficiently represent data about the world e.g. (Anderson 2008). Conversely, technology and engineering, being also computerized e.g. through Computer Aided Design (CAD), have never been so scientific and formal and away from their roots in the 'know-how' of the traditional crafts.
Could these trends be confirmation that the theory/practice opposition is outdated, at least in the realm of scientific knowledge? And that we should get rid of old, implicit representations of science as culminating in theoria i.e. contemplation? The pragmatist school of the first half of the 20th century did issue similar claims (e.g. cf. Hickman 1992): we will ask whether it anticipates and prepares the emergence of the computational paradigm. If so, the pragmatist epistemology, which is today coming back in force, could be seen as bridging Ryle's know-how with computational science's procedural stance. We will aim to show that the concept of 'method' is fundamental in this shift, morphing into related notions such as 'procedure' or 'algorithm', progressively extending the middle ground between theory and practice, and eventually conquering the central stage in epistemology. Herbert Simon, who put 'design' at the center of this new conception of science and technology, will appear as a pivotal figure in this debate. As a conclusion, we will advance some hypotheses about the limits of such a conception of science and theory.
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INDUCTIVE INFERENCE AND STRUCTURES: HOW TO LEARN EQUALITY IN THE LIMIT

Session 29M

Congress section(s): A1, A2, B2

(Joint work with Ekaterina Fokina and Timo Kötzing)
Algorithmic learning theory (ALT) is a vast research program, initiated by Gold and Putnam in the 1960's, that comprises different models of learning in the limit. It deals with the question of how a learner, provided with more and more data about some environment, is eventually able to achieve systematic knowledge about it. Classical paradigms of learning concern either learning of formal languages or learning of total functions.
In this work, we want to make sense of the following question: what does it mean to learn a structure? To do so, we combine the technology of ALT with notions coming from computable structure theory, and develop a formal framework for learning structures in the limit. We focus on the following case-study.
Consider a learner observing (a countably infinite number of) different items to be equivalent or not equivalent. The learner would like to arrive at a conjecture about the structure of this equivalence relation, that is, the learner would like to determine the isomorphism type of the equivalence structure embodied by the items. If the first guess has to be correct, we call the setting finite learning (denoted Fin), if the conjecture may be changed an arbitrary (but finite) number of times before stabilizing on a correct conjecture, we call the setting explanatory learning (denoted Ex). In each case, the data available to the learner is a complete accurate list of which elements of the structure are equivalent and which are not. Following standard convention in ALT, we call this learning from informant (Inf), where both positive and negative information is available.
We carefully distinguish many learning criteria and completely characterize which equivalence structures are learnable according to the (arguably) most natural one. We also discuss the philosophical significance of this approach and its relation with induction.

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THE INCOMPLETENESS OF EXPLANATORY MODELS OF ABDUCTION IN DIAGNOSIS: THE CASE OF MENTAL DISORDERS

Session 30D

Congress section(s): C4

Abduction is known as "procedure in which something that lacks classical explanatory epistemic virtue can be accepted because it has virtue of another kind (Gabbay and Woods: 2005, Magnani: 2017). In classical explanations this lack implies that specific explanation model of abduction should be considered as special case of abduction. That is because to be an explanation implies something more than to be an abduction: a conclusion, in the sense that the burden of proof falls on the abductive product. To have a conclusive form means that explanation and the theory that needed it are already attuned and, of course, this case eliminate the possibility of accept something because it has virtue of another kind. It is interesting to note that this causal transformation is the cause of the confusion between the explanation model of abduction and inference to the best available inference, which it is also known IB(A)E. On the other hand, the difference between each other is the role of the conclusion.

This last point is important because the special case of explanatory abduction is also suitable to conceptualize medical diagnosis, while IB(A)E not. The reason is that medical diagnosis is only possible if the relation with the medical knowledge of the doctor is tentative. This is, only if there is the lack that abduction implies. In other words, the causality form of abduction is substantially different than IB(A)E because diagnosis needs a virtue of another kind for being accepted (Aristotle: Rh, I, 1355b10-22).

However, the other face of this situation is that the specific and causal form of explanatory abduction is only useful in specific medical diagnosis: in the cases where it is possible to draw a causal course of facts as Neurology (Rodríguez, Aliseda and Arauz: 2008). I want to use this last medical area as example because in it is possible to see one mechanism for do it diagnosis around brain problems. From this medical area I want to do a contrast with another medical area, which studies the brain too but from a different point of view: Psychiatry. When trying to explain psychiatry diagnosis through classical explanatory abduction is possible to see that there is something wrong. One the one hand, the generalization from enumeration is more difficult than other kind of medial areas and, on the other hand, here is more visible that a difference between simple diagnosis and diagnosis plus prescription is needed in the characterization of abduction.

The reason is that abduction is one form of human reasoning and if there is one area which diagnosis does not has causal dependency, then it is possible that classical explanatory model of abduction: a) is a more specific kind of diagnosis (some part of general abduction) or b) diagnosis needs something more for their good conceptualization. I want to try to defend b from an analysis of EC-Model of abduction in which I try to defend the necessity to imply moral values.

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GROUNDING NUMERALS

Session 3F

Congress section(s): C5

The study of our innate numerical capacities has become an active area of recent cognitive research. Given that these capacities appear to be very limited, it is widely assumed that the use of external aids—such as numeral systems—expands our natural ability to reason about numbers. In fact, people have identified arithmetic as an important case of the use of external aids in thinking, but the question of how these "thinking tools" acquire numerical content remains unsettled. After all, written numerals (say) are material inscriptions that—under some suitable interpretation—could stand for anything whatsoever. What constrains the range of available interpretations so that these otherwise meaningless symbols can achieve their representational aims?

Extant accounts either pull the relevant content out of thin air or make it parasitic on some antecedently available interpretation. On pain of circularity or regress, we have to explain how numerals come to represent what they do without relying on some prior—and mysterious—grasp of their intended interpretation.

I will start with the recognition that numeral symbols, in and of themselves, do not represent anything at all. In isolation, they are representationally idle. It is only by being embedded in broader systems of representation that these symbols acquire numerical content. Numeral systems, I suggest, have distinctive features that relate individual symbols to one another and thereby constrain their representational content.

This, however, still doesn't uniquely determine the system's representational target. Our familiar decimal base system, for instance, can stand for linear sequences but it can also stand for circular ones, depending on the case at hand. Thus, I will further argue that systems of numerical representation, in turn, need to be grounded in specific cultural practices, which govern their use and are carried out by agents naturally equipped to exploit some of their distinctive (structural) features. I will illustrate these claims by means of a case study involving different numeral systems (such as tallies and positional systems) and the practices in which they are deployed (most notably, counting and calculation).

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ATTACK AT DAWN IF THE WEATHER IS FINE

Session 8L

Congress section(s): A2

The present paper is an attempt to undertake a critical estimation of several issues connected with a criterion of validity for imperative inferences. Imperatives are neither true nor false. But they have a value analogous to indicatives, namely, compliance-value or. Josh Parsons raises the issue of content-validity as a criterion of imperative inference. An argument is content-valid iff the contents of the premises jointly entail the content of the conclusion. This criterion has been illustrated in terms of possible worlds (Parsons has introduced the term 'preposcription' here). This theory however, ignites certain problems which require discussion. Following are some of the reflections.

1. According to Parsons, indicatives and imperatives both have propositions as their contents. But according to Speech Act theory it is not the 'proposition', but the 'propositional act' (reference + predication) which is the common element that an indicative and imperative may possibly share. An imperative sentence is better understood in connection with some action.

2.The insufficiency of standard view in respect to content-validity has been elaborately shown by Parsons. Some the 'evil twins' of some content-valid arguments are found to be intuitively invalid.

Example. 1

(A1) Attack at dawn if the weather is fine!

(A2) The weather is fine.

Therefore, (A3)Attack at dawn!

Example. 2 [Evil Twin]

(B1)Attack at dawn if the weather is fine!

(B2) Let the weather be fine!

Therefore, (B3) You attack at dawn.

Symbolic form of example 1 and 2 :

Argument A: $I(p \rightarrow q)$

$A(p)$

Therefore,$I(q)$

Argument B: $I(p \rightarrow q)$

$A(p)$

Therefore, $I(q)$

The symbolic form of example 2 is a matter of worry which is evident from the following:

The weather is fine (antecedent of the conditional proposition)

Let the weather is fine (an independent proposition which is not the antecedent)

3. The concept of imperassertion includes the notion of belief and intention. Possible worlds are thought to be linked with a person's belief-intention-set. But no inconsistency can be presupposed , i.e., no question of considering the imperassertion and belief of the person making a command along with the hearer's lacking the intention to carry out the command. It is to be noted that in case of argument having one conditional imperative proposition as a premise, the "success" of an imperassertion is only to be presupposed as true.

The upshot of the whole discussion leads to a serious observation. In case of a sentence "Attack at dawn if the weather is fine" only compliance and non-compliance are not enough to project all possible worlds. It requires another world to exhibit the possibility of the weather being so so. It proves the inadequacy of Parson's theory.

The concept of imperassertion as found in Parsons' writing is praiseworthy in tying the knot among logic, theory of mind and theory of action. Perhaps it may help unearthing layers of the logic of imperative.

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ON A STRUCTURALIST VIEW OF THEORY CHANGE:STUDY OF SOME SEMANTIC PROPERTIES IN FORMAL MODEL OF BELIEF REVISION

Session 25K

Congress section(s): B2

Formal models of belief revision throw light on understanding the scientific theory change in general, and in particular the distinction between accidental and radical (revolutionary) theory change. In this paper we present a structuralist framework of belief revision. This is considered to be an extension of AGM framework of belief revision with some semantic properties which constitute the structure. The properties are causal properties, causal mechanism, causal process, intervention. In the theories of belief revision based on classical AGM [AGM:1985], beliefs are being represented as a set of sentences that are closed under logical implication. Operations for belief change such as expansion, contraction and revision, are defined only with respect to belief sets. As a result, structural information given by an epistemic entrenchment ordering disappears after operations of belief change. Although the AGM model is mathematically elegant, simple, it only merely serve as an idealized model for belief change. Also, some of the postulates such as success(in case of revision), recovery (in case of contraction) are found to be unsatisfactory in case of rational belief revision. Success Postulate states that new information needs to be reflected in the revised belief state of an agent, no matter whether it is relevant or not. Hence, an agent has no choice of rejecting the undesirable irrelevant information. Some of the other postulates are also found to be controversial, counter-intuitive. Belief revision models based on AGM are geared up for one step revisions of simple plain beliefs and fails to handle cases where the new information comes in iterations and in the form of conditional beliefs. In AGM all conditional beliefs have same informational value as they are structurally 44 similar and hence there is no difference between laws and accidental generalizations.
Keeping the above issues in mind the problem we are addressing is as follows: how does an agent after discovering causal relations from the environment revises her beliefs such that beliefs that are close to the core are not lost? The problem lies in finding a mechanism with which one can give up conditional beliefs in the process of Belief revision while preserving the core conditional beliefs such as laws or causal statements.
Following the constructive approach to belief revision, we propose a new entrenchment ordering called “causal epistemic entrenchment ordering”, which is constrained by the structural and semantic properties such as structure, intervention, causal properties, causal mechanisms. We show how the resulting ordering of beliefs are influenced by these semantic factors. The resulting entrenchment ordering of beliefs (CEE) would help us in ordering conditional beliefs.
2. preservation of structure in the iterated revisions 3. It can be applied to the scientific theory change.
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COGNITIVE AND EPISTEMIC FEATURES: A TOOL TO IDENTIFY TECHNOLOGICAL KNOWLEDGE

Session 6H

Congress section(s): B3, C8

Is what engineers and applied scientists are looking for in their inquiries a specific kind of knowledge? Is there something as a genuine technological knowledge, distinct from an epistemic knowledge? At least three kind of answers have been made to address this issue:
– indistinction between them: from the general analysis of techno science (all knowledge are determined by technological instruments) to a radical pragmatism (all knowledge can be reduced in fine to action);
– emancipation of the technological knowledge: the scientific knowledge, based on the contemplative idea of a true belief with reasons, is distinct from the technological knowledge, based on prescriptive rules (Bunge 1964);
– distinction between degrees: there is something slightly distinct between these two forms of knowledge, but it is rather in varying degrees between common features (e.g. Meijers & Kroes 2013).
In this presentation, I propose to continue the “distinction between degrees” approach, by mobilising concepts from the debates around epistemic values. I defend the following thesis: this distinction between technological and epistemic knowledge must not be drawn between two kinds of knowledge, but rather between epistemic (linked with contemplative knowledge) and cognitive (values unnecessary but linked to epistemic elements, such as fruitfulness, breadth, etc.) features. This perspective is grounded in the fruitful distinction launched by Laudan 1994 and developed by Douglas 2013 between cognitive and epistemic values, which sheds lights of epistemological differences that lead to the choice of theories, concepts, laws, etc.
Together with this theoretical clarification, an empirical survey into the field of nanotechnology allows me to describe more closely the importance and the diversities of cognitive features such as control of a phenomenon, characterization of a surface, functionalisation of a material, etc. Indeed, such objectives cannot be considered as epistemic in a narrow sense (one can control something without knowing it) but they cannot be considered as well as purely applied objectives (they cannot be reduced to practical features). Analysing them as cognitive features can be an interesting way to avoid both the radical distinction of two forms of knowledge and the blurring of these distinctions.
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THE FUNDAMENTALITY OF FIELDS

Session 20I

Congress section(s): C2

In the quantum field theory of quantum electrodynamics, photons and electrons are generally treated quite differently. For photons, we start with the classical electromagnetic field and then quantize it. For electrons, we start with classical particles, then move to a relativistic quantum description of such particles (using the Dirac equation) before taking the final step from relativistic quantum mechanics to quantum field theory. In brief, photons are treated as a field and electrons as particles. There are a number of reasons to be unhappy with this inconsistent approach. We can achieve consistency either by treating photons as particles or electrons as a field (the Dirac field). In this talk, I will evaluate both of these strategies and argue that

the second strategy is preferable to the first. Thus, we should view the electromagnetic and Dirac fields as more fundamental than photons or electrons.
The first strategy for finding consistency requires one to develop a relativistic quantum mechanics for the photon. One can attempt to construct such a theory by reinterpreting the electromagnetic field (when it is sufficiently weak) as a quantum wave function and finding a Schrödinger equation in Maxwell's equations. Around 1930, Majorana and Rumer suggested a way of doing this: take the complex vector field formed by summing the electric field with i times the magnetic field to be the wave function for the photon. However, this is not an acceptable wave function: its amplitude-squared is not a probability density. Good (1957) proposed a more complicated but better motivated way to form a photon wave function from the electric and magnetic fields. Unfortunately, his wave function is not suitably relativistic to be part of a relativistic quantum mechanics for the photon.
The second strategy is to treat electrons as a field, not particles. This involves first interpreting the Dirac field as a classical field, not a quantum wave function, and then quantizing that field. Such an approach yields a classical picture in which the mass and charge of electrons are spread out, not concentrated at points. This spread-out charge distribution serves as a more sensible source for the electromagnetic field than troublesome point charges. The spread of mass and charge also helps us understand electron spin. Analyzing the distribution and flow of mass and charge in the Dirac field allows us to see the classical electron as truly spinning and provides a new explanation of the electron's gyromagnetic ratio.
Reference:
Good, Roland H., Jr. 1957. Particle Aspect of the Electromagnetic Field Equations.
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THE INTERACTION BETWEEN DIAGRAMS AND COMPUTERS IN THE FIRST PROOF OF THE FOUR-COLOR THEOREM

Session 7C

Congress section(s): C1

The use of diagrams and the use of computers are two significant themes within the philosophy of mathematical practice. Although case studies concerning the former are abundant – from the notorious case of Euclidean geometry to the uses of diagrams within arithmetic, analysis, topology, knot theory, and even Frege's Begriffschrift –, the latter has received less attention in the field.
I show in my talk how the two themes can be investigated simultaneously via an analysis of the famous case of the Four-Color Theorem (4CT). Whenever the use of computers in mathematical practice is considered, the computer-assisted proof of the 4CT is mentioned. Philosophical discussion of the proof has centered mostly on Tymoczko's argument for the introduction of experimentation in mathematics via 4CT – notably made in (Tymoczko 1979). (See Johannsen & Misfeldt 2016 for a recent version of this position.)
In previous work, I revised central leitmotifs in rejoinders presented against Tymoczko's claims, arguing from a Wittgensteinian perspective that the 4CT is relevant to contemporary discussions on the use of computers in mathematics (especially in Author 2017). Aiming a discussion about the criteria for the identity of computer-assisted proofs through an examination of the various proofs of the 4CT, in my talk, I will show the main lines of articulation between the more than 3000 diagrams and the computational machinery mobilized in the construction and the verification of Appel and Haken first version of the proof.
After presenting the way diagrams and computers participate in the proof, dealing with the passage from topology to combinatorics operated in it, my primary strategy consists in projecting the methodological contribution recently suggested by De Toffoli – namely, the three criteria she proposes as tools for evaluating the effectiveness of mathematical notations (expressiveness, calculability, and transparency; cf. De Toffoli 2017) – into the case of Appel and Haken's proof of the 4CT. In so doing, I will specify the ways in which the diagrams of this case study can be considered a perspicuous mathematical notation, as well as to propose some questions regarding the way this notation is related to the computational devices indispensable to the proof.

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SUBSTRUCTURAL PROPOSITIONAL DYNAMIC LOGIC

Session 29D

Congress section(s): A2, A3

Propositional dynamic logic, PDL, is a well known modal logic with applications in formal verification of programs, dynamic epistemic logic and deontic logic, for example [2]. More generally, PDL can be seen as a logic for reasoning about structured actions modifying various types of objects; examples of such actions include programs modifying states of the computer, information state updates or actions of agents changing the world around them.
In this contribution we study a version of PDL where the underlying propositional logic is a weak substructural logic in the vicinity of the full distributive non-associative Lambek calculus with a weak non-involutive negation. Our main technical result is a completeness proof for the logic with respect to a class of modal Routley-Meyer frames. The motivation for this endeavor is to provide a logic for reasoning about structured actions that modify situations in the sense of Barwise and Perry [1]; the link being the informal interpretation of the Routley-Meyer semantics for substructural logics in terms of situations [3]. In the contribution we inform on our partial progress in this area (the version of the Lambek calculus used in our paper is weaker than the logic related to situation semantics in [3]) and comment on the problems that need to be solved.
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TAKING A MACHINE AT ITS WORD: APPLYING EPISTEMOLOGY OF TESTIMONY TO THE EVALUATION OF CLAIMS BY ARTIFICIAL SPEAKERS

Session 13F

Congress section(s): C8

Despite the central role technology plays in the production, mediation, and communication of information, formal epistemology regarding the influence of emerging technologies on our acquisition of knowledge and justification of beliefs is sparse (Miller & Record 2013), and with only a couple exceptions (Humphreys 2009; Tollefsen 2009) there has been almost no attempt to directly apply epistemology of testimony to analyze artifacts-as-speakers (Carter & Nickel 2014). This lacuna needs to be filled. Epistemology of testimony is concerned with identifying the conditions under which a hearer may be justified in trusting and forming beliefs based on a speaker claims. Similarly, philosophers of technology and computer scientists alike are urgently pushing to ensure that new technologies are sufficiently explainable and intelligible to appropriately ground user understanding and trust (Tomsett et al. 2018; Weller 2017). Given the convergent goals of epistemologists and philosophers of technology, the application of epistemology of testimony to the evaluation of artifact speakers may be incredibly productive. However, we must first determine whether an artifact may legitimately hold the role of 'speaker' in a testimonial relationship. Most epistemologist assume that testimonial speakers are intentional, autonomous agents, and methods for evaluating the testimonial claims of such agents have developed accordingly making technology difficult to slot into the conversation.
In this paper I demonstrate that epistemology of testimony may be applied to analyze the production and transmission of knowledge by artificial sources. Drawing on Gelfert (2014) I first argue, independently of my goal to apply testimony to technology, that our current philosophical conception of testimony is ill-defined. I then differentiate between the theoretical and pragmatic aims of epistemology of testimony and argue that the pragmatic aim of epistemology of testimony is to provide tools for the evaluation of speaker claims. I explicate a more precise ‚continuum view' of testimony that serves this pragmatic aim, and conclude by describing how the explicated continuum view may be usefully and appropriately applied to the evaluation of testimony from artificial speakers.

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PROGRAM OPTIMISATION THROUGH PROOF TRANSFORMATION

Session 26C

Congress section(s): A1

In earlier work it has been shown that the well-known DPLL SAT solving algorithm can be extracted from a soundness and completeness proof of the corresponding proof system. We carry this work further by showing that also program optimisation techniques such as clauselearning can be obtained by a transformation on the proof level.

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DE FINETTI MEETS POPPER OR SHOULD BAYESIANS CARE ABOUT FALSIFICATIONISM?

Session 14C

Congress section(s): B6

Views of the role of hypothesis falsification in statistical testing do not divide as cleanly between frequentist and Bayesian views as is commonly supposed. This can be shown by considering the two major variants of the Bayesian approach to statistical inference and the two major variants of the frequentist one.
A good case can be made that the Bayesian, De Finetti, just like Popper, was a falsificationist. A thumbnail view, which is not just a caricature, of De Finetti's theory of learning, is that your subjective probabilities are modified through experience by noticing which of your predictions are wrong, striking out the sequences that involved them and renormalising.
On the other hand, in the formal frequentist Neyman-Pearson approach to hypothesis testing, you can, if you wish, shift conventional null and alternative hypotheses, making the latter the straw-man and by 'disproving' it, assert the former.
The frequentist, Fisher, however, at least in his approach to testing of hypotheses, seems to have taken a strong view that the null hypothesis was quite different from any other and there was a strong asymmetry on inferences that followed from the application of significance tests.
Finally, to complete a quartet, the Bayesian geophysicist Jeffreys, inspired by Broad, specifically developed his approach to significance testing in order to be able to 'prove' scientific laws.
By considering the controversial case of equivalence testing in clinical trials, where the object is to prove that 'treatments' do not differ from each other, I shall show that there are fundamental differences between 'proving' and falsifying a hypothesis and that this distinction does not disappear by adopting a Bayesian philosophy. I conclude that falsificationism is important for Bayesians also, although it is an open question as to whether it is enough for frequentists.

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MODELLING MINIMALISM AND TRIVIALISM IN THE PHILOSOPHY OF MATHEMATICS THROUGH A NOTION OF CONCEPTUAL GROUNDING

Session 17K

Congress section(s): C1

Minimalism [Linnebo 2012, 2013, 2018] and Trivialism [Rayo 2013, 2015] are two forms of lightweight platonism in the philosophy of mathematics. Minimalism is the view that mathematical objects are thin in the sense that "very little is required for their existence" [Linnebo 2018, 3]. Trivialism is the view that mathematical theorems have trivial truth-conditions, in the sense that "nothing is required of reality in order for those conditions to be satisfied" [Rayo 2013, 98]. Minimalism and trivialism can be developed on the basis of abstraction principles, universally quantified biconditionals stating that the same abstract corresponds to two entities of the same type if and only if those entities belongs to the same equivalence class (e.g. Hume's Principle, HP, which states that the cardinal number of the concept F is identical to the cardinal number of G if and only if F and G can be put into one-to-one correspondence; cf. Frege [1884, § 64]). The minimalist claims that the truth of the right-hand side of HP suffices for the truth of its left-hand side. The trivialist claims that for the number of F to be identical with the number of G just is for F and G to stand in one-to-one correspondence. Moreover, the minimalist and the trivialist alike submit that the notion of the notion of sufficiency, on one side, and the 'just is'-operator, on the other, cannot be identified with, or are not be interpreted as, a species of grounding or metaphysical dependence. More precisely, Linnebo [2018, 18] requires that "any metaphysical explanation of [the right-hand side] must be an explanation of [the left-hand side], or at least give rise to such an explanation"; the notion of grounding, by contrast, would fail to provide an explanation where one is required. Rayo [2013, 5] argues that 'just is'-statements are not understood in such a way that "[they] should only be counted as true if the right-hand side 'explains' the left-hand side, or it is in some sense 'more fundamental'"; the notion of grounding, by contrast, would introduce an explanation where none is expected. In this paper we argue that both minimalism and trivialism can be modelled through an (appropriate) notion of grounding. We start off by formulating a 'job description' for the relevant notion(s). As for minimalism, grounding must be both non-factive – viz. a claim of grounding, 'A grounds B', must not entail that either A or B are the case – and non-anti-reflexive – viz., it must not be the case that for any A, it is not the case that A grounds A. As for trivialism, the relevant notion of grounding must be non-factive and reflexive – viz., for any A, it is the case that A grounds A. Alternatively, trivialism can be formulated by introducing the notion of portion of reality, consisting in a (possibly fundamental) fact and whatever is grounded by that fact, and arguing that the two sides of a 'just is'-statement represent (different) facts belonging to the same portion of reality. We then suggest some definitions of both the minimalist's notion of sufficiency, on one side, and of the trivialist's 'just is' operator, on the other, in terms of (weak) ground. Finally, we point out that a suitable elaboration of the notion of conceptual grounding [Correia & Schnieder 2012, 32], which takes into account the relations of priority among the concepts by which the two sides of HP are described, effectively responds to Linnebo's and Rayo's objections.

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A ROUNDABOUT TICKET TO PLURALISM

Session 13L

Congress section(s): C1

A thriving literature has developed over logical and mathematical pluralism (LP and MP, respectively) – i.e. the views that several rival logical and mathematical theories can be correct. However, these have unfortunately grown separate; we submit that, instead, they both can greatly gain by a closer interaction.
To show this, we present some new kinds of MP modeled on parallel ways of substantiating LP, and vice versa. We will use as a reference abstractionism in the philosophy of mathematics (Wright 1983). Abstractionists seek to recover as much mathematics as possible from abstraction principles (APs), viz. quantified biconditionals stating that two items have the same abstract just in case they belong to the same equivalence class; e.g. Hume's Principle (HP), which states that two concepts have the same cardinal number iff they can be put into one-to-one correspondence (Frege 1884, §64). The proposed new forms of pluralism we will advance can fruitfully be clustered as follows:
1. CONCEPTUAL PLURALISM – From LP to MP: Just as LPs argue that different relations of logical consequence are equally legitimate by claiming that the notion of validity is underspecified (Beall & Restall 2006) or polysemous (Shapiro 2014), abstractionists might deem more than one version of HP acceptable by stating that the notion of "just as many" – and, consequently, of cardinal number – admits of different precisifications.
2. DOMAIN PLURALISM – From MP to LP: Just as MPs claim that rival mathematical theories can be true of different domains (Balaguer 1998), it could be argued that each version of HP introduces its own domain of cardinal numbers, and that the results these APs yield might differ with respect to some domains, and match with respect to some others (e.g., of finite and infinite cardinals). The proposal, in turn, prompts some reflections on the sense of "rivalry" between the logics accepted by LPs, which often agree on some laws, while diverging on others. Is the weaker logic genuinely disagreeing or just silent on the disputed rule? Do rival logicians employ the same notion of consequence in those rules about which they agree or, given some inferentialist view, always talk past each other?
3. CRITERIA PLURALISM – From LP to MP, and back: Another form of pluralism about abstractions could be based on the fact that more than one AP is acceptable with respect to different criteria (e.g. irenicity, conservativity, simplicity); accordingly, LP has so far been conceived as the claim that more than one logic satisfies a single set of requirements, but a new form of LP could arise from the acceptance of several legitimacy criteria themselves (e.g. compliance with our intuitions on validity, accordance with mathematical practice).
These views – besides, we will argue, being in and of themselves attractive – help expanding and clarifying the spectrum of possibilities available to pluralists in the philosophy of both logic and mathematics; as a bonus, this novel take can be shown to shed light on long-standing issues regarding LP and MP – in particular, respectively, the "collapse problem" (Priest 1999) and the Bad Company Objections (Linnebo 2009).
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REVISING LOGIC: ANTI-EXCEPTIONALISM AND CIRCULARITY

Session 10B

Congress section(s): C1

According to anti-exceptionalism (AE) about logic, (i) logical laws do not have any epistemologically or metaphysically privileged status; in particular, they are neither a priori, nor analytic, nor necessary. Rather, (ii) logical theories should be justified, revised and compared just as scientific ones are – that is, via an abductive methodology.
I will first try to clarify claim (i), by reviewing which properties AEs think logical laws should be deprived of. It will turn out that there is a substantial disagreement on what logic (allegedly) cannot be, the only agreed upon feature being non-apriorism; furthermore, it seems that AEs use 'a priori/a posteriori' in an unspecific sense, in that they do not make reference to empirical kinds of evidence, but rather equate non-aprioricity with revisability. I will then move on to (ii), and try to unpack the abductive methodology and its criteria. In order to do this, I will first review the main implementations of the AE model – namely, Priest's (2016) and Williamson's (2017); secondly, I will take a closer look at the abductive criteria, and in particular at the most prominent among them, that of adequacy to data.
I will then advance some objections to the AE view, which stem from a well-know argument in the philosophy of logic, i.e., the Centrality Argument (CA; e.g. Putnam 1978). According to CA, logical laws are so central in rational reasonings that any attempt either to revise or to justify them ends up using those laws themselves and, so, winds up being illegitimate. I will build some forms of CA that are specifically targeted against the AE account, and show that the latter is at several levels threatened by circularities, both when evaluating theories with respect to given abductive criteria, and when performing the general computation.
Hence, though AEs are right in claiming that logical theories have often been revised through an abductive methodology, their account faces some serious (broadly definable) metatheoretical objections. I will conclude by proposing a way of reconciling these two opposing cases, which resorts to Priest's (2014) distinction between logica docens and logica ens – that is, between what logicians claim about logic, and what is actually valid. I will argue that AEs seem to submit only that logica docens is revisable, while remaining silent on logica ens' fate; on the other hand, a minimal form of CA shows only that we can neither revise nor justify the laws of the correct logic – i.e., of logica ens – whatever this logic is. Hence, some compatibility can be worked out, at least between modest versions of the two opposing positions.
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ON THE FORKING TOPOLOGY OF A REDUCT OF A SIMPLE THEORY

Session 29J

Congress section(s): A1

Abstract. Let T be simple and T^- a reduct of T. For variables x, we call an $\emptyset$-invariant set $\Gamma(x)$ of with the property that for every formula $\phi^-(x,y) \in L^-$: for every a, $\phi^-(x,a)$ L^--forks over $\emptyset$ iff $\Gamma(x) \wedge \phi^-(x,a)$ L-forks over $\emptyset$, a *universal transducer*. We show that there is a greatest universal transducer $\tilde{\Gamma}_x$ (for any x) and it is type-definable. In particular, the forking topology on $S_y(T)$ refines the forking topology on $S_y(T^-)$. Moreover, we describe the set of universal transducers in terms of certain topology on the Stone space and show that $\tilde{\Gamma}_x$ is the unique universal transducer that is L^--type-definable with parameters. In the case where T^- is a theory with the wnfcp (the weak nfcp) and T is the theory of its lovely pairs we show $\tilde{\Gamma}_x = (x = x)$ and give a more precise description of all its universal transducers in case T^- has the nfcp.

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THE COMPETITION OF INTERESTS IN PUBLIC SCIENTIFIC KNOWLEDGE PRODUCTION. AN ANALYSIS OF CHINESE CASE

Session 8G

Congress section(s): B5

Public scientific knowledge refers to the kind of scientific knowledge that is used as the basis of fact judgement in public policy-making, typically characterized by scientific rationality, economic sharing and political legitimacy. This kind of knowledge originates from the scientific knowledge produced by the scientific community, but get formed and reproduced in the public policy-making process through the negotiation of all participants. The production of public scientific knowledge and public policy-making is the identical process. Through the analysis of public scientific knowledge production in China's safety assessment decision on GM Crops since the 1990s, this paper discovers that the competition of different interests is inevitable for all participants. Among all the interests competition, competing for material interests is an essential one, such as competing for research funds, high-value crops, and so on; while discourse competition are more crucial, participants striving to manipulate public policy-making through the control of the safety discourse on GM Crops. The competition has the following four characteristics: 1. all participants strongly support the principle of democracy. 2. confined and limited by specific state systems, not all interests-related parties can become participants. 3. participants sometimes practice non-normative ways and means of participation. 4. government officials have the overwhelming authority, but they sometimes have confusions on choosing whose interests to represent for, causing constant disputes or even disruption of negotiation. Therefore, this paper comes to the conclusion that there exists competition of interests in the public knowledge production process and the key for solving the competition lies on choosing appropriate participants under different state systems.

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FEYERABEND'S WELL-ORDERED SCIENCE: HOW AN ANARCHIST DISTRIBUTES FUNDS

Session 6G

Congress section(s): B1, B6

To anyone vaguely aware of Feyerabend, the title of this paper seems oxymoronic. For Feyerabend, it is often thought, science is an anarchic practice with no discernible structure. Against this trend, I argue that Feyerabend's pluralism, once suitably modified, provides a plausible account of how to organize science which is superior to contemporary accounts.
Ever since the foundation of the National Science Foundation in 1950, there has been little philosophical analysis of how resources should be distributed amongst diverse and, often, competing research programs. In Science, Truth, and Democracy, Kitcher introduces the notion of a 'well-ordered science' where he provides his understanding of how science should be organized. In a nutshell, he posits that democratic deliberations should determine which theories are pursued and how they are prioritized. Since then, others have introduced more fine-grained models that, unwittingly, make use of Kitcher's conception of a well-ordered science (Strevens 2003; Weisberg and Muldoon 2008; Zollman 2010). However, these models conflate the goals of research and the means of attaining those goals. This conflation is the result of assuming that the goals, plus our current scientific knowledge, determines the means of attaining them. For example, if a cure for cancer is a goal of research, we should increase funds for lines of research that seem 'promising' for finding such a cure where the 'promise' comes from our existing knowledge of cancer and its possible treatments (e.g., various subfields of oncology, radiation therapy, etc.). Against this, I argue that Feyerabend was correct in asserting that we should pursue theories that contradict currently accepted knowledge and appear to have no initial practical value. Therefore, the attainment of the goal (a cure for cancer) also requires funding research that conflicts with current background knowledge (e.g., music therapy) and research that appears to have nothing to do with cancer research by our current lights. In my talk, I will reconstruct the methodological argument Feyerabend provides for this view and show how it supported by the social scientific literature on theory pursuit which shows how solutions to problems came from unexpected sources (Roberts 1989; Foster & Ford 2003; McBirnie 2008).
After this, I go on to show how Feyerabend's pluralism can provide an alternative method of organizing research. Feyerabend's pluralism is essentially the combination of the principle of proliferation, which asserts that we should proliferate theories that contradict existing theories, and the principle of tenacity, which asserts that we can pursue theories despite their theoretical and empirical difficulties indefinitely (Shaw 2017). However, Feyerabend provides no means for balancing these principles and, therefore, his own well-ordered science is incomplete. I argue that this balance can come from C.S. Peirce's 'economics of discovery' which provides limits to the principles of proliferation and tenacity (cf. McKaughan 2008). I conclude by gesturing at recent work on the economics of theory pursuit that provides empirical confirmation of this view (Stephan 2012).

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SCIENTISTS' SOCIAL RESPONSIBILITIES IN THE CONTEXT OF SCIENCE COMMUNICATION

Session 28B

Congress section(s): B1, B5

This study considers the issue of scientists' social responsibilities in the context of science communication. I accentuate this question by rethinking scientists' tasks as they were outlined by Max Weber in his "Science as a vocation and profession". In the twenty-first century, researchers are increasingly focusing on the new evolving mission of a science communicator engaged in interactions with the public. First of all, it includes science popularization, the image of which is being re-actualized in the contemporary mediatized society and culture. I briefly depict the picture of science popularization in Germany of the 1920s, illustrated particularly by popular science magazines and radio programs, the emergence of which

originally had to do with the concept of Kulturnation (German: “cultural nation”). In those times, however, science popularization was not propagated as a scientist’s mission and generally proceeded within the ‚deficit‘ model. What is also important is that the necessity to disseminate scientific knowledge for lay audiences was recognized by scientists earlier than it was by the state authorities, and state support of science communication started mainly in the 20th century.
In this respect, I focus primarily on countries with accelerated modernization and an acute need in a rapid science and technology development, with the Soviet Union as an illustration. The Soviet state supported science studies and science dissemination projects – science popularization was rooted in the overall political and social ideology. Here, my point is that if somewhat reconsidered, the idea of science communication as a vocation and profession would find a partial application to the Soviet context (the image of science popularization as both invoking younger generations’ interest in scientific endeavor and maintaining the future career guidance). In his lecture, Weber differentiates between professional scientific activities, related to solving specific research objectives basing on certain methods and techniques, and science as an inner calling for the truth, inspired by the desire to know the unknowable. These two characteristics of scientists seem contradicting: professional activity is to limit their inward calling for science, and commitment to implement the “calling” is contrary to professionalism.
In the Soviet context, however, science communication tended to combining those aspects. It was the idea of “vocation” to be propagated when involving and recruiting younger generations in research activities. It might be well illustrated by the practices of the Society “Znanie” (Knowledge), established in 1947 and effectively contributed to evoking society’s interest in science and implementing the Soviet science and technology policy. In this respect, the Society fulfilled three interrelated objectives: disseminating scientific knowledge; building and propagating the heroic image of the scientist, and by that facilitating the fulfillment of a large number of technical and socio-political tasks (the Society aimed to form a scientist’s image in terms of the “vocation” – scientists were meant to make the world better); shaping a value attitude to scientific knowledge as a significant constituent of people’s common worldview and their guide in everyday practices, e.g. in solving various problems. The Society’s system included open people’s universities, museums, and libraries, “Houses of Knowledge,” publishing and printing houses, etc. Every year it gave 25 million public lectures for 280 million people throughout the USSR. It participates in radio and television programs, including the creation of popular science films.
Science popularization intensely employed popular science magazines, the target audience of which included children and young adults. The recruitment of younger generations also proceeded within scientific events for schoolchildren, school competitions, and technical groups engaging in radio electronics, automatics, biochemistry, cosmonautics, etc. All of those were carried out within a model aimed at maintaining a situation when the idea of vocation grew in a seemingly natural way into appraising the idea of profession.
The current tendency for scientists to engage in dialogue with the public is justified by the actualization of such phenomena as the mediation of social institutions and processes, the need to demystify the unequivocal negative perception of the consequences of science and technology progress in public consciousness, and the importance of public awareness regarding scientific developments. At the same time, I consider and problematize some debatable aspects of external science communication. A significant question here is who should be a communicator. Nowadays, it involves not only scientists, but also science journalists, press secretaries, and others, right up to amateurs. Should scientists still play a major role in it? If in the Soviet Union their involvement in many ways occurred under the institutional coercion, today it is often up to their inner calling for interacting with the public.

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THE LINKAGE BETWEEN BAYESIAN AND FREQUENTISM STATISTICS IS EASIER THAN BETWEEN BAYESIAN STATISTICS AND PHILOSOPHY

Session 31F

Congress section(s): C1

Bayesian statistics and Bayesian philosophy (Bayesianism) originated from the same Bayes rule; a prior and a statistical model are transformed to a posterior when data are given. Despite the increasing popularity, the current Bayesian statistics is biased to computation, numerical estimation and classification, and most of the recent Bayesian statistics studies do not intend to test a hypothesis such as H0: unknown parameter = 0, H1: > 0. In fact, examples of applied studies are, in cases of ecology, estimation of a population size, modeling population dynamics (unifying mortality, reproduction and growth), identification of animal behaviors, hierarchical classification of ecological communities, and so on. On the other hand, the primary targets of the Bayesianism are, in many cases, logic and inference. During these decades, the gap between the Bayesianism and Bayesian statistics have been expanding.
Conventionally, Bayesian statistics have suffered from conflicts to frequentism because the two have couple of essential differences in fundamental concepts.
(1) In Bayesian statistics, a dataset is given and a posterior is obtained from that one dataset, whereas in frequentism, data are assumed to be repeatedly obtainable and expectations over datasets are compared with the data.
(2) Unknown parameters should be optimized for frequentism, while random samples from a posterior play crucial roles in Bayesian statistics.
(3) For model evaluation, the expectation over hypothetically repeatedly obtainable datasets plays a central role when deriving the equation of Akaike information criterion (AIC), while the marginal likelihood of given one dataset is the key concept in Bayesian (e.g. BIC, Bayes factor).
(4) A true model is assumed to exist and to produce the data in the frequentism, whereas the Bayesian statistics does not require the existence of a true model.
Even though, some of recent developments in Bayesian statistics use concepts in both Bayesian statistics and frequentism. For example, WAIC (widely applicable information criterion, Watanabe (2010)) extends the idea of AIC to posterior predictive models in Bayesian statistics. The Stein paradox was proposed under the contexts of the frequentism, and is now reformulated and intensively investigated under the Bayesian framework.
On the other hand, very few Bayesian statisticians take efforts to import a concept in Bayesian philosophy. Presumably, nearly none has interests with a posterior probability of a hypothesis, simply because the catch-all problem prevents us from computing that probability, except for very special cases (e.g. cancer or not, SPAM mail or not).
In this study, I show examples of statistical theories that use concepts both in Bayesian and frequentism and some proposals to reconstruct a linkage between Bayesian statistics and philosophy.
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M. WEBER’S “INCONVENIENT FACTS” AND CONTEMPORARY STUDIES OF SCIENCE-SOCIETY COMMUNICATION.

Session 29B

Congress section(s): B1, B5

In his well-known text “Wissenschaft als Beruf”, M. Weber associates understanding of science as a vocation and its value with the scientist’s ability to present to the audience “inconvenient” facts. The “inconvenience” here means the unwillingness or even inability to recognize such facts from a particular party position belonging to the audience or scientists themselves. Weber argues that this presentation provides “full understanding of the facts” and overcomes the personal value judgment. This overcoming refers to Weber’s understanding of the scientific objectivity. Moreover, Weber uses the expression “moral achievement” to describe this presentation not only as an intellectual task. In this description, he combines epistemological and ethical aspects of scientific normativity and opens the way to speak on the ethics of science and on epistemological virtues.
I propose to interpret this double normativity in the context of STS. Firstly, in these studies, we can find an appeal to the difference, to the multiplicity of facts as a methodological premise, and discovering or inventing better ways of living together, “generating common responses to common problems in a common world” as their objective (Law). Secondly, in the contemporary historical epistemology, which is related to STS, we can discover the investigation of the objectivity not only as a scientific criterion but also as an epistemic virtue (Daston, Galison). Certainly, we should not equate Weber’s understanding of “inconvenient” facts presentation and the interpretation of scientific objectivity in the contemporary

science studies. I will describe the difference of these understandings and emphasize how important it is to insist on similarities.

In my talk, I would also like to question if the above-mentioned epistemic and ethical sides of the 'inconvenient" facts presentation are currently compatible and condition the development of science, i.e. if they serve the production, the legitimization and the distribution of knowledge. There exist almost unambiguous positive answers in two cases: in the analysis of the interdisciplinary communication between different academic cultures; and in the study of the relationship between the teacher and the audience in the sphere of education. However, the contemporary communication between science and society, between experts and non-experts is more problematic. Different strategies and objectives of this communication are widely discussed in the studies of science, technology and society. In these studies, there are no unequivocal answers to the next questions: Should the scientists represent to the public "inconvenient" facts to generate responses to common problems or should they forget about openness, honesty and transparency in communication with public? Should the scientists take in this communication a position of "leader" or persist in a "teacher's" position? Which epistemic virtues should determine science-society relationship? I describe different approaches to this subject in social studies of science, give examples of this relationship in the contemporary Russian context and explain the compatibility of epistemic and ethical sides of "inconvenient" facts presentation requirement in contemporary science communication.

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FIRST-DEGREE ENTAILMENT AND STRUCTURAL REASONING

Session 18M

Congress section(s): A2

In my talk I will show how the logic of first-degree entailment of Anderson and Belnap Efde can be represented by a variety of deductively equivalent binary (Fmla-Fmla) consequence systems, up to a system with transitivity as the only inference rule. Some possible extensions of these systems, such es "exactly true logic" and "non-fasity logic" are briefly considered as well.

System Efde:

a1. A & B |- A
a2 A & B |- B
a3. A |- A v B
a4. B |- A v B
a5. A & (B v C) |- (A & B) v C
a6. A |- ~~A
a7. ~~A |- A
r1. A |-B, B |- C / A |- C
r2. A |- B, A |- C / A |- B & C
r3. A |- C, B |- C / A v B |- C
r4. A |- B / ~B |- ~A

System Rfde is obtained from Efde by replacing the contraposition rule r4 with the following De Morgan laws taken as axioms:

dm1. ~(A v B) |- ~A & ~B
dm2. ~A & ~B |- ~(A v B)
dm3. ~(A & B) |- ~A v ~B
dm4. ~A v ~B |- ~(A & B)

Efde and Rfde are deductively equivalent. Yet, the latter system has less derivable rules, and allows thus certain non-classical (and non-trivial) extensions, which are impossible with Efde.

Consider the following set of consequences:

(dco) A v B |- B v A
(did) A v A |- A
(das) A v (B v C) |- (A v B) v C
(dis2) A v (B & C) |- (A v B) & (A v C)
(dis3) (A v B) & (A v C) |- A v (B & C)
(dni) A v B|- ~~A v B
(dne) ~~A v B |- A v B
(ddm1) ~(A v B) v C |- (~A & ~B) v C
(ddm2)(~A & ~B) v C |- ~(A v B) v C
(ddm3) ~(A & B) v C |- (~A v ~B) v C
(ddm4) (~A v ~B) v C |- ~(A & B) v C

We obtain a system of first-degree entailment with conjunction introduction as the only logical inference rule (together with the structural rule of transitivity) FDE(ci) by adding this list to a1-a3, and taking as the inference rules r1 and r2.

Lemma1. Systems Efde, Rfde and FDE(ci) are all deductively equivalent.

Since the rule of disjunction elimination is not derivable in FDE(ci), it allows for some interesting extensions not attainable on the basis of Rfde. In particular, a Fmla-Fmla version of "exactly true logic" by Pietz and Rivieccio can be obtained as a straightforward extension of FDE(ci) by the following axiom (disjunctive syllogism):

(ds) ~A & (A v B) |- B.

A duality between the rules of conjunction introduction and disjunction elimination suggests a construction of another version of the logic of first-degree entailment FDE(de) with only one logical inference rule (accompanied by transitivity), but now for disjunction elimination. This system is obtained from FDE(ci) by a direct dualisation of all its axioms and rules.

FDE(de) is indeed an adequate formalization of first-degree entailment, as the following lemma shows:

Lemma 2. Systems Efde, Rfde, FDE(ci) and FDE(de) are all deductively equivalent.

Absence of conjunction introduction among the initial inference rules of FDE(de) enables the possibility of extending it in a different direction as compared with FDE(ci). Namely, it is possible to formalize Fmla-Fmla entailment relation as based on the set {T, B, N} of designated truth values, preserving thus any truth value among the four Belnapian ones, except the bare falsehood F. One obtains the corresponding binary consequence system of non-falsity logic NFL1 by extending FDE(de) with the following axiom (dual disjunctive syllogism):

(dds) A |- ~B v (B & A).

Another formalization of the first-degree entailment logic with the only (structural) inference rule of transitivity can be obtained by a straightforward combination of FDE(ci) and FDE}(de).

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OBJECTIVITY AS MIND-INDEPENDENCE – INTEGRATING SCIENTIFIC AND MORAL REALISM

Session 10K

Congress section(s): B4

In general, philosophy of science and metaethics are second-order disciplines, whose subject is the status of their respective first-order discipline – science or normative ethics (see Losee 2001: 2; Miller 2013: 1). In particular, the debates on scientific and moral realism reflect this interdisciplinary parallel insofar as proponents often conceptualize these two views as applications of the same generic realism to different domains (Boyd 1988: 182; Brink 1984: 111). Given this systematic background, it should not be surprising that, for instance, Richard Boyd vindicates scientific as well as moral realism and particularly attempts to transfer insights from philosophy of science to metaethics, and vice versa.

However, recent developments in both debates challenge this attempt of unification. Whereas Boyd's unified realism is based on his naturalist assumption that ethics is part of the empirical sciences, today's proponents tend to advocate a non-naturalist version of moral realism, thus depriving Boyd's approach of its foundation (see, for instance, Shafer-Landau 2003: 68, 72ff). Consequently, if we seek to preserve the opportunity to exchange insights between the two debates, we must look for an alternative connection between both views.

In my talk, I claim that the common ground of scientific and moral realism is their commitment to the objectivity of scientific statements or moral judgments, which, apart from semantic and epistemic components, unfolds in the metaphysical thesis that scientific as well as moral truths obtain mind-independently. I show that this conceptualization allows us, first, to include non-naturalist versions of moral realism and, second, to notice similarities between the arguments proposed for each realism. Especially, David Enoch proposes an indispensability argument for a robust version of moral realism that is supposed to be similar to the no-miracles argument for scientific realism (see Enoch 2011: 50, 55ff). While we are justified to believe in the existence of electrons, because electrons are explanatorily indispensable for the success of science, we are, according to Enoch, also justified to believe in the existence of moral norms, because they are deliberatively indispensable for our practice of exchanging reasons in ethical discussions.

As a result, philosophers of science are supposed not to exaggerate the epistemic component of scientific realism, namely that we have good reasons to believe that our most successful theories are (at least approximately) true. Rather, they should focus on the metaphysical core thesis about objectivity and mind-independence, which is central for every realist position in philosophy – particularly, for both scientific and moral realism (see, for instance, Sankey 2008; Godfrey-Smith 2003: 174, 177).

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SCIENTISTS’ REFLECTIONS ON MESSY SCIENCE

Session 3A

Congress section(s): B1

It has become a commonplace in recent discussions about scientific practice to point out that science is “messy”, so messy in fact that philosophical concepts and arguments are not very useful for the analysis of science. However, the claim that science is messy is rarely spelled out. What does it mean for science to be messy? Does it mean that scientific concepts are intricate or confused? Or that procedures are dirty and often unreliable? Or that methodological criteria are often quite sloppily applied? Is it really such a novel insight that actual science is messy?

Moreover, it is not entirely clear what such messiness means for philosophy and philosophy’s role for understanding or improving science. Philosophical analysis aims at clarifying concepts and arguments, at making distinctions, and at deriving insights that transcend the particulars of concrete situations. Should we be worried that philosophical concepts and arguments have become too far removed of actual scientific practice to capture how science really works or to provide any guidance to scientists? In my presentation, I want to address these sets of questions in an indirect way. I shift the focus from analyzing scientific concepts, methods, and practices to analyzing scientists’ reflections on scientific practice. In doing so, I carve out a new niche for philosophical thinking about science.

I begin with a brief survey of recent philosophical debates about scientific practice. I want to clarify what it is that analysts of science have in mind when they are referring to the “messiness” of research practices, and also what, in their view, the messiness entails for philosophical analysis. In the main part of my talk, I examine what scientists themselves have said about messy science. Do they acknowledge that science is messy? If so, what aspects of scientific research do they highlight? Do they see the messiness of science as a problem for its functioning, and if so, why? To answer these questions, I draw on a diverse set of materials – among other things, methods sections in experimental reports, articles and editorials in general science journals, as well as interviews with scientists.

Analyzing scientists’ own conceptualizations of scientific research practice proves illuminating in a number of ways. We will see that today, scientists themselves are often reluctant to admit that science is messy – much more reluctant than they were a century or two ago. We will also see that it matters – and why it matters – whether scientists themselves are right or wrong about how science really works. In conclusion, I want to suggest that examining scientists’ reflections about “messy science” can fulfill two complementary purposes. On the one hand, such an analysis helps to clarify in what ways science can be considered “messy” and thus improves philosophical understanding of everyday research practice. On the other hand, this analysis points to specific pragmatic challenges in current research that philosophers of science can help address.

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STRUCTURALISM AS A MATHEMATICAL STYLE: KLEIN, HILBERT, AND 19TH-CENTURY GEOMETRY

Session 18D

Congress section(s): C1

Structuralism in contemporary philosophy of mathematics is, roughly put, the view that mathematical theories study abstract structures or the structural properties of their subject fields. The position is strongly rooted in modern mathematical practice. In fact, one can understand structuralism as an attempt to come to terms philosophically with a number of wide-ranging conceptual and methodological transformations in 19th and early 20th century mathematics, related to the rise of modern geometry, number theory, set theory, and abstract algebra. The aim in this talk is twofold. The first one is to focus on the geometrical roots of structuralism. Specifically, we will survey some of the key conceptual changes in geometry between 1820 and 1910 that eventually led to a “structural turn” in the field. This includes (i) the gradual implementation of model-theoretic techniques in geometrical reasoning, in particular, the focus on duality and transfer principles in projective geometry; (ii) the unification of geometrical theories by algebraic methods, specifically, by the use of transformation groups and invariant theory in Felix Klein’s Erlangen Program; and (iii) the successive consolidation of formal axiomatics and the resulting metatheoretic approach to axiomatic theories in work by Hilbert and others. The second aim in this talk is more philosophical in nature. It will be to characterize this structural turn and the structural methods developed in nineteenth-century geometry as a fundamental “change of style” in mathematical reasoning that brought with it a new conception of the nature of mathematical knowledge. Analyzing the “methodological structuralism” underlying 19th and early 20th century mathematics as a mathematical style in this sense will draw both from Ian Hacking’s work on different “styles of reasoning” in science (in particular Hacking 1992) as well as from Granger’s analysis of collective styles in mathematics in his Essai d’un Philosophie du Style of 1968. With respect to the former work, the focus will be in particular on Hacking’s discussion of “standards of objectivity“ related to particular scientific styles. According to Hacking, „every style of reasoning introduces a great many novelties including new types of: objects, evidence, sentences, new ways of being a candidate of truth and falsehood, laws, or at any rate modalities, possibilities“ (Hacking 1992, p.11). Based on the survey of the different methodological developments in geometry mentioned above, we will attempt to spell out the novel standards of objectivity, including a new conception of the very subject matter of geometrical theories, implied by the turn to a structuralist style in mathematics.

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ON THE "MECHANICAL" STYLE IN 19TH-CENTURY LOGIC

Session 20D

Congress section(s): A4, C1

The idea that logic is a mechanical enterprise is so widespread that it is expressed regularly in introductory textbooks. For example, van Dalen writes that "It may help the reader to think of himself as a computer with great mechanical capabilities, but with no creative insight [...]" (2008, 2–3). Quine, after presenting a "silly but mechanical routine for finding any proof" concludes: "So our heavy dependence on luck and strategy, in our own deductive work, is merely a price paid for speed: for anticipating, by minutes or weeks, what a mechanical routine would eventually yield" (Quine 1972, 190). These views are based on the theoretical notions of decision procedures, formal systems, models of computation, and the Church-Turing thesis.

It is curious to note, however, that similar views were also put forward by logicians in the 19th century, who developed very diverse logical systems based on algebra (Boole), combinatorics (Jevons), and diagrams (Venn), when none of these theoretical notions were at hand. An early, and influential, proponent of the mechanical style in mathematics was John Stuart Mill, who wrote in his System of Logic:

The complete or extreme case of the mechanical use of language, is when it is used without any consciousness of a meaning, and with only the consciousness of using certain visible or audible marks in conformity to technical rules previously laid down. This extreme case is, so far as I am aware, nowhere realized except in the figures of arithmetic, and the symbols of algebra [...] Its perfection consists in the completeness of its adaptation to a purely mechanical use. (Mill 1843, 292–293, Book VI, Ch. VI, §6; emphasis by me)

Boole referred approvingly to Mill in his The Mathematical Analysis of Logic (1847, 2) and he also speaks of his own laws of logic as "the very laws and constitution of the human intellect" that underlie the "entire mechanism of reasoning" (Boole 1847, 6). Some 20 year later, in 1865 Jevons began with the design of a reasoning machine that "will be played like a piano, and give give the results of what you read to it without any trouble of thinking further" (Jevons 1886, 213). After two unsuccessful attempts Jevons completed this machine in 1869 (Jevons 1870). An engraving of "the Logical Machine" is shown on the frontispiece of Jevons (1874), where its use is also described. Also Venn, who promoted the use of diagrams in logical reasoning, speaks of "mechanical representation' or propositions and notes that 'It will be easily seen that such methods as those here described readily lend themselves to mechanical performance" (1880, 15).

In this talk, views on the mechanical character of logic will be presented and discussed, with the aims of elucidating the sense in which logic was considered to be mechanical and what epistemological role the mechanical aspect was supposed to play.

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SCENES FROM A MARRIAGE: ON THE CONFRONTATION MODEL OF HISTORY AND PHILOSOPHY OF SCIENCE

Session 7A

Congress section(s): B1

According to the "confrontation model", integrated history and philosophy of science operates like an empirical science. It tests philosophical accounts of science against historical case studies much like other sciences test theory against data. However, the confrontation model's critics object that historical facts can neither support generalizations nor genuinely test philosophical theories. Here I argue that most of the model's defects trace to its usual framing in terms of two problematic accounts of empirical inference: the hypothetico-deductive method and enumerative induction. This framing can be taken to suggest an unprofitable one-off confrontation between particular historical facts and general philosophical theories. I outline more recent accounts of empirical inquiry, which describe an iterative back-and-forth movement between concrete (rather than particular) empirical exemplars to their abstract (rather than general) descriptions. Reframed along similar lines, the confrontation model continues to offer both conceptual insight and practical guidance for a naturalized philosophy of science.

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VALUES IN SCIENCE: ETHICAL VS. POLITICAL APPROACHES

Session 7G

Congress section(s): B5

I take as my starting point the now widely accepted conclusion that doing science requires making value judgments. Philosophical work in value theory offers two very different ways of determining values: distinctly ethical and distinctly political approaches. These approaches often yield conflicting conclusions, since any meaningful commitment to democracy must involve deferring to the wishes of the public in at least some cases in which the public makes a normative error — i.e., reaches a conclusion that is ethically sub-optimal. Thus, it is important to consider whether the value judgments required by science should be grounded in ethical or political reasoning.

In the first part of the paper, I show that although this issue is rarely discussed explicitly, philosophers more commonly take an ethics-oriented approach to values-in-science questions. Douglas, for example, famously argues that scientists have the same moral responsibilities as the rest of us (ch. 4), and Elliott says that "the values influencing our research... should adequately represent fundamental ethical principles" (106). I show that the same can be seen even more clearly in discussions of specific value judgments — e.g. particular inductive risk decisions, discussions of the economic discount rate, and debates about the construction of QALYs. (The biggest exception to this is Kitcher. His work is, though, somewhat detached from much contemporary literature on values in science.)

STS scholars, on the other hand, much more commonly take a political approach. They, however, typically restrict themselves to thinking of scientists as political agents in a descriptive sense. They therefore rarely tie their discussions to work in normative political philosophy and political theory, which could be used to offer concrete recommendations to scientists about how they should navigate scientific value judgments, conceived politically.

In the second part of the paper, I try to more carefully clarify the difference between distinctly ethical and distinctly political approaches. Approaches rooted in ethics typically focus on substantive questions about which value judgements are correct or incorrect, or about which are well- versus poorly-supported by reasons. Approaches rooted in political philosophy typically work from a different set of concepts, setting aside substantive analysis of the values in question in favor of concerns with procedure, legitimacy, representation, accommodation for opposing viewpoints, accountability, and so forth. I (in contrast to most philosophers) favor a more political approach to most values-in-science questions, and (moving beyond most STS scholars) believe that the tools of normative political philosophy and theory can tell scientists how they ought to navigate such cases. And so in the third part of the paper I consider what role the distinctly ethical arguments typical of

philosophers can have, given a political approach to values in science. I conclude that they can serve at least two valuable roles. First, they can be seen as (mere) persuasive exercises: arguments that we might hope will move our fellow citizens to adopt a particular point of view on issues of value. Second, and more interestingly, liberal political philosophies since Rawls have relied on a distinction between reasonable and unreasonable pluralism: values that lie within the range of reasonable disagreement are owed a certain kind of respect and are eligible for adoption through democratic processes. Values outside that range are not. The substantive ethical arguments of philosophers, I argue, can help us see what lies within versus outside the range of reasonable disagreement. I conclude by offering suggestions about how philosophers' (ethical) arguments can be modified to better fulfill these roles.

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NEUROSCIENCE: SCIENCE WITHOUT DISGUISE. A CRITIQUE OF MANZOTTI'S AND MODERATO'S DUALISTIC ACCOUNT OF NEUROSCIENCE

Session 5F

Congress section(s): C5

In their 2014 article, Riccardo Manzotti and Paolo Moderato state that neuroscience is "dualism in disguise". According to the Authors, neuroscience uses a dualistic framework implicitly, as classical physicalist accounts are not capable of explaining all mental phenomena, consciousness in particular. In this paper, after shortly referring to Authors' main arguments, I present the classical definitions of basic neuroscience and cognitive neuroscience, and the relevant frameworks of explanation (compared to some of the other classical concepts of explanation). I enlist several – methodological and epistemic – problems that neuroscientific accounts of mental phenomena meet and describe a recent mechanistic/functionalistic framework by application of which some of these obstacles could be overcome. In effect, I am aiming at showing that Manzotti's and Moderato's "accusations" of neuroscience being dualistic are invalid, and – from the methodological perspective – are most likely rooted in some form of a misunderstanding of the concept of scientific explanation.

Manzotti, R., Moderato P., (2014) "Neuroscience: Dualism in Disguise", Contemporary Dualism: A Defense, Andrea Lavazza, Howard Robinson (Eds.), Routledge.

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APPROACHING OBJECTIVE PROBABILITIES BY META-INDUCTIVE PROBABILITY AGGREGATION

Session 5D

Congress section(s): B2

Section 1: Predictive success of probabilistic hypotheses and methods.
Objective statistical laws over infinite domains inform us about frequency limits in random sequences, but they don't deductively entail any observation statements. The only way to obtain from them predictions about observable events is by way of subjective probability assertions, derived by means of a ‚statistical principal principle' which connects statistical with subjective probabilities.
The probabilistic single case predictions obtained from objective probability hypotheses have the form "the probability of an unobserved event is such and such", formally "$P(X=1) = r$", where X is an (assumedly) binary event variable(X in {0,1}). But how should the truthlikeness of such a probabilistic prediction in relation to the actual event e be scored? It is a well-known fact that according to the scoring function that is based on the absolute distance $|r-e|$ one should not predict one's probabilities, but rather, one should predict according to the so-called maximum rule: "predict 1 if r greater-equal 0.5 and otherwise predict 0". A solution of this problem going back to Brier is the use of so-called proper scoring functions (based, e.g., on quadratic loss functions): with their help one maximizes one's expected score exactly if one predicts one's probabilities.
Section 2: Bayesian prediction games.
Thus I suggest to estimate the truthlikeness of a probabilistic hypotheses or, more generally, of a probabilistic prediction method, by its predictive success as measured by a proper scoring function. Technically I implement this in the form of a probabilistic prediction game ((e),PR), which consists of a sequence (e) of events $e_1,e_2,...$ and a finite set $PR = \{P_1,...,P_m\}$ of probabilistic predictors (hypotheses or methods) which are identified with their probability functions. At each time point n the task of every predictor P_i consists in delivering a probabilistic prediction of the next event: "$P_i(X_{n+1}=1) = r$". Probabilistic prediction methods are assumed to be able to learn based on the events observed so far; apart from that they may be of any sort.
Section 3: Meta-inductive probability aggregation. In the final part of the paper I turn to the problem of probability aggregation. Here the problem is to find a reasonable collective probability function based on a given set of individual probability functions P_i (e.g., of experts or peers). A well-known method is arithmetic probability aggregation; the open question in this field is how to determine the optimal weights. If we assume that probability aggregation takes place within a prediction game as describe above, then deep results about meta-induction (author 2008, 2019) can be utilized to define a aggregation method P_MI that is guaranteed to be optimal in the following sense: for every possible event-sequence (e) and set of individual prediction methods $PR = \{P_1,...,P_m\}$ there exists a success-dependent weight assignment $w_n(i)$ such that the long-run success of $P_{MI,n} =def [Sum_{1<=i<=m}w_n(i).P_i]$ is at least as good or better than the maximal success among the individual predictors P_i, even if the best predictor is permanently changing."

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THE JACOBSON RADICAL AND GLIVENKO'S THEOREM

Session 27C

Congress section(s): A1

Alongside the analogy between maximal ideals and complete theories, the Jacobson radical carries over from ideals of commutative rings to theories of propositional calculi. This prompts a variant of Lindenbaum's Lemma that relates classical validity and intuitionistic provability, and the syntactical counterpart of which is Glivenko's Theorem. Apart from perhaps shedding some more light on intermediate logics, this eventually prompts a non-trivial interpretation in logic of Rinaldi, Schuster and Wessel's conservation criterion for Scott-style entailment relations (BSL 2017 & Indag. Math. 2018).

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TRUTH LIES: TAKING YET ANOTHER LOOK AT THE THEORY-LADEN PROBLEM

Session 5I

Congress section(s): B1, B3, B4

As a point of departure, it would be reasonable to accept that observation is "Theory Laden" and that therefore the idea of objectivity in scientific theory is bogus. The issue of the "Theory-Laden" problem is closely related to almost everything else in the philosophy of science, for it concerns the epistemological understanding of what is scientific knowledge, and it bears upon one of the most central rivalries of the 'realism-antirealism debate.'
One way to describe this rivalry is by focusing on the concept of truth and its relevance to the understanding of rationality in science. Both the realist protagonist and the antirealist antagonist acknowledge the central place truth has in science and

in understanding its rationality. However, many studies in the last few decades were reluctant about the neutrality of this rivalry towards the cultural, social, moral and political aspects of science. Subsequently, there was growing entanglements within the philosophy of science of these aspects. The cultural approach among others relativizes science to these aspects while dispensing altogether with the truth as a critical concept for the understanding of science and its rationality.
Science is just one means among many of gaining knowledge and is the product of a specific knowledge culture, the Western one. Although it has been preferred by governments, industry and others, knowledge is not unique to this culture since there are alternative forms of knowledge and other historical knowledge cultures. Accordingly, science is a ‚knowledge culture' in the sense that methodology, research methods, and the facts which are obtained by them, are shaped by culture.
There is no doubt that this shift in the philosophy of science brought many valuable insights into the nature of science. However, there is no satisfactory explanation in philosophical terms to the commitment to truth in science. Although studies in the logic of confirmation and methodology have put an end to the naive notion of truth in science, still modern scientific practice relies virtually on the notions of right or true within the inter-subjective domain. The truism that objectivity is unattainable does not change the fact that, in practice, scientists and researchers adhere to these notions.
I suggest a way in which the cultural approach can be maintained without excluding the concept of truth. The idea is inspired by the atypical treatment of this concept by the French psychoanalyst Jacques Lacan and using his ideas in a more metaphorical way rather than analogous.
According to Lacan, there is an intricate play within the psychoanalysis discourse between truth on the one hand and deception and lies on the other. Psychoanalysis aims to reveal truth, which is inscribed in the deception of the analysand's speech. The analysand may well think that he or she is telling the truth, but in the context of psychoanalysis, deception and lies are inscribed in the text of truth. Moreover, the analyst knows perfectly well that lies can reveal the truth about desire more eloquently than what the analysand will think as honest or true statements. Thus, truth according to Lacan is in language and speaking rather than part of reality itself.
Accordingly, for the philosophy of science, and within the cultural approach, there is no truth in the scientific research. However, from the point of view of the scientist and within the scientific practice and discourse, truth has and should have a dominant regulative role in directing and executing scientific research. Thus, although the cultural approach forecloses the concept of truth as an explanatory concept in science, truth cannot be dismissed altogether as the raison d'être for the scientific practice from the point of view of the scientists, the scientific community and the scientific discourse."

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PHILOSOPHICAL AND DEMARCATION ASPECTS OF GLOBAL WARMING THEORY

Session 15D

Congress section(s): B1, C2

In their effort to explain a phenomenon, scientists use a variety of methods collectively known as "the scientific method". They include multiple observations and the formulation of a hypothesis, as well as the testing of the hypothesis through inductive and deductive reasoning and experimental testing. Rigorous skepticism and refinement or elimination of the hypothesis are also part of the process. This work presents an updated concept of the scientific method with the inclusion of two additional steps: demarcation criteria and scientific community consensus. Demarcation criteria such as hypothesis testing and falsifiability lead to a proposed "Popper Test". The method is applied to fundamental aspects of Global Warming theory (GW).
David Hume's "problem of induction" is the concern of making inductive inferences from the observed to the unobserved. It is shown that this issue is crucial to GW, which claims that temperature observations of the last 100 years create a new pattern of systematic warming caused by human activity. We use the term "Global Warming" rather than "Climate Change" as it is more amenable to falsification and is therefore a stronger scientific theory.
Natural phenomena can have multiple causes, effects and mitigating factors. Many interrelationships among these are not well understood and are often proxied by statistical correlations. Certain statistical correlations exist because of a causal relationship and others exist in the absence of causal relationships. Statistical correlations can lead to the formulation of a theory but do not constitute proof of causality. This must be provided by theoretical and experimental science. Trial and error leads to model enhancement as, for example, climate models have recently been modified to include the effect of forests, an important missing variable in prior models.
Tests comprising the proposed method are applied on fundamental assumptions and findings of both parts of GW theory: (1) Rising global temperatures, (2) Anthropogenesis of rising global temperatures. Several premises of the theory are found falsifiable within the means of current technology and are therefore scientific theories. Certain other premises are found to be unfalsifiable and cannot be included in a scientific theory. The latter must be eliminated or be substituted by alternative falsifiable proposals.
1 Popper, Karl, 1959, The Logic of Scientific Discovery, Routledge.
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OPTIMAL TEAM STRUCTURES IN SCIENCE

Session 9G

Congress section(s): B2

Simulations of scientific inquiry are largely based on idealized communication structures. Agent networks structured as a wheel in comparison to completely connected graphs dominate the literature, e.g. (Grim 2009, Zollman 2010). These structures do not necessarily resemble the typical communication between scientists in different fields. The data on team structures, from the high energy physics laboratory Fermilab, show that scientists are usually organized in several groups. Furthermore, the work on Fermilab project efficiencies by Perović al. (2016) allows us to identify the group structures which are usually more efficient than others. They showed that smaller groups outperform large ones. We developed models resembling the team structures in high energy physics analyzed by Perović al. (2016) and tested whether efficiency results coincide.
These models have very interesting properties. First, they show how beliefs can be strongly self-enforced in subgroups, even when most scientists have opposite views. This is particularly evident when we consider that scientists in the subgroups interact more with each other than with scientists from other groups. Our model agrees with the empirical results from Perović al. (2016). Groups separated in as few teams as possible outperform the groups separated in a higher number of teams.
Team structures in science are field-dependent. In biology, laboratories are typically structured hierarchically. We developed models simulating three different management styles: from groups with one leader controlling everybody to groups with two levels of hierarchy. These structures and their effects on group performance were brought up and discussed during qualitative interviews we performed with biologists. We compared a group in which only the professor communicates with twenty junior scientists with a group in which the professor communicates with postdoctoral researchers who in turn supervise PhD students. As last group structure we studied how the communication develops when the group leaders in addition communicate among each other. We also included time constrains: strongly connected nodes communicate less with their partners than weakly connected ones. E.g. a professor might be connected with 20 PhD students, but will not be able to talk to every single one all the time.
When we abstract from time constrains, our simulations indicate that centralized groups reach the conclusion faster than decentralized networks. However, when we consider that professors have a limited time for communication, we can see that groups with additional levels of hierarchy perform much better than the centralized group. In addition, the group structure where the scientists exchange information on the postdoctoral researcher level outperforms the structures where this interaction is lacking. The results highlight that group leaders should communicate with each other. Also, they should not have too many students. Finally, large groups should be decentralized.
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FOOD, IDENTITY AND END OF LIFE

Session 18I

Congress section(s): C4, C7

Food is socio-cultural carrier of value and philosophically important source of meaning that becomes ever more complex and fraught when approaching the end of life. This paper examines the intersection of food as a locus of meaning, identity, belonging, and community from the perspective of individuals whose experience of food is shaped by terminal illness (Andrews, 2008; Telfer, 2012). Specifically, I discuss how food, at the end of life, becomes instrumentalized as pure sustenance and how its medicalization can lead to changes in family dynamics, reflect the deprivation of sources of comfort, pleasure, and identity, and produce a profound sense of social loss.
Food, in this context, can reflect an embodied breakdown in connection, as a result of the breakdown in bodily autonomy, but also potentially serve as a way to exert agency against further treatment (Quill et al, 2000). It can also, however, in the last phase of life, be used to revive sources of pleasure, known as 'pleasure feeds,' where food reasserts itself as a source of enjoyment often with the participation of the family and friends in eating with the individual or helping them to do so.
In this talk, I draw on contemporary work on the philosophy of food, food and pleasure, food and sociality, food and identity, as well as the sociology of medicine, using a distinctly feminist lens, to make the case that this understudied subject can provide rich and significant insights into role food plays in our contemporary social milieu and how its loss and/or transformation can be dealt with at the end of life. In doing so I draw on the historical work of scholars that engage with the examination of hedonism, pleasure, virtue, sociality and food systems (Symons, 2007; Toussaint-Samat) as well as more contemporary studies of food that examine conceptions of liberal subjectivity, agency, medicalization, and community (Watson, 2018; Coveney, 2006).
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THE UNITY OF SCIENCE: FROM EPISTEMIC INERTIA TO INTERNAL NEED

Session 5J

Congress section(s): B6

Science sprang from the spirit of unity, starting from the general idea of knowledge. Despite the fact that it developed until the beginning of modernity, it also preserved the idea of unity. Once with modernity, and the first broad classification achieved by Francis Bacon (1605), followed by the ontological clashes within knowledge, the idea of unity becomes an alternative, while the development of science in its own matrix is seen as a priority. Today, by complex processes of division, hybridization, concatenation, the number of sciences, study fields, and subjects increase exponentially, while the problem of unity is back into discussion.
In this paper we want to analyze whether the question of unity of science, as it is theorized today, is quite a real problem or just an epistemic inertia. In doing this, we will firstly describe the process of the transition from unity to diversity, from antiquity through the Middle Ages up to the beginning of modernity. We will also focus on the turning point in "The Advancement of Learning", arguing why the Baconian perspective was a unitary one, together with the dichotomous development of science in modernity. We will then analyze the internal needs of the interdisciplinary dialogue and how these can be covered by the current theoretical main directions of unity of science: encyclopedias, reductionism and transdisciplinarity. Finally, we will synthesize a methodological vision of interdisciplinary natural communication that can cover the internal need of the sciences to develop into a coherent integrated system and we will support its imperative character, even if apparently from a pragmatic point of view, it does not seems to be a priority.
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LEŚNIEWSKI, LAMBDA, AND THE PROBLEM OF DEFINING OPERATORS

Session 25M

Congress section(s): A2, A4

The aim of this paper is to tackle a metalogical problem (uncharacteristically) left unsolved by Stanisław Leśniewski (1886–1939).
Leśniewski considered definitions to be object-language equivalences adding to his nominalistically-conceived logical systems. He therefore took extreme care about the conditions governing proper definitions, and regarded his "definition of definitions" as his greatest achievement. His canons of definition, for the two systems of protothetic ([1], 479–81) and ontology ([2], 613–5, 619–22) applied only to sentences, names, and functors with syntactic categories ultimately defined in terms of sentences and names. Among variable-binding operators however, his "official" systems used only the universal quantifier, considered as syncategorematic. While in everyday use he also employed its dual, the particular quantifier, he found himself unable to formulate canons of definition for operators, and his offer of any degree to any student who could do so went unclaimed. The question then remains open as to whether adequate conditions for the introduction of new variable-binding operators in logic can be given in a Leśniewskian fashion.
Since the time of Ajdukiewicz ([3], 230 f.) and Church it has been realised that a general abstraction operator in conjunction with suitable functors can be deployed to define other operators. The system in Church's version of the simple theory of types" [4] is exemplary in this regard. Church's promising technique cannot be applied directly to Leśniewski's systems however, for one thing because it omits polyadic functors, but more crucially because it presupposes a platonistic infinity of formulas and axioms, which is inimical to Leśniewski's nominalistic stance.
In this paper we adapt Church's typed lambda calculus to extend Leśniewski's language with general abstraction operators, each assigned to a syntactic category, using them alone for binding variables, and combining them with functors to allow operator definitions. The idea is that Leśniewski's canons of definition for functors together with the rules for lambda will allow defined operators to be introduced in a controlled way. The question of the conformity of this combination to the strict requirements of Leśniewski's metalogic will then be considered.
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ON THE SEVERAL KINDS OF NUMBER IN BOLZANO

Session 20B

Congress section(s): B6

Modern philosophy of arithmetic almost invariably begins with a discussion of Gottlob Frege and his failed attempt to give a logicist derivation of the laws of arithmetic. A much less well known but similarly revolutionary treatment of the natural numbers and their close cousins was given half a century earlier, by Bernard Bolzano. Despite sharing many platonist assumptions with Frege, Bolzano's treatment of numbers is markedly more differentiated than that of Frege. It is therefore instructive to pull together Bolzano's somewhat scattered remarks (found principally in [1] § 85 ff., [2], [3] and [4]), investigate the variety of objects he was prepared to call "number", and see to what extent we can learn from this variety even today. We confine attention to what Bolzano himself called "the whole numbers". Those quantities which call for negative, rational and irrational numbers form in Bolzano a topic too vast for brief discussion. By contrast with his account of continuous magnitudes, his theory of numbers stands up remarkably well.

Bolzano's treatment of the numbers presupposes his theory of collections (Inbegriffe), itself a rich and extensive topic in his ontology. The four most important kinds of collection for Bolzano's theory of number are multitudes (Mengen), sequences (Reihen), sums (Summen) and pluralities (Vielheiten), whose informal definitions we give. On this basis, Bolzano distinguishes concrete and abstract units of a given kind A, concrete and abstract pluralities of As, named and unnamed pluralities, and finally, the abstract natural numbers themselves. All of these distinctions make perfect sense and have straightforward application. The main flaw in Bolzano's treatment is his account of sequences, which, because he does not admit repetitions (unlike the modern concept of a sequence) undercuts his theory of counting. Sequences are usually now defined as functions from the natural numbers or an initial segment thereof, but that presupposes numbers. It would be preferable to avoid such dependence. We show how Bolzano's theory can be modestly extended and given a better basis by admitting collections of collections. The chief remaining gaps are then a proper understanding of the ancestral of a relation, and a recognition of collections of different infinite cardinalities: both of these innovations followed a good fifty years later.

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V.N. IVANOVSKY'S CONCEPTION OF SCIENCE

Session 18L

Congress section(s): B6

The multidimensional model of science created by Russian philosopher Vladimir Nikolayevich Ivanovsky at the beginning of the 1920s covered social, psychological, logical and methodological, ideological aspects of science, took into account the variety of types, methods and contents of various sciences, associated their development with both internal factors and interaction with other sciences and other areas of culture. Ivanovsky presented his conception in "Methodological Introduction to Science and Philosophy" (1923). Unfortunately, he and his works currently are unknown not only to the world scientific community, but also to Russian philosophers and historians of philosophy.

In the beginning of the XX century, however, V.N. Ivanovsky was famous in international academic community. He participated in the 1st International Philosophical Congress in Paris and represented Russia in the Bureau of Philosophical Congresses until the October Revolution. He studied in Moscow University, then in the main scientific European centers (Berlin, London, Oxford, and Paris). Ivanovsky taught philosophy, psychology, and history of pedagogy in universities of Kazan, Moscow, Samara, and Minsk. He also was a secretary both of journal "Voprosy filosofii i psikhologii" (Questions of philosophy and psychology) in 1893-1896 and of Moscow Psychology Society in 1893-1900.

The multidimensional model of science by V.N. Ivanovsky demonstrated socio-psychological, logical-methodological, and philosophical aspects of science. Socio-psychological aspects of science are due to the fact that living conditions affect the content of knowledge, as they give sciences a stock of experience, provide with analogies suitable for explaining the unknown, for formulating hypotheses. The recognition of scientists' works in particular era depends on "life", because a scientist always risks getting ahead of his time, being misunderstood, not appreciated, not recognized by contemporaries. Ivanovsky emphasizes that science is a system of views that is not only proven, but also recognized as the true known by many people.

Ivanovsky drew attention to the importance of psychological preferences and traditions in the development of social and natural sciences, the role of the scientific community.

Considering the influence of social and psychological factors on the form and content of scientific knowledge, V.N. Ivanovsky determined their social role and importance in opposite to the manifestations of Soviet Marxism in vulgar "class approach" to science in the post-revolutionary period.

Describing science as a multidimensional system, V.N. Ivanovsky expresses a number of ideas relating to the psychology of knowledge and creativity. He wrote about the "theoretical instinct of curiosity" as the psychological basis of the pursuit of truth.

Logic, by V.N. Ivanovsky, is the crucible through which thought must pass in order to become a science. Requirements of proof, truth, significance depersonalize all achievements of thought; tear them away from their subjective roots and motives. A researcher may be driven by purely theoretical curiosity, motivations of a socio-ethical, religious, aesthetic, or other nature. The "logical test" ensures the objectivity of science. V.N. Ivanovsky interpreted it quite widely, referring to this and the procedures for confirming by experience, i.e. empirical verification.

Systematic is a necessary property of science, which distinguishes it from a collection of disparate information. By Ivanovsky, science is always based on certain principles, general prerequisites. Information becomes scientific when it is included in a logical whole.

He stressed that not all the results of science are and must be put to practical use. Speaking against the narrow practicality in relation to science, V.N. Ivanovsky argued that the social value of genuine scientific creativity is immense.

The scale, depth and clarity of the concept of science developed by V.N. Ivanovsky, put it on par with the achievements of postpositivism.

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PHILOSOPHY (AND METHODOLOGY) OF THE HUMANITIES: TOWARDS CONSTRUCTING A GLOSSARY

Session 12L

Congress section(s): C7

It is hard to challenge the point of view according to which our century is a century of Humanities. Undisputable evidence in favor of this point is a list of thematic sections of 14th, 15th and our 16th Congresses of Logic, Methodology and Philosophy of Science (and Technology). We mean that the list of 14th Congress did not include any section with the term “Humanities” in its title; the programme of 15th Congress included a section devoted the philosophy of the Humanities, so did the present Congress, although this section has – if it is possible to say – a “palliative” title “Philosophy of the Humanities and the Social Sciences”, And now among topic areas of the 16th Congress one can see “Philosophical Traditions, Miscellaneous”. Now there is the intricate spectrum of different approaches to the philosophical and methodological problems of the Humanities, each of which is connected with its own “philosophy”, ideology, and visions. The fact is that the attempt to form the philosophy of the Humanities along the lines of the philosophy of science is definitely – and perhaps irrevocably – failed. It is time to scrutinize this spectrum with the aim to find a certain sustainable set of terms and notions for creating a basis of philosophy (and methodology) of the Humanities.

We propose not the dictionary, not the encyclopedia (in Umberto Eco’s sense), but namely glossary, each entry of which will contain clear, straightforward definitions, practice and examples of using, from one side, and, from the other side, each entry will not be closed – it may be supplemented and advanced. The order of entries will not be alphabetical; it will rather be determined by the functional features of the terms and notions, by their relationships to each other. These relations can be historical, methodological, ontological, lexico-terminological, socially-oriented etc. The terms and notions included in the glossary give us opportunity to form a certain kind of the frame or better to say, some kind of net for further researches. The net (frame) may be expanded, by means of including new notions, terms, phrases and collocations; the frame may be deepen by means of forming new connections between “old” notions or between “old” and “new” notions and terms. For example, we include notion “text” in the glossary, this inclusion forces to include such notions as “author”, “reader”, “language”, “style”, “(outer) world”, “history”, “value”.

We suppose that the initial list of basic notions must include the following set: representation, intention, sign and sign system, code, semiosis and retrograde semiosis (as a procedure of sense analysis), sense, meaning, dialogue, translation, text (and notions connected with text), interpretation and understanding. It is easy to see that basic notions are used in different realms of the Humanities (semiotics, hermeneutics, significs, history of notions and history of ideas, theory of literature, philosophy, logic and linguistics); this fact emphasizes their basic features.

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WHAT IS CONSTITUTIVE FOR FLAVOUR EXPERIENCES?

Session 16M

Congress section(s): C5

Within contemporary philosophy of perception, it is commonly claimed that flavour experiences are paradigmatic examples of multimodal perceptual experiences (Smith 2013; Stevenson 2014). Typically, the phenomenal character of a flavour experience is determined by the activities of various sensory systems processing, inter alia, gustatory, olfactory, tactile, thermal, and trigeminal data. In fact, virtually all sensory systems, including vision and audition, are believed to influence how we experience flavours. However, there is a strong intuition that not all of these sensory systems are constitutive for flavour perception while others merely influence how we experience flavours (see Prescott 2015; Spence 2015).

From the philosophical perspective, addressing the above issue requires explicating what it means to say that some factors are ‘constitutive’ for flavour perception and providing a criterion for distinguishing constitutive and non-constitutive factors. My presentation aims to address this theoretical question in a twofold way. First, a theoretical framework is developed which defines the stronger and weaker senses in which the activities of sensory systems may be constitutive for flavour perception. Second, relying on empirical results in flavour science (e.g., Delwiche 2004; Spence et al. 2014), the constitutive status of activities related to distinct sensory systems in the context of flavour perception is investigated.

In particular, I start by providing a notion of minimal constitutivity that is developed relying on considerations presented in works regarding analytic metaphysics (Wilson 2007) and philosophy of science (Couch 2011; Craver 2007). The main intuition behind my conceptualization of constitutiveness, is that being constitutive is closely connected to being necessary. From this perspective, activities of a sensory system S are minimally constitutive for flavour perception if there is a way of obtaining a flavour experience F such that this way of obtaining it requires the presence of an activity of system S. Subsequently, stronger notions of constitutivity are defined, and I explicate how they can be applied in considerations about flavour perception. Finally, I consider the constitutive status of activities associated with functioning of the selected sensory systems relevant for the flavour perception: olfactory, gustatory, tactile, auditory, and visual. I argue that activities of all these systems, except the visual one, are at least minimally constitutive for flavour perception.

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ADOLF GRÜNBAUM ON “ZENO’S METRICAL PARADOX OF EXTENSION”

Session 3C

Congress section(s): B6

Adolf Grünbaum explained to “modern eleatics” in Philosophy how measure theory answers Zeno’s “metrical Paradox”. I will present a reconstruction of this paradox, ancient responses, and Grünbaum’s analysis. As counterpoint, I argue that Zeno invented a fundamental argument for non-measurable sets and that responses of Democritus and Aristotle remain viable.

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THEORY OF IMPETUS AND ITS SIGNIFICANCE TO THE DEVELOPMENT OF LATE MEDIEVAL NOTIONS OF PLACE

Session 25G

Congress section(s): B4, B6

In the discourse around theories explaining scientific progress, Natural philosophy of the Late Medieval period is seen as playing the role of apologetics. For philosophers of science, with their repudiation of metaphysics the task of providing a rational reconstruction of the way scientific progress has occurred is nigh impossible. Even explanations such as the Popperian and the Kuhnian strain under great difficulty and provide only partly satisfactory results. In his "Logik der Forshung" (1934), Karl Raimund Popper argues that metaphysics plays an accidental part in the emergence of new scientific ideas. Correspondingly, in "Structure of Scientific Revolutions" (1962) by carrying out theoretical interpretations and classification of empirical facts without their metaphysical premises Thomas Kuhn explains the development of Natural Science, but ignores changes of worldviews. As a result, he comes to the conclusion that Natural Science was formed under the influence of erroneous interpretations of Aristotelian Natural Philosophy made by Medieval Natural philosophers. These are some of the reasons why medievalists are still made to defend Late Medieval Natural philosophy from shallow convictions that at medieval universities, nothing of any significance to contemporary science and philosophy took place. Seeking to render a fragment of a rational and coherent reconstruction of the development of Natural philosophy I investigate one idea of Late Medieval philosophy, the explanation of motion (impetus), and its relation to other ideas, such as the principle of parsimony, often associated with William of Ockham, and Nicole Oresme's notion of place. I base this inquiry on the presumption that two ancient Greek philosophical approaches toward the Natural world, Aristotelian and Platonic, were of crucial significance to the Medieval notion of Natural science. Not only do both programs have metaphysical underpinnings, but also theological premises. The main statement of the paper holds that the ideas of Late Medieval Natural philosophy have a decisive significance for the development of modern Natural science instead of accidental or negative one. Thus, following the two aforementioned programs, the premises of Jeans Buridan's theory of impetus are reconstructed. Then, the debates over the necessity of empty space are presented in the works of Nicole Oresme, William of Ockham and Jean Buridan, and finally, the pivotal role of these ideas in the modifications of Natural philosophy is ascertained.

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TITLE WHAT'S IN A NAME? TO THE HISTORY OF A 'SCIENTIST'

Session 30B

Congress section(s): B6

In his notorious lecture "Science as a Vocation" (Weber, 1989) given in 1919, Max Weber attempted to catch such an escaping and changing entity as the very nature of the scientific profession. According to Weber, the ambivalent position of a scientist consists of an ambivalent feature of science itself. On the one hand, a scientist has to have a very special state of mind to proceed with the scientific activity. On the other hand, science has a specific academic structure, which, in its turn, depends on its links to political and economic institutions. By comparing German and American academic careers, Weber demonstrates the failures and benefits of both academic foundations for scientists and scholars.
However, this analysis became one of the most fruitful tools to explicate the Academy in its links to (a) a special pursuit of a scientist; (b) the struggle for academic achievements; (c) the perpetual progress of academic work; and (d) the role of politics, this was not the first case for a continuous debate on the nature of a scientist and its role in a society. In this talk, I propose to dig deeper into the debates on the definitions of a scientist in the period, when the science found itself in a process of becoming a complex institutional structure, i.e. to the XIXth century. In this talk, I would like to focus on three cases from the debates on (1) the social and political role of a scientist, and (2) the definition of a scientist within this role.

Case 1.The Demand of Governmental Paternalism: Charles Babbage and David Brewster's criticism of the conditions of scientific development in England (Brewster 1830).
Case 2. The Controversy of a Name. In 1834 William Whewell coined the term 'scientist' in the purpose to create a 'general term by which these gentlemen could describe themselves with reference to their pursuits' (Whewell 1834, 61) and 'a name to describe a cultivator of science in general' (Whewell 1840, cxiii).
Case 3. The Nurture of a Scientist. Alphonse de Candolle's analysis of the European Academies of Sciences, where by scientists he defined 'those who have a noble desire to make a discovery or to publish something new, [and] must concentrate their efforts on the only one scientific discipline, and sometimes even on the only one section of this discipline' (de Candolle 1873, 74).
These debates on the role and pursuit of a scientist lead to the XXth century debates on the phenomenon of public intellectuals and more recent debates on the demands of the society to the scientific community as well as the requirements to the scientists to fulfill these demands. By reconstructing the formation of a contemporary image of a scientist via XIXth century I will follow the path of the creation of a contemporary 'scientist'.
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THE ROLE OF COGNITIVE AND BEHAVIORAL RESEARCH ON IMPLICIT ATTITUDES IN ETHICS

Session 15M

Congress section(s): C5

Empirical research and philosophy have both recently been interested in what implicit attitudes and biases are and in how they can predict prejudicial behavior towards certain groups (Brownstein 2017; Brownstein, Saul 2017: 1–19). The assumption on the basis of such data is that human beings are sensitive to their own and others' group identities. These researches I focus on show an effect that mostly goes under the radar: even subjects that have non-racist explicit attitudes can and often are biased by implicit stereotypes and prejudices concerning group identities.
The aim of this talk is to look at this set of data from the perspective of moral philosophy. Thus, on the one hand, I will be interested in analyzing if and to what extent implicit attitudes have an impact on abilities that are crucial for moral judgment and for moral decision-making – the descriptive goal – and, on the other, I will consider whether this influence should bear any normative significance for moral theory – the normative goal. I will deal with whether these implicit attitudes have an impact on some of the key components constituting the basis of our moral abilities, regardless of whether one can be deemed responsible or not. I will thus consider the effects – if any – that implicit attitudes have on empathy and trust, understanding both as related to our way of judging and behaving morally (for discussion on this, e.g. Faulkner, Simpson 2017; Stueber 2013). I will focus on the effects on empathy and trust on the basis of the widely shared – though not universally accepted – assumption that they both play a relevant – though not exclusive – role in morality, and given that experimental data on implicit attitudes seem to suggest at least an unconscious proclivity towards empathizing and trusting in-groups more than out-groups.
My main claims will be:
(a) The descriptive claim: Implicit in-group bias directly modulates empathy and automatic trust, while it has only a derivative influence on sympathy and deliberated trust.

(b) The normative claim: If moral duties or standards are meant to guide human behavior, then knowing about our implicit biases towards in-groups restricts the set of moral theories that can be prescribed (according to a criterion of psychological realizability). And yet this is not tantamount to claiming that we cannot and should not take action against our implicit attitudes once we have recognized their malevolent influence upon us.

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EMPLOYING COMPUTERS IN POSING AND ATTACKING MATHEMATICAL PROBLEMS: HUMAN MATHEMATICAL PRACTICE, EXPERIMENTAL MATHEMATICS, AND PROOF ASSISTANTS

Session 6C

Congress section(s): C1

Computers are impacting mathematical research practice in diverse and profound ways. The use of computers has clearly transformed practices connected to typesetting and communication, but computers have also – although less visibly – influenced practices connected to proofs and formulation of theorems; since the 1970s, computers have been important in running extensive searches as parts of proofs of mathematical theorems, and in the past 20 years two new and very different research programs concerning the use of computers in mathematics have emerged, one centered on the use of computers as aids in formal proof processes and another investigating exploratory and experimental uses of computers. Despite these efforts, the construction of mathematical proofs is still mainly a human activity; mathematical creativity has not been mechanized, neither in proof construction nor in hypothesis formulation. This leads to the following question: What parts of human mathematical practice cannot easily be automated, and why? Or on a more positive and pragmatic note: How can human and machine practice be integrated fruitfully in the field of mathematics?

In this paper we will address these two questions by comparing automated and interactive theorem-provers with the practice of human research mathematicians. We will do so by drawing on the results of recent qualitative studies (conducted by ourselves as well as other researchers) investigating how research mathematicians choose mathematical problems and how they attack and work with the problems they have chosen. Especially, we will look at the roles played by embodied cognition (broadly conceived), representational tools and mathematicians’ understanding of social norms and values in the mathematical practice. Furthermore, we will describe the basic ideas behind the current computer tools for formal proof construction (i.e. automated and interactive theorem-provers) and discuss how and to what extent the construction of such tools can be informed by the insights into the practice of research mathematicians such that the strengths and weaknesses of the automated system are best matched by the strengths and weaknesses of the human mathematician.

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THE ALIENATED/SUBJECTIVE CHARACTER OF SCIENTIFIC COMMUNICATION

Session 30G

Congress section(s): B3

In this presentation, we will address some features of scientific communication, which, on the one hand, which we claim are, from a methodological viewpoint, universally valid and, on the other, characterise exchanges between scientific cultures. To do so, we will focus on Peter Galison’s [Galison 1999] notable concept of trading zones and aim at clarifying their definition. We will argue that these zones could be considered ‘alienated forms of scientific communication’ that depend on the characteristics of the actor’s actions. This definition can be understood in the context of Hegel’s concept of alienation [Hegel 2018]. For Hegel, the problem of alienation is that of the spirit that alienates the results of its actions. An analysis of the [Hegel 2018] shows that such results may include those of the activities of self-consciousness that aim at obtaining new knowledge, in particular, in the course of research.

We wish to stress that the history of scientific activity testifies to how such processes have continuously taken place. Our analysis accounts, for example, equally for Kuhn’ paradigm, where the results of knowledge are alienated from a concrete individual. Within the latter framework, the representation of scientific activity is that a researcher has to solve only puzzles. Kuhn’s paradigm accounts for forms of stability in science. This stability is embodied in the concept of ‘normal science’. In accepting this paradigm, Kuhn suggests, the scientific community embraces the idea that the basic and fundamental problems have been solved. The results are alienated from concrete studies. Within a paradigm, the world of scientific and theoretical knowledge loses its agility and ability to develop. It freezes within the established terminology and the forms that were once alienated and thus can be transferred between cultures. As a result, the paradigm prevents the researcher from acting as an independent force that regulates and controls his or her investigations.

Communication regarding the alienated results that are enshrined in scientific texts can be reduced to researcher’s individual work with text. Each individual asks the text her or his own questions. These questions are affected by the cultural tradition, the individual’s knowledge and competencies, and her or his attitudes and abilities. Thus, communication in science can be considered as a continuous process of interactions between alienation and subjectivity, which betrays the assumption of unity of science. Hence, we define trading zones as alienated (and, therefore, intercultural) forms of communication.

Acknowledgements

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INDEPENDENCE AND METASEMANTICS

Session 10B

Congress section(s): C1

One of the key issues in the debate about pluralism in set theory is the status of sentences that are independent of the axioms, such as the Continuum Hypothesis. In this paper, I argue that one can make progress on this issue by appealing to a metasemantic constraint, i.e., a constraint on what determines the meanings of sentences. According to this constraint, one’s intentions and willingness to adjust one’s beliefs are relevant to determining the content of what one expresses. More specifically, a factor, F, is relevant to the meanings of one’s utterance only if one is disposed to adjust one’s relevant beliefs to

F, provided one is rational. I call this thesis ‘Compliance’. Similar principles have been held even by prominent proponents of externalist views, such as Saul Kripke. But such principles have never been explicitly argued for, nor have people drawn any of their important consequences.
After having established Compliance, I apply it to the case of set theory. I start from the observation that set theorists are unwilling to adjust certain beliefs about sets that go beyond the standard axioms to any kinds of factors, for example the belief expressed by ‘The universe of sets is maximally high’. Given that set theorists clearly do possess the concept of set, by Compliance, it follows that they do not possess the concept of set in an externalist way. This leads to a kind of partial descriptivism, i.e. the view that such beliefs also contribute to determining the content of set-theoretic expressions like ‘set’, ‘membership’, and ‘universe of sets’. And this means, in turn, that the view that the content of set-theoretic expressions is given only by the axioms of set theory—the so-called ‘algebraic’ or ‘model-theoretic’ conception of set theory—is untenable. This is hugely important, because the algebraic conception is an underlying assumption in the famous model-theoretic arguments for the indeterminacy of set theoretic language, which form the basis for pluralist views of the universe of sets (so-called ‘multiverse’ views). The algebraic conception, coupled with the mathematical fact that there are many radically different models of the ZFC axioms—for instance, models in which ‘there is an inaccessible cardinal’ is true, models in which it is false, models in which the continuum hypothesis is true, models in which it is false—is supposed to show that all these sentences that are independent from the axioms of ZFC have indeterminate truth-values. If what I am saying is right—that set-theorists are unwilling to adjust more than the axioms, and that these resilient beliefs are content-determining—then this whole argumentative structure for indeterminacy and pluralism collapses.
So, more generally, what we get from Compliance is a program for finding out what, besides set-theoretic axioms, has determinate truth-values in set theory: We have to look at what set-theorists are unwilling to adjust. The questions of whether independent sentences have truth-values, and, if so, how we can come to know their truth values, are among the most pressing questions in the philosophy of mathematics since Gödel’s incompleteness results. As my paper demonstrates, we can make progress on these questions by doing metasemantics.

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A NOTION OF SEMANTIC UNIQUENESS FOR LOGICAL CONSTANTS

Session 26M

Congress section(s): A2

The demarcation problem for the logical constants is the problem of deciding which expressions of a language to count as part of the logical lexicon, and for what reason. Inferentialist approaches to meaning hold that the inferential behaviour of an expression is meaning-constitutive, and that logical expressions are special in that (among other things) the rules that govern their behaviour uniquely determine their meaning. Things become more complicated when properly semantic (model-theoretic) considerations enter the picture, yet the notion of a consequence relation or a set of rules uniquely determining the meaning of a constant is gaining currency among semantic approaches to the question of logical constanthood as well. In this talk we would like to explore the issues a proponent of a semantic criterion of logicality will encounter when adopting the inferentialist conception of uniqueness, and what a properly semantic notion of uniqueness for logicality could look like.
The notion of uniqueness gained importance in the inferentialist approach to the meaning of the logical constants as it constituted a natural complement to the constraint of conservativity -- ruling out Prior’s defective connective tonk (Prior 1960, Belnap 1962), -- cohering with the ‚rules-as-definitions‘ approach pioneered by Gentzen (Gentzen 1934). Identifying the meaning of a logical constant with its inferential role, the demand of uniqueness, in its simplest form, amounted to the requirement that, for constants c, c‘ obeying identical collections of rules, only synonymous compounds can be formed, i.e. that in a language containing both c and c‘ we have that
(UC) $c(A_1, ..., A_n) \dashv\vdash c‘(A_1, ..., A_n)$
Shifting perspective to a truth-theoretic framework, in which the meaning of a logical constant is given by a model-theoretic object, gives rise to interesting issues in the implementation of this kind of notion of uniqueness. For not only are semantic values underdetemined by (single-conclusion) proof-rules (a set of results collectively termed Carnap’s Problem; cf. (Carnap 1943)), it is moreover not immediately clear in what way (UC) is to be ‚semanticized‘. Different ways of conceiving of such a semantic analogue to (UC) can be found in the literature (cf. (Bonnay & Westerstahl 2016), (Feferman 2015), (Zucker 1978)), but a comprehensive comparison and assessment of their relative merits is still outstanding. This is somewhat surprising given the central role the notion of unique determinability by rules plays in approaches to the nature of the logical constants (cf. (Hacking 1979), (Hodes 2004), (Peacocke 1987). This talk aims to investigate and compare some of the different ways in which (UC) could be adapted to a model-theoretic framework, and assesses the adequacy of these different implementations for the demarcation problem of the logical constants.

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“THOUGHT EXPERIMENTS” IN MATHEMATICS?

Session 3M

Congress section(s): B3

The philosophy of thought experiments (TEs) focus mostly on philosophy of science. Few (e.g. Jim Brown, Irina Starikova and Marcus Giaquinto, Jean-Paul Van Bendegem) tackle the question of TEs in mathematics, where, as they point out, it is not at all strange to see our knowledge increase from thought alone.
Jean-Paul Van Bendegem, former opponent of TE in mathematics in (1998), now accepts and even defends the possibility of (specific types) mathematical experiments (2003), by showing several cases in which a highly abstract mathematical result is the outcome of research that has a concrete empirical origin. In recent discussion (PhilMath Intersem 2018, Paris) he draws the conclusion that mathematical experiments are indeed a genuine possibility, but that there is unlikely to be a uniform definition of “thought experiment”, given the heterogeneity of cases in mathematics.
Starikova and Giaquinto, being also against use of the label ‘thought experiment’, narrow mathematical TEs to the most interesting creative candidates from mathematical practice, namely, those in which (a) experimental thinking goes beyond the application of mathematically prescribed rules, and (b) uses sensory imagination (as a way to drawing on the benefits of past sensory experience) to grasp and mentally transform visualizable representations of abstract mathematical objects, such as knots, graphs and groups. Van Bendegem taking into account that the thinking in these examples is primarily aimed at a better understanding and more detailed consideration of abstract constructions than at real experimentation and suggests avoiding the use of the term “thought experiment” as applied to mathematical practice and replacing it with “an aid to proof” or “evidential mediators”.
Bearing in mind that TEs are a rare combination of historical, philosophical, cognitive and social practicies and a very special way of extracting new knowledge, before sweeping away ‘thought experiments’ from mathematical practice I will discuss:
a. Do TEs under conditions (a) and (b) constitute a class we can properly define?
b. Can we define other classes of mathematical thinking practices which may be reasonably be counted as TEs?
c. Should we aim to find a uniform general definition of TEs in mathematics, one which includes various subkinds but excludes anything not accurately described as a TE? Or should we take the notion of TE to be a loose family-resemblance concept, with no proper definition?

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MATHEMATICAL PROVING AS SPATIO-TEMPORAL ACTIVITY OF MULTI-AGENT SYSTEMS

Session 9G

Congress section(s): B2

Our aim is to develop a framework theory of mathematical proving, which is not based on the traditional concepts of mathematical fact and truth, but on the concept of proof-event or proving, introduced by Goguen [2001]. Accordingly, proof is no longer understood as a static purely syntactic object, but as a social process, that takes place at a given location and time, involves a public presentation of a purported solution to a particular problem before an appropriate mathematical community
Sequences of proof-events can be described as activity of a multi-agent system evolving in time. The agents of the system may enact different roles; the fundamental roles are those of the prover (which might be a human or a machine or a combination of them (hybrid proving)) and the interpreter (who generally should be human (person or group of experts) or a machine (or group of machines) or a combination of them). These agents interact between each other at various levels that form an ascending hierarchy: communication, understanding, interpretation, validation.
Different agents may exhibit different capacities (expertise, virtuosities, skills, etc.) during a proving activity, which results in a kind of increased collective capacity directed towards the same-shared goal and enhanced efficiency in achieving the goal (to solve a posed problem).
Proof-events are problem-centered spatio-temporal processes; thus, they have history and form sequences of proof-events evolving in time. The agents might join (or abandon) a proof-event in a sequence of proof-events at a definite time. Accordingly, the system of agents may vary over time.
We attempt to model certain temporal aspect of proof-events, using the language of the calculus of events of Robert Kowalski type [Kowalski, Sergot 1986]. Using the language of the calculus of events, we can talk about proof events and their sequences, evolving in time. The semantics of proof-events follow a logic that can be expressed in terms of Kolmogorov’s calculus of problems [Kolmogorov 1932], initially used for explication of intuitionistic logic [Stefaneas, Vandoulakis 2015].
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SPACETIME: SUBSTANTIVE OR RELATIONAL?

Session 10J

Congress section(s): C2

According to substantivalism spacetime is an autonomous entity. According to the relational view, only material objects really exist, while space and time are nothing else but the spatial and temporal relations among them.
1. The void argument. The following argument, which could be called the “void argument”, stays in support of relationalism: “Let us imagine that all material structures are taken out of space. What would remain then is an empty space. It is a void deprived of matter, and to this effect it turns into an imaginary thing that displays no property of its own. But a thing without any properties is rather nothing than something. So, space does not exist as an entity of its own.”
An objection to the void argument would be a claim that empty space is not deprived of qualities. Let us turn to the figure of a triangle. The sum of the inner angles of every triangle drawn on a plane equals 180 degrees. However, if it is found on the surface of a sphere, this sum is more than 180 degrees. Thus basic properties of triangles may differ, when they reside within different spaces. And this is due to the differences of spaces themselves.
A counter objection may be raised that the qualities of a geometrical space do support a view about some autonomy of geometrical spaces, but not the stronger thesis about the substantive character of the empty physical space (spacetime).
If one presumes that what is taken to be the physical space is correctly represented geometrically by the flat three-dimensional Euclidean space, then the void argument might go well. But the quality of a physical space to be either flat, or to possess a kind of curvature, can affect the motion of material objects. Thus the void argument may well be rejected.
2 The Energy of the empty space-time. If space-time were relational, it could not possess non-relational properties, which are possessed by material systems. But spacetime really exhibits such a property that has obtained the popular name “dark energy”. It is ruling the expansion of the Universe and cosmologists refer it to spacetime itself. Dark energy opposes the effect of mutual attraction among stars and galaxies due to the universal gravitation.
If the nature of space-time were relational, then spacetime could hardly possess such an intrinsic dynamic quality. Energy is a fundamental property of material systems, and they have an existence of their own. So, we must concede that spacetime has also a specific existence of its own.
3. Gravitational waves. Space-time demonstrates yet another feature, which would hardly be conceivable for it to possess, if its nature were relational.
One hundred years after Einstein hypothesized their existence, a large group of scientists announced the first observation of gravitational waves. Relationalism could hardly account for their existence. Relations are relational properties of objects, and as being dependent on the specific configuration among them, they have no existence of their own. But if so, relations cannot possess non-relational properties on their part, and in particular, spacetime could not initiate gravitational waves.

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EASTON'S FUNCTION AND THE TREE PROPERTY BELOW ALEPH$_\Omega$

Session 24B

Congress section(s): A1

Abstract. It is known that the usual large cardinals (we include the assumption of inaccessibility in the definition of a large cardinal) do not have any effect on the continuum function on small cardinals: in particular, if κ is a large cardinal, all we can say in general is that $2^{\aleph_n}$, for any $n < \omega$, is smaller than κ. Things may be different if we consider a cardinal κ which shares some properties with large cardinals (typically some sort of reflection), but it is not inaccessible: it may even be smaller than $\aleph_\omega$, and therefore may have an effect on the continuum function below $\aleph_\omega$. In this talk, we are interested in the *tree property* at κ: we say that a regular cardinal $\kappa > \aleph_0$ has the tree property if there are no κ-Aronszajn trees (κ-trees without a cofinal branch). It is known that if $2^\kappa = \kappa^+$, then there are κ^{++}-Aronszajn trees. Thus the tree property at $\aleph_2$ implies the failure of CH. It is natural to ask whether the tree property at κ^{++} puts more restrictions on the continuum function apart from requiring $2^\kappa > \kappa^+$. We discuss this problem with the focus on the cardinals below $\aleph_\omega$ and show that the tree property on every $\aleph_n$, $2 \leq n < \omega$, is compatibile with any continuum function on the $\aleph_n$'s wich complies with the restriction $2^{\aleph_n} > \aleph_{n+1}$, $n < \omega$. Our result provides a generalization of Easton's theorem for the context of compactness principles at successor cardinals. At the second part of the talk we will discuss extensions of our result to other widely studied compactness principles such as *stationary reflection* and the failure of the *approachability property* and also mention connections to generalized cardinal invariants.

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SYMMETRY, GENERAL RELATIVITY, AND THE LAWS OF NATURE

Session 29L

Congress section(s): C1, C2

In modern physics, the laws of nature are derived from the fundamental symmetries of physical theory. However, recent attempts to ground a philosophical account of natural law on the symmetries and invariances of physical theory have been largely unsuccessful, as they have failed to address the framework-dependence of symmetry and invariance, the disparate nature of physical theory, and the recalcitrance of the natural world (e.g. see van Fraassen, 1898; Earman, 2003; and Brading and Castellani, 2003). In response, this paper provides a detailed study of the mathematical foundation of symmetries and invariances in modern physics in order to present a novel philosophical account of the constitutive role that mathematics plays in grounding the laws of nature.

In the first section of the paper, I provide a discussion of the geometrical connection between symmetries, invariances, and natural law within modern physical theory. In this context, I present an account of Noether's theorem, which states that every continuous symmetry in a set of dynamical equations corresponds to a conserved quantity. In fact, Noether's theorem is often taken to establish the connection between symmetries and conservation laws within modern physics (Butterfield, 2006). However, what is often let out of this discussion is the fact that the symmetries in a set of dynamical equations are not sufficient, on their own, to establish the relevant invariant quantities as the symmetries must also be present in the underlying spacetime structure as well (Shutz, 1999). This leads me to a discussion of the spacetime structures of modern physical theory. Through analyzing these structures, I show that in the case of classical mechanics, quantum mechanics, and special relativity, the spacetime structures are all maximally symmetric and Noether's theorem is sufficient to characterize natural law. But the situation drastically changes when we allow for the possibility of a dynamical non-Euclidean geometry. In general relativity, the symmetries in a set of dynamical equations are not sufficient to establish a conserved quantity, as these symmetries are not typically present in the underlying spacetime structure. In fact, the relevant symmetries often have to be imposed on the spacetime in contradiction to the causality constraint of general relativity. The grounding of natural law in the symmetry structure of physical theory appears to be undermined by general relativity.

In the second section of the paper, I consider one possible solution to this concern that emerges from the fact that the mathematical structures of classical, quantum, and relativity theory are all formulated within a more general mathematical conception of nature characterized by the Lie calculus. I suggest that this mathematical framework may be able to provide a viable foundation to ground a philosophical account of natural law. Here I take my motivation from Feynman (1964, p. 59) who notes that mathematics may act as "a great sweeping principle" from which all laws of modern physics are derived. Despite the fact that each mathematical formalism for physical theory will entail a slightly different conception of symmetry and invariance, they all share a common understanding of natural law grounded in a geometrical-mathematical representation of reality. In this sense, each theory may provide a representation of reality from a particular mathematical vantage point. The common features of the geometrical-mathematical formalism of physical theory may offer the possibility of grounding a viable perspectival account of natural law. To conclude, I consider whether this perspectival account of natural law is able to ground a viable scientific realism, and discuss its broader implications for the philosophy of physics.

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FITNESS INCOMMENSURABILITY AND EVOLUTIONARY TRANSITIONS IN INDIVIDUALITY

Session 17D

Congress section(s): C3

The world of living objects possesses a hierarchical nature. Genes are nested within chromosomes, chromosomes within cells, cells within organisms, organisms within groups, etc. This hierarchical attribute of the natural world is currently considered a consequence of the fact that evolution is a process that not only selects individuals, but also leads to the emergence of higher-level individuals. These events, called evolutionary transitions in individuality (ETIs), consist of mergers of autonomously reproducing units, to the extent that, after an ETI, such units no longer reproduce independently, but jointly, as a single entity. One of the most outstanding examples of an ETI is endosymbiosis, a process during which a host engulfs a free-living bacterium and subsequently (on an evolutionary time scale) transforms it into a part of its body, thus rendering it incapable of a free-living lifestyle. Although this might seem to be a rare event, it is currently established among biologists and philosophers that endosymbiosis has had a tremendous effect on the evolutionary history of species. For instance, the mitochondrion, one of the most important organelles within cells, has an endosymbiotic origin. Due to its extraordinary role in the evolution of species, endosymbiosis has recently been the object of careful study. Specifically, its genetic aspect has been studied intensively. However, the ecological aspect of endosymbiotic events is still poorly understood, especially the question of whether endosymbiosis is a kind of parasitism or, perhaps, mutualism for the endosymbiont. In other words, figuring out whether endosymbiosis reduces or enhances the fitness of the bacterium in comparison to its free-living relatives is a hard nut to crack. Therefore, the popular approach is to argue that endosymbiosis is a kind of slavery, i.e. the endosymbiont is a slave of the host. Although metaphorically this analogy sounds interesting, it has not provided much illumination. The aim of my speech is to show that science can obtain a more precise understanding of the ecological aspects of endosymbiosis, one that transcends shallow analogies. I will do this by using an idea of fitness incommensurability which basically states that it is not always possible to compare the fitness of two objects. As a case study, I will analyse the origin of aphids' endosymbiotic bacteria, Buchnera sp., and show that, in this symbiotic system, inquiring about the fitness benefits to the endosymbiont is not theoretically justified. As a result, I will argue that asking whether endosymbiosis is beneficial or harmful to the bacteria is not always appropriate, and thus, before we start looking for an answer to such a question, we should first determine whether, in a given symbiotic system, it makes sense to pose it at all.

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EMPIRICAL UNDERDERMINATION FOR PHYSICAL THEORIES IN C* ALGEBRAIC SETTING: COMMENTS TO AN ARAGEORGIS'S ARGUMENT

Session 13I

Congress section(s): C2

In this talk I intend to reconstruct an argument of Aristidis Arageorgis(1) against empirical underdetermination of the state of a physical system in a C*-algebraic setting and to explore its soundness. The argument, aiming against algebraic imperialism, the operationalist attitude which characterized the first steps of Algebraic Quantum Field Theory, is based on two topological properties of the state space: being T1 and being first countable in the weak*-topology. The first property is possessed trivially by the state space while the latter is highly non-trivial, and it can be derived from the assumption of the algebra of observables' separability. I present some cases of classical and of quantum systems which satisfy the separability condition, and others which do not, and relate these facts to the dimension of the algebra and to whether it is a von Neumann algebra. Namely, I show that while in the case of finite-dimensional algebras of observables the argument is conclusive, in the case of infinite-dimensional von Neumann algebras it is not. In addition, there are cases of infinite-dimensional quasilocal algebras in which the argument is conclusive. Finally, I discuss Martin Porrmann's(2) construction of a net of local separable algebras in Minkowski spacetime which satisfies the basic postulates of Algebraic Quantum Field Theory.

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NON-CAUSAL EXPLANATIONS OF NATURAL PHENOMENA AND NATURALISM

Session 12F

Congress section(s): B4

The aim of this paper is to assess whether a counterfactual account of mathematical explanations of natural phenomena (MENP) (Baker 2009) is compatible with a naturalist stance. Indeed, nowadays many philosophers claim that non-causal explanations of natural phenomena are ubiquitous in science and try to provide a unified account of both causal and non-causal scientific explanations (Reutlinger, Saatsi 2018). Among the different kinds of non-causal explanations of natural phenomena, MENP are regarded as paradigmatic examples of non-causal scientific explanations (Lange 2013). According to many philosophers, among the unified accounts of scientific explanations that have been proposed so far, the most promising ones are those that try to extend the counterfactual theory of scientific explanations to cover non-causal scientific explanations (Reutlinger 2018). We thus focus on Baron, Colyvan and Ripley (2017) (BCR), since it is one of the most well-developed attempts to provide an account of MENP that is based on a counterfactual theory of scientific explanations. More precisely, we examine BCR counterfactual account of why the shape of honeycomb cells is hexagonal. Such account rests on the idea that through a counterfactual about mathematics, one can illuminate the reason why the shape of the cells cannot but meet an optimality requirement. We firstly analyse whether BCR account is an adequate explanation of cells' shape, and then we assess whether such account would be acceptable to those who wish to adopt a naturalist stance. To do that, we specify what minimal requirements a stance has to meet in order to be defined as naturalist. We show that BCR account of the shape of honeycomb cells is unsatisfactory, because it is focused on the bidimensional shape of the cells, while actual cells are tridimensional, and the tridimensional shape of the cells does not meet any optimality requirement (Räz 2013). We also show that it might be in any case very difficult to make BCR account compatible with a naturalist stance, because of its metaphysical assumptions on how mathematics might constrain the physical domain. We claim that such a kind of "explanations by constraint" (Bertrand 2018; Lange 2013) is incompatible with a naturalist stance, because there is no naturalist account of how such a constrain might obtain.

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POINCARÉ READ AS A PRAGMATIST

Session 17L

Congress section(s): B6

Although there are scant direct connections between Poincaré and the pragmatists, he has been read as one from early on, for example by René Berthelot (1911). Berthelot's idea was to present Poincaré as the most objective of the pragmatists, while presenting Nietzsche as the most subjective. The idea of a book on pragmatism based on two authors neither of whom are typically put in the cannon of pragmatism may seem bizarre, but there is a compelling logic to looking at the extremes in order to define what pragmatism is and to find common themes throughout the movement. Poincaré certainly shares some themes with the pragmatists, especially the idea of a human element in knowledge that can be seen in his theory of the role that conventions play in science. Poincaré also emphatically rejects a metaphysically realist account of truth as correspondence to an external reality. Perhaps wisely, he does not specify precisely what he does mean by truth, but he frequently uses the language of "useful" or "convenient" theories. Of course, for Poincaré there are limits to conventions. First, he holds that conventions are guided by experience so that we are more likely to choose certain alternatives. Second, he directly and forcefully rejects LeRoy's interpretation that conventions are found everywhere in science. Poincaré insisted that there are empirical facts, along with conventions. His position is easily comparable to Dewey's insistence that science is objective even if we reject the metaphysical realist account of representation and hold that values and aims play a role in defining scientific knowledge. Besides clarifying Poincaré's philosophy of science, reading him as a pragmatist puts his writings into a larger context. The development of 20th century philosophy was influenced heavily by dramatic developments in mathematics and physics. Poincaré was a pioneer in incorporating these developments into philosophy of science and his pragmatic attitude towards the development of non-Euclidean geometries and relativity in physics was a profoundly influential contribution to the philosophy of science. The development and professionalization of the philosophy of science is often seen as part of the eclipse of pragmatism. In fact, pragmatic ideas were used in many areas of the philosophy of science and continue to provide guidance in current debates. Indeed pragmatism was always a form of scientific philosophy, maintaining a connection to scientists and philosophers of science. From our current perspective, the pragmatists were right on several issues where they disagreed with the logical positivists. Pragmatists advocated the continuity of inquiry into values and natural science, a type of holism, thoroughgoing fallibilism, and focused on the practice of science, rather than its logical reconstruction. Reading Poincaré as a pragmatist will give us a new perspective on the development of the philosophy of science.

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STABILITY OF TRAITS AS THE KIND OF STABILITY THAT MATTERS AMONG HOLOBIONTS

Session 20F

Congress section(s): C3

Holobionts are biological assemblages composed of a host plus its symbiotic microbiome. Holobionts are pervasive in biology and every macrobe (animal or plant) is believed to host a huge number of microorganism, some of which are known to have a substantial influence on the realized phenotype of the animal. Because of this, a considerable number of biologists have suggested that holobionts are units of selection, i.e. entities that exhibit inherited variance in fitness (Gilbert et al. 2012; Rosenberg & Zilber-Rosenberg 2014; Roughgarden et al. 2018).
Contrary to their claim, some authors have recently argued that holobionts cannot be considered units of selection. They argue that the existence of independent reproductive regimes among the entities that compose the holobiont (the host, on the one hand; and the symbionts, on the other) makes it impossible to talk about holobiont inheritance (Moran & Sloan 2015; Douglas & Werren 2016). In other words, even if there might be phenotypic variation among different holobionts in a population, this phenotypic variation cannot be intergenerationally transmitted from parent to offspring. Or, at least, it cannot be transmitted with the degree of fidelity that would be required for natural selection to act on the holobiont, instead of on the different entities that compose it. Therefore, according to them, the microbiome is, at most, an environmental factor that influences (sometimes substantitally) the phenotype that the host expresses, but which cannot be selected as a unit with the host.
In this talk, building on some recent evidence about holobionts, I elaborate an extended notion of inheritance for symbiotic conglomerates that overcomes the difficulties posed in Moran & Sloan (2015) and Douglas & Werren (2016), which I call “stability of traits” (SoT). I argue that “SoT” is adequate to capture some phenomena of intergenerational preservation of variation among holobionts that could not be captured with the restricted notion of inheritance that the critics use, and thus SoT allows arguing that holobionts are units of selection, at least in some cases.
My talk will be divided in three parts:
- First, I will argue that critics of holobiont inheritance use a very restricted notion of inheritance that equates inheritance with genome replication, a notion I refer to as “stability of species” (SoS). I argue that, since the genome that is replicated and intergenerationally transmitted does not exhaust the full range of factors that have an influence on the expressed phenotype, SoS is insufficient to capture the concept of “inheritance”.
- Second, I argue that the definition of inheritance should be widened to include those factors that are actively acquired and maintained for an organism to express its phenotype, that is, to guarantee SoT. Since these factors are net contributors to phenotype variation in the population they are the raw material among which natural selection can “select”. I argue that the microbiome plays this role in many holobionts, and thus should be considered as “inherited” material. An important aspect of SoT is that it only requires that the phenotypic variation is intergenerationally preserved in the population, and not only between parents and offspring, as some conceptions of natural selection demand (Charbonneau 2014).
- Finally, I will argue that there are reasons to believe that some holobionts satisfy SoT, and thus it is reasonable to considered them as units of selection.

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CREATING EPISTEMICALLY SUCCESSFUL INTERDISCIPLINARY RESEARCH INFRASTRUCTURES: TRANSLATIONAL COGNITIVE NEUROSCIENCE AS A CASE STUDY

Session 29E

Congress section(s): C5

Mental illness and neurodegenerative diseases are widely understood to involve impairments in cognition. During the past two decades, neuroscientists have sought to characterize these impairments and identify the neural mechanisms that give rise to them. A common approach has been to investigate the neural mechanisms of cognitive functions in non-human animals using classic paradigms such as fear-conditioning and the Morris water maze and use findings to ground inferences about the causes of cognitive dysfunction in humans. One obstacle to progress in this research area, however, has been the lack of analogous experimental tools to assess cognitive function and dysfunction in experimental animals and humans. Specifically, it is unclear whether experimental tools used in human and non-human animal studies probe the same cognitive functions.
In this talk, I describe and evaluate a collaborative open-science research initiative, translational cognitive neuroscience (TCN), which began in the 1990s with an eye towards developing a set of complementary experimental tools to investigate cognition in humans and non-human animals. At that time, cognitive neuroscientists were using computer-based touchscreen tasks to assess cognitive impairments in mental illness and neurodegenerative diseases, which were known to correlate with underlying neural dysfunction. With an eye towards facilitating the translation of results from rodent studies to human studies, TCN researchers developed an experimental apparatus for use with rodents, the rodent touchscreen operant chamber and a set of experimental tasks that closely resemble human touchscreen tasks.
In this talk, I argue that the success of these tools for producing such data is contingent on them meeting a number of epistemic requirements (e.g., face validity, replicability, construct validity). Moreover, ensuring that these requirements are met involves an unprecedented amount of coordination within individual laboratories and across research groups. I examine the nature of this coordination and address the question of whether it provides any insights with respect to the correct recipe for cumulative science more generally.“

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LANGUAGE GAMES AND PARADOXES OF DEONTIC LOGIC(S)

Session 6M

Congress section(s): A2

The Ross paradox and the paradox of free choice permission pose problems that have troubled deontic logic for decades and though they were many times pronounced to be solved they still keep returning as ‘alive and kicking’. Both the paradoxes center around the ambiguity which is characteristic of the use of the word “or” in normative/deontic discourse, or, from another viewpoint, on the ambiguity of the concept of disjunction in the related philosophical frameworks.
In my paper, I will first analyze the background of the paradoxes and try to remove some confusions that can be found in the relevant literature. I will argue that if we want to get a grasp on the relevant issues we have to make clear what we aspire to achieve when we strive to build a system of deontic logic. I will then argue that the paradoxes can be solved (or dissolved) removed if we approach the problems from the perspective of a model language game proposed by David Lewis (Lewis 1979a, Lewis 1979b).
Lewis’ language game involves three players: the Master, the Slave, and the Kibitzer. The Master’s moves consist in issuing commands and permissions to the Slave, whose moves consist in making what the Master requires. The Kibitzer’s moves are his descriptions of the normative situation. Situations (or possible worlds) conforming to the Master’s commands and permissions together create the Sphere of Permissibility. At the start of the game, the sphere of permissibility does not differ

from the Sphere of Accessibility, i.e. the space of all possible situations (worlds) that come into consideration as alternatives to the actual world of the language game.
To manifest that that the languages of the players are different, I will suppose that the Master only uses sentences in the imperative mood and permissive sentences that (typically) employ the phrase "you may...". The Kibitzer, on the other hand, has in his repertoire only sentences describing the normative situation, i.e. statements to the effect what the Slave is obliged (must), is forbidden (must not) or is allowed (may) to do.
I will argue that thought both the language of the Master and the language of the Kibitzer are governed by certain logical rules we shouldn't suppose that the rules are identical or parallel. The main moral of the paper is that deontic logic should not be seen as a homogenous discipline but as a complex of complementary logical systems that focus on different linguistic discourses and have different aspirations.
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A BIMODAL LOGIC OF CHANGE WITH LEIBNIZIAN HYPOTHETICAL NECESSITY

Session 25L

Congress section(s): A2

We present a new extension of LCS4 logic of change and necessity. LCS4 (formulated in [2]) is obtained from the LC sentential logic of change, where changeability is expressed by a primitive operator (C... to be read: it changes that ....). LC was mentioned as a formal frame for a description of the so-called substantial changes analysed by Aristotle (disapearing and becoming of substances) ([1]). Another interesting philosophical motivation for LC (and LCS4) comes from the philosophy of Leibniz. In this case the considered changes concern global states of compossible monads. Both LC and LCS4 are interpreted in the semantics of linear histories of dichotomic changes. In the case of LCS4, in addition to the concept of C change, there is considered a second primitive notion: unchangeability represented by $\Box$. LC and LCS4 are complete in the intended semantics (proofs in [1], [2]). Semantically speaking, the subsequent changes cause the rise and passage of linear time. This is a well-known concept of both Aristotle and Leibniz. An idea to link LCS4 with the Leibnizian philosophy of change and time encourages us to introduce in its semantics the concept of a possible world understood as a global description of compossible atomic states of monads. A possible global state of compossible atomic states: j, k, l, ... of monad m may be represented by a conjunction of sentential constants: $\alpha^m_j \wedge \alpha^m_k \wedge \alpha^m_1 \ldots$. For every monad there may be considered many different global states, but only one of them is actual. The possible worlds of m which are not in contradiction with the actual world of m are mutually accessible. These actual atomic states of m, which occur in all possible worlds of m which are accessible from the actual one, are necessary in the sense of our new modal operator N$\Box$. If any actual state of m occurs in at least one possible world of m accessible from the actual one, we say that it is possible in the actual world in sense of N$\Diamond$. Regardless of simultaneous competitive global states of m, each of them may change in a sense of C or may be unchangeable in a sense of $\Box$. Our new semantics contains many linear possible histories of C changes and $\Box$ durations, which generate the flow of time. At the syntactical level we extend LCS4 by new axioms containing primitive symbols N$\Box$, C, $\Box$. We prove a completeness theorem for our new logic. In this frame we can also explicate the specific concept of the Leibnizian hypothetical necessity. It may be said that these sentences are hypothetically necessary, which are general laws of possible worlds 'analogous to the laws of motion; what these laws are, is contingent, but that they are such laws is necessary' [3, 69]. In our terms a sentence α is hypothetically necessary in a possible world w iff α is unchangeable in some world accessible from w, this means: N$\Diamond\Box\alpha$ is true in w.
[1] Świętorzecka, K., Czermak, J., (2012) "Some Calculus for a Logic of Change", Journal of Applied Non-Classical Logic, 22(1):1–8; [2] (2015) "A Logic of Change with Modalities", Logique et Analyse, 232, 511–527; [3] Russell, B., (1900), A critical exposition of the philosophy of Leibniz, Cambridge University Press.

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SOME PROBLEMS IN THE PREDICTION VS ACCOMMODATION DEBATE

Session 4I

Congress section(s): B1

A perennial problem in the philosophy of science concerns the relative epistemic value of predicted evidence versus accommodated evidence. The question is, should we have more confidence in a scientific theory if it successfully predicted some evidence than if it was merely designed to accommodate that evidence? Predictivists think so, but they have had a hard time of producing a convincing argument for why this should be so. Numerous theories on the case have been proposed, both for and against predictivism. As it stands, most of the current defenses of predictivism fall under the banner of "weak predictivism". Weak predictivist theories argue that prediction doesn't have any inherent epistemic advantage over accommodation, but that sometimes it does have more value indirectly because it correlates more strongly with some other virtue that is epistemically relevant. In cases where this other virtue isn't directly evaluable, the prediction-accommodation asymmetry emerges.
Weak predictivist theories include both agent-centered and purely content-centered approaches. The agent-centered approaches, such as those presented in White (2003) and Barnes (2008), argue that prediction is more valuable than accommodation because it tells us something about the reliability of the agent making the predictions. These inferences about the agent then redound to provide support for the theory he or she is advancing. Purely content-centered theories, on the other hand, such as advanced by Worrall (2014), argue that the prediction-accommodation issue is purely a matter of the logical relation between the theory and the evidence. A theory is supported by the evidence if the evidence is not used in the construction of the theory. The time when the theory is constructed and the evidence is discovered is wholly irrelevant.
I argue that both the agent-centered and purely content-centered theories face problems that show that they alone are incapable of accounting for the epistemic role that prediction plays in science. The agent-centered theories make the epistemic support that theories gain contingent on conditions external to the contents of the theory and its evidential consequences. The problem is, a theory can be well justified by its evidential consequences even if the external conditions that the agent-centered theories require are not met. A purely content-centered theory, on the other hand, is unable to account for the fact that sometimes external conditions do come to bear on the epistemic justification of scientific theories, and they do so in a way that produces a genuine epistemic asymmetry between prediction and accommodation. An adequate theory on the prediction vs. accommodation case must avoid the shortcomings of both agent-centered and purely content-centered theories.
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GÖDEL'S AND POST'S PROOFS OF INCOMPLETENESS

Session 29C

Congress section(s): C1

In the 1920s, Emil Post worked on the questions of mathematical logic that would come to dominate the discussions in the 1930s: incompleteness and undecidability. To a remarkable degree, Post anticipated Gödel's incompleteness theorem for

‚Principia Mathematica', but did not attempt to publish his work at the time for various reasons. Instead, he submitted it for publication in 1941, adding an introduction and footnotes discussing how his results relate to the ones of Gödel, Turing and Church. In the Introduction, written in 1941, Post declared that "[t]here would be a little point in publicizing the writer's anticipation [...] merely as a claim to unofficial priority." Although he saw his own work as "fragmentary by comparison" he emphasized that

"with the ‚Principia Mathematica' as a common starting point, the roads followed towards our common conclusions are so different that much may be gained from a comparison of these parallel evolutions."

However, his submission was declined for publication. In the rejection of March 2, 1942 Weyl wrote:

"I have little doubt that twenty years ago your work, partly because of its then revolutionary character, did not find its due recognition. However, we cannot turn the clock back; in the meantime Gödel, Church and others have done what they have done, and the American Journal is no place for historical accounts".

Post's paper, ‚Absolutely Unsolvable Problems and Relatively Undecidable Propositions - Account of an Anticipation', was published only posthumously in Davis' (1965).

Altough it might be claimed that both Gödel and Post formalize the same informal idea, a diagonal argument, we agree with Post, that there is a lot to learn from a careful comparison of the two, quite different, formal proofs.

Examining their proofs presented in strikingly different formal frameworks, we distill and emphasize two key dissimilarities between the proofs of Incompleteness by Gödel and Post. The first considers the scope and generality of their proofs. Gödel was dissatisfied with the specificity of his (1931), i.e. being tied to ‚Principia Mathematica' and related systems. On the other hand Post took a purely syntactic approach which allowed him to characterize a much more general notion of formal systems which are shown to be affected by the incompleteness phenomena. The second dissimilarity arises from the fact that Post was first and foremost interested in the decidability of symbolic logics and of ‚Principia Mathematica' in particular. As a consequence he arrived at incompleteness as a corollary to undecidability. This "detour," compared to Gödel's more direct proof of incompleteness, convinced Post that his characterization of formal systems is not only very general, but gives the correct characterization. The argument that this characterization is the correct one mirrors how Kleene convinced himself that lambda-definability was a right characterization of "effective calculability".

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INTRINSIC, EXTRINSIC, AND THE CONSTITUTIVE A PRIORI

Session 15J

Congress section(s): B4

On the basis of what I call physico-formalist philosophy of mathematics, I will develop an amended account of the Kantian–Reichenbachian conception of constitutive a priori. It will be shown that the features (attributes, qualities, properties) attributed to a real object are not possessed by the object as a "thing-in-itself"; they require a physical theory by means of which these features are constituted. It will be seen that the existence of such a physical theory implies that a physical object can possess a property only if other contingently existing physical objects exist; therefore, the intrinsic–extrinsic distinction is flawed.

The paper is available from here: http://philsci-archive.pitt.edu/15567/

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THE OPEN DIHYPERGRAPH DICHOTOMY FOR DEFINABLE SUBSETS OF GENERALIZED BAIRE SPACES

Session 24B

Congress section(s): A1

Abstract. The open graph dichotomy for a given set X of reals [1] is a generalization of the perfect set property for X which can also be viewed as a definable version of the Open Coloring Axiom restricted to X. Feng [1] showed that it is consistent relative to the existence of an inaccessible cardinal that the open graph dichotomy holds for all sets X of reals that are definable from a countable sequence of ordinals. In [2], higher dimensional versions of the open graph dichotomy are introduced and several well-known dichotomy theorems for the second level of the Borel hierarchy are obtained as special cases of the ω-dimensional version. We study the uncountable analogues of the open graph dichotomy and its higher dimensional versions for the generalized Baire space ${}^\kappa\kappa$. Given a subset X of ${}^\kappa\kappa$ and a set D of size at least 2, we let $OGD(\kappa, D, X)$ denote the following statement: if H is a D-dimensional box-open dihypergraph on X then either H has a coloring with κ many colors, or there exists a continuous homomorphism from a certain specific "large" D-dimensional dihypergraph to H. (When $D = 2$, the existence of such a continuous homomorphism is equivalent to the existence of a κ-perfect subset Y of X such that the restriction of H to Y is a complete graph.) We extend Feng's above mentioned theorem to the generalized Baire space ${}^\kappa\kappa$ and also obtain higher dimensional versions of this result. Namely, we show that for any infinite cardinal κ with $\kappa^{<\kappa} = \kappa$, the following statements are consistent relative to (and are therefore equiconsistent with) the existence of an inaccessible cardinal above κ. 1. $OGD(\kappa, D, X)$ holds for all sets D of size $2 \leq |D| < \kappa$ and all subsets X of ${}^\kappa\kappa$ which are definable from a κ-sequence of ordinals. 2. If a subset X of ${}^\kappa\kappa$ is definable from a κ-sequence of ordinals, then (κ, κ, X) holds restricted to a certain class of κ-dimensional box-open dihypergraphs H on X. This class includes those box-open dihypergraphs H on X which are definable by Σ^1_1-formulas over ${}^\kappa\kappa$.

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SORTAL INTERPRETATION OF ARISTOTELIAN LOGIC

Session 28H

Congress section(s): A4

Though Aristotelian logic is interpreted as just a minor part of the canonical modern extensional logic, namely, first-order logic, there still remains one point deliberately left aside and unmentioned. That is the assumption of non-empty terms. Without this assumption, Aristotelian logic as a part of modern extensional first-order logic lost its essential features: the square of opposition collapses and several hitherto valid modes of syllogism yield invalid patterns of inference. The reason, as it is usually claimed, is that first-order logic does not require the non-emptiness of terms and in this sense is much broader than Aristotelian logic. However, this could be put to doubt with some success, for any possible grammatical subject of a first-order logical formula either denotes an object in the domain (constant) or ranges over a non-empty domain of individuals and therefore in a certain sense the latter logic also requires non-emptiness of its terms. I will follow the idea that the key lies in the "nature" of the domain of individuals. The point is to divide the domain into the so-called "sorts". The logic which studies the domain of quantification conceived in such a way is called the sortal quantification theory.

The key concept of the sortal quantification theory is, obviously, the concept of a sortal. The simplest and widely accepted interpretation of sortals is that they provide a criterion for counting items of a kind, as it is in Cocchiarella's definition of

a sortal concept – “a socio-genetically developed cognitive ability or capacity to distinguish, count and collect or classify things” (Cocchiarella 1977). Now, the sortal quantification theory introduces a so-called sortal quantification. The key idea could be stated as follows: in a sentence e.g. all men are mortal, its canonical interpretation in first-order logic tells us that for every object it is the case that if the object is S, then the object is also P. This sounds somewhat strange, because the original sentence does not seem to be “about” all objects. In the sortal quantification theory, the original sentence is reformulated in a way that the universal quantifier does not quantify over all individuals, but over the individuals which fall under S, put briefly, quantifies over all Ss. Moreover, in some versions of the sortal quantification theory, sortals are subordinated to other sortals, so we can build a hierarchy of sortals. Every sortal is subordinated to some ultimate sortal, i.e., a sortal which is subordinate to no other sortal. To sum up, with the help of the apparatus of the sortal quantification theory, it seems possible to reconstruct Aristotelian logic without losing its essential features. So, sorted universe of discourse and sortals can be successfully implemented into modern logic. What remains a problem, however, is the philosophical motivation and justification. So, at the conclusion, I would like to show that Aristotelian ontology, especially his theory of categories, could provide a satisfactory philosophical background for the questions of definition and relevance of sortals and generally for the belief that there are metaphysically distinct kinds of entities.

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LOOKING AT BOLZANO’S MATHEMATICAL MANUSCRIPTS.

Session 19B

Congress section(s): B6

Large parts of Bolzano’s mathematical manuscripts are today published in the Bernard Bolzano-Gesamtausgabe (BBGA), the most important of them being several volumes of the Grössenlehre (GL) containing Einleitung in die GL und Erste Begriffe, Reine Zahlenlehre, and Functionslehre. The manuscripts of GL also contain fragments of algebra and of the theory of series, and a beautifully written complete text Raumwissenschaft. A small volume Zahlentheorie appeared as Bolzano’s Schriften, vol. 2, Prague 1931, which is in fact a part of the future volume 2A9 of the BBGA, Verhältniss der Theilbarkeit unter den Zahlen. Many of the manuscripts are preliminary sketches or auxiliary notes of later published works.
Bolzano’s earlier manuscripts (1810-1817) are on the one hand a continuation of the Beyträge where appears the concept of the possibility of thinking together (Zusammendenkbarkeit), yielding the concept of whole or system, on the other hand similar contributions to the foundation of mathematics with the concepts of collection (Inbegriff), of number, of quantity (Grösse), of imaginary (=complex) number and of infinity, and those of analysis and of geometry (several developments about the theory of parallels). Bolzano returns to these subjects very often in his mathematical diaries, which are an exceptional source for the study of the state of mathematical knowledge in the first half of the 19th century.
Eventually, Bolzano’s manuscripts contain important extracts, comments and annotations of the books he studied, e.g. those of Carnot (Géométrie de la position), Wallis, Wolff, Kästner, Legendre, Lagrange (64 pages of the summary of the Théorie des fonctions analytiques), Laplace, and Gauss among others.

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UNDERSTANDING SCIENTIFIC INQUIRY VIA AGENT-BASED MODELING

Session 8E

Congress section(s): B3

Computational modeling has in recent years become an increasingly popular method in philosophy of science and social epistemology. In this talk I will discuss the role of simulations of scientific inquiry in the form of agent-based models (ABMs), which are at the heart of this trend. I will start by arguing that a primary function of ABMs of scientific inquiry developed in philosophy of science---in contrast to ABMs in empirical sciences---is to contribute to our understanding of the process of inquiry and factors that may have an effect on it. In view of this, I will defend two specific ways in which ABMs can increase our understanding of science: first, by providing novel insights into socio-epistemic factors that may have a significant impact on the process of inquiry, and second, by providing evidence for or against previously proposed explanations of concrete historical episodes. I will illustrate each of these functions by a set of ABMs, which my collaborators and I have developed. While these models are abstract and highly-idealized, I will show how the results obtained from them can be analyzed in terms of their robustness and empirical validity.

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THOU SHALT NOT NUDGE: TOWARDS AN ANTI-PSYCHOLOGICAL STATE

Session 13J

Congress section(s): C7

The neoclassical economics defines market failures as an uncompensated impact of one agent’s actions on the other agents’ well-being. The favored solution is the use of economic incentives like taxes and subsidies to correct these situations. Recently, the findings of behavioral economists have provided support for the argument that market failures should also comprise the cases where individuals harm themselves due to systematic mistakes they make (Sunstein 2014; Allcott and Sunstein 2015). Also, the set of regulatory tools should be expanded beyond economic incentives towards the use of subtle manipulation of the choice architecture (Thaler, Sunstein, and Balz 2014).
I argue that both of these steps would serve to increase the arbitrary power of the government and the fragility of the liberal democratic institutions. While it is easy to muster intuitive support for the claim that exploitation of systematic mistakes in decision-making is an inherent feature of the free market exchange (Akerlof and Shiller 2015), no one has yet succeeded in establishing a coherent and practically useful notion of ‘true preferences’ against which these mistakes could be defined (Sugden 2018). Thus, the concept of market failure due to self-harm is vague. Therefore, the government interventions to prevent these failures lack a general theoretical framework and, where applied, proceed on an ad hoc basis. Moreover, as far as individuals’ choice is no longer to be taken at face value, the voters’ choices can be contested at least as easily as the consumers’ choices (Brennan 2016). Use of nudges instead of economic incentives to bring people’s choices closer to their nebulous true preferences lowers the transparency of the intervention and increases the temptations to misuse it to strengthen the incumbents’ hold on political power (Schubert 2017).
I propose to use the government’s regulatory power to preempt the most dangerous manipulative techniques rather than to engage the government in them. Such ‘anti-psychological’ role has significant advantages. Regulation of the forms of commercial (and political) communication can capitalize on the scientific knowledge of human cognitive limitations, and yet avoids the necessity to establish what the true preferences are. It also takes a form of general rules which are more transparent than measures that need to target particular situations.
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IMPACT OF TEACHING ON ACCEPTANCE OF PSEUDO-SCIENTIFIC CLAIMS

Session 14D

Congress section(s): B7, C7

Can teaching have any impact on students‘ willingness to embrace pseudo-scientific claims? And if so, will this impact be significant. This paper aims to present an ongoing research conducted in two countries and four universities which aims to answer these questions. The research is based on a previous work McLaughlin & McGill (2017). They conducted a study among university students which seems to show that teaching critical thinking can have a statistically significant impact on the acceptance of pseudo-scientific claims by students. They compared a group of students that attended a course on critical thinking and pseudo-scientific theories with a control group of students who attended a course on a general philosophy of science using the same questionnaire containing the pseudo-scientific claims. The questionnaire was administered at the onset of the semester (along with a Pew Research Center Science Knowledge Quiz), and then at the end of the semester. While there was no significant change in a degree of belief in pseudo-scientific claims in the control group, the experimental group showed a statistically significant decrease in belief in pseudo-scientific claims.
In the first phase of our research, we conducted a study similar to that of McLaughlin & McGill, though we were not able to replicate their results. There was no significant change in belief in pseudo-scientific claims among the study’s participants. This, in our opinion, is due to the imperfections and flaws in both our and McLaughlin & McGills studies.
In this paper, we would like to present our research along with the results obtained during its first phase. We will also discuss the shortcomings and limitations of our research and the research it is based on. Finally, we would like to present and discuss future plans for the next phase of our research into the teaching of critical thinking and its transgression of critical thinking in cases focusing on humanities and science.
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MENDELEEV’S DEDICATED SUPPORTER AND FRIEND. THE CZECH CHEMIST BOHUSLAV BRAUNER AND THE WORLDWIDE RECEPTION OF THE PERIODIC SYSTEM

Session 26G

Congress section(s): B6, C2

Bohuslav Brauner (1855-1935), pupil of Robert Bunsen in Heidelberg, and Henry Roscoe in Manchester, was appointed extraordinary professor of inorganic chemistry in 1890 and full professor in 1897 at the Czech Charles-Ferdinand University in Prague. As early as in the 1870s, when he was still student at the Prague University, Brauner became enthusiastic promoter of the Periodic System. His contacts with Mendeleev started at Brauner’s initiative in 1881, when he sent to the Russian chemist a letter with the reprint of a paper published jointly with his English colleague John I. Watts. In this article Brauner referred to Mendeleev’s “Osnovy khimii”, and “expressed regret that this excellent treatise was quite unknown in western countries”. Mendeleev answered with a long letter and sent Brauner his photograph. This was the beginning of their correspondence, cooperation, and personal encounters that lasted until Mendeleev’s death in 1907. Brauner devoted his life-long research to the exemplification and perfection of Mendeleev’s Periodic Law, especially to the placement of the rare earth in the Periodic Table, estimation and revisions of atomic weights of some elements and (unsuccessful) search for new elements predicted by the Periodic Table. His publicizing of the Periodic System and international scientific authority led to the almost unconditional acceptance of Mendeleev’s system in the Czech chemical community as early as in the 1880s, and supported the process of its dissemination in other European countries and even in Japan. Brauner’s motivations were not only scientific; they also had political and social connotations due to the rising anti-German nationalism in the Czech society where the prevalent Russophilia played an important role in the reception of Mendeleev’s teaching.

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REFLECTIONS ON THE TERM “PHILOSOPHICAL LOGIC”

Session 13M

Congress section(s): A2

I will discuss questions “What they call and what must be named a “philosophical logic”, and is it possible and relevant”. After analyze the usage of term “philosophical logic” and objection against its usage I will explain my conception about “philosophical logic”
The main questions are “Are there a significant field of study for what there is no suitable term”, “where is this field of study named “philosophical logic”, “Is “philosophical logic” a (kind of) logic, or it is philosophy but not logic.
There are 2 main reasons for the term “philosophical logic” – “Scientific and Theoretical” and Social and practical.
Theoretical reasons - There are a significant problem fields in XX-th century logic. New philosophical problematic was developed because of the paradoxes in set theory and the limitative theorems (Tarski, Gödel). They necessitated the elaboration of new philosophical and conceptual investigation into the methods, nature and subject of mathematics and logic and the broad epistemological topics connected with them. But the really new research field in logic was non-classical logic. The non-classical logic and especially modal logic became central topic of logical research in the Xx-th century and by the same token was considered as highly significant for philosophy.
Social and practical reasons - Approving a such term is convenient for a two group of scientist for their work and career.
- Philosopher with good knowledge in some other fields - ontology, epistemology, philosophy of science, with traditional training in logic.
- Scientists with good skills in formal (mathematical) method, frequently with firmly mathematical education working in the field of non-classical logic’s, which find job as logic lecturers in philosophical departments. Non-classical logic’s are not related to the logic of mathematics, they (with the exception of intuitionistic logic) do not serve as the basis of mathematical theory, that is why most mathematicians for a long time were not interested in non-classical logic’s. In mathematics there is no contextual ambiguity and modality, such notions are not interesting for the mathematicians. This led to the employment of logicians interested in such problematic in philosophical faculties.
And also term sounds impressive and hardly provokes objection from deans and foundation.
Objections
The main objections against the term “Philosophical logic” are “It in unnecessary - “Logic” is well enough in all cases.”; ”Which problems belong to logic but not to “Philosophical logic”, “are papers of Aristotle’s, (Frege. Hilbert) “philosophical logic”?”; “If Logic is a part of philosophy, so why we must Restrict logic through the more general concept “Philosophy” “.
I see four expressive interpretations of the term “Philosophical logic”
-Philosophical logic as (some types of) logic and studding logical systems with connection to -philosophy. Especially it as the logic, which investigate nonmathematical reasoning.

-“Philosophical logic” is “The logic in (of) philosophy” and explores the rules of the logical inference, the modes of deduction from and in philosophy.
“Philosophical logic” as “Philosophy in logic”;
-“Philosophical logic” as “Philosophy of logic”.

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HOW TO BUILD A COMPUTATIONAL NOTION OF TRUST

Session 19F

Congress section(s): B2

Many disciplines recognize the importance of trust for the emergence and development of collaborative behaviours. However, being a multi-faceted concept, trust always defiled a comprehensive analysis that could define its core features and thus identify it as a clear notion. This aspect is highly problematic when the concept has to be modelled and, successively, implemented in formal environments. Therefore, it comes with no surprise that there is little consensus, in the computer science literature, on the nature of computational trust and how to properly model it. Even though disagreements in scientific research are not rare and neither exceptionally troublesome in most cases, the lack of a unified conceptualization of the notion of trust is a big issue when it is realized that social interactions are gradually transitioning from the physical realm to the digital one. In digital environments, all the trust-relevant biological traits that human beings intuitively identify are missing. Trusting or not can’t be a matter of instinct anymore and effective mechanisms to establish trust relationships must be explicitly implemented in the design of the digital systems. Those mechanisms can then aid the users to consciously assess whether to trust or not another user during interactions. This talk is structured in two parts. In the first part, a conceptual analysis of the notion of trust is provided, in order to obtain general features common to socio-economical researches on trust. Starting from previous generalist analysis of trust, various discipline-specific studies of trust are summarized and then merged into a unified theory. This theory is then review with respect to laboratory experiments on trust. In the second part, the set of core features attributed to trust by the theory presented in part one is assessed and adjusted with respect to standard paradigms employed to build digital communities. In particular, focus will be put on the Beta paradigm and on the assumptions placed at the base of the EigenTrust algorithm. This adjusted set of features of trust will then help in defining a suitable computational notion of trust. This computational notion of trust is then, finally, used to build a logical language. The logical language will be a modal logic augmented with trust operators on formulas. The semantics of such language will be given in terms of augmented neighbourhood semantics with an added trust structure. Decidability results for the language will be proved and possibilities for computational implementations of the language are taken into consideration. Even excluding the actual model built and the proposals made, all researchers in the area of computational trust can greatly benefit from the methodological insights presented in the talk on how to construct computational versions of social notions. Therefore, the talk achieves two goals: i) it builds a methodology suited to build formal version of social notions; ii) it provides deep insights on the notion of trust; iii) it presents an actual logical language employable to reason about trust.

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EXAMINATION OF THE LINKAGE BETWEEN BAYESIAN PHILOSOPHY AND STATISTICS FROM A LOGICAL POINT OF VIEW

Session 30F

Congress section(s): C1

Recent Bayesian statisticians emphasize that prior specification as subjective or objective is meaningless (Gelman, A. and Hennig, C. 2017). According to their suggestion, the concepts of subjectivity and objectivity should be replaced with other concepts such as transparency, consensus, and so on.

Their suggestion seems feasible, but there still remains a problem. What is the linkage between Bayesian philosophy and statistics? To answer this question, I think one of the promising ways is to focus on the logical relations between them. Although Bayesian philosophy and statistics are often associated with inductive inference, its meaning is in many cases not so clear. In order to reconstruct a good linkage between the two Baysianisms, we need to think about on what logical basis, specifically on what inductive logic, Bayesianism can be seen established.
Historically, this kind of attempt was made for the first time by Carnap (but this might be traced further back to Laplace in his famous ‘rule of succession’). As is well known, Carnap’s system of inductive logic consists in the idea that deductive inference can be defined as a special case of inductive inference (Carnap, R. 1971, 1980). And what is important, his argument indicates not only the logical or empirical assumptions which seem to be implicitly used in our inductive inferences, but also hints a close relation which could be found between inductive logic and – Bayesianim, through his confirmation measure c, orλ-continuum. Recently, following this line of argument, Festa explicated logical and empirical conditions necessary for choosing prior distribution in Bayesian statistics, and tried to show a parallel relation (or rather, reductive relation) between Bayesian statistics and Carnap’s system of inductive logic, which holds in the case of multinomial distribution (Festa, R. 1993).
In this talk, I examine how this attempt can be extended any further. There are two stages of the extension, if it is possible. First, we can try to extend Festa’s argument to other cases of likelihood than multinomial distribution (in this latter case, the prior distribution is fixed to Dirichlet distribution). And second, which would be more difficult, we can try to extend this to the more general Bayesian philosophy, in which priors are our degrees of belief. A complete reduction of Bayesianism to a system of inductive logic is very hard to achieve, but if we can show some parallelism which holds between Bayesianisms as a whole and Carnapian logic, then that would be a great help to the reconstruction of the linkage between two Bayesianisms.
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THE PROBLEM OF RULE-CHOICE REDUX

Session 7A

Congress section(s): B1

In this paper, we tackle the contribution that history of science can make to the problem of rule-choice, i.e., the choice from among competing methodological rules. Taking our cue from Larry Laudan’s writings, we extensively discuss what we call historicist naturalism, i.e., the view that history of science plays a pivotal role in the justification of rules, since it is one source of the evidence required to settle methodological controversies. As we illustrate, there are cases of rule-choice that depend on conceptual considerations alone, and in which history of science does not factor. Moreover, there are cases in which methodological change is prompted – and explained – by empirical information that is not historical in nature: as suggested by what we call scientific naturalism, the justification of methodological choices comes from our knowledge of the structure of the world, as expressed by our currently accepted scientific theories. As we argue, due to its backward-looking character, historicist naturalism does not satisfactorily deal with the case of newly introduced rules, for which no evidence concerning their past performance is available. In sum, we conclude, the contribution that history of science can make to rule-choice is more modest than Laudan suggests.

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THE PROBLEM OF CAUSAL INFERENCE IN CLINICAL PSYCHOANALYSIS: A RESPONSE TO THE CHARGES OF ADOLF GRÜNBAUM BASED ON THE INDUCTIVE PRINCIPLES OF THE HISTORICAL SCIENCES.

Session 11M

Congress section(s): C7

Aside from a therapy, clinical psychoanalysis can also be a method to produce knowledge on prominent human dimensions, such as mental suffering, sociability and sexuality. The basic premise that justifies clinical psychoanalysis as a research device is that the psychoanalyst's neutral and abstinent questioning would promote the patient's report of uncontaminated mental phenomena, that is, mental phenomena unregulated by immediate social demands. The method should draw out evidence for the inference of particular causal relations between the patient's mental representations, mainly memories and fantasies, and between these and the patient's actions and emotions. In his epistemological critique on psychoanalysis, Adolf Grünbaum claims that the formalization of this method by Sigmund Freud does not present the conditions to cogently test causal hypotheses. Two of Grünbaum's arguments specifically against the logic of causal inference in clinical psychoanalysis are analysed, the argument around inference based on thematic kinship and the one around post hoc ergo propter hoc fallacy. It is defended that both arguments are valid, but also that their premises are artificial. These premises are confined to the Freudian text and disregard the potential that the Freudian method has of becoming cogent without losing its basic features. Departing from these arguments, this work discusses the epistemological potential of the method of causal inference in clinical psychoanalysis by describing some of its inductive principles and by exploring the justification of these principles. This work reaches the conclusion that the inductive principles of clinical psychoanalysis and the ones of the historical sciences are similar in the sense that they all infer retrospectively to the best explanation with the support of "bootstrapping" auxiliary hypotheses and they all make general inferences through meta-analysis of case reports. In the end, this work discusses some responses to the justificatory burden of these inductive principles in the context of clinical psychoanalysis.

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STUDYING ACTIONS AND IMPERATIVES IN MATHEMATICAL TEXTS

Session 15A

Congress section(s): C1

In this paper, we examine words relating to mathematical actions and imperatives in mathematical texts, and within proofs. The main hypothesis is that mathematical texts, and proofs especially, contain frequent uses of instructions to the reader, issued by using imperatives and other action-focused linguistic constructions. We take common verbs in mathematics, such as "let", "suppose", "denote", "consider", "assume", "solve", "find", "prove" etc. and compare their relative frequencies within proofs, in mathematical texts generally, and in spoken and written British and American English, by using a corpus of mathematical papers taken from the ArXiv. Furthermore, we conduct 'keyword' analyses to identify those words which disproportionately occur in proofs compared to other parts of mathematics research papers.
Previous analyses of mathematical language, such as those conducted by de Bruijn (1987) and Ganesalingam (2013), have largely been carried out without empirical investigations of actual mathematical texts. As a result, some of the claims they make are at odds with the reality of written mathematics. For example, both authors claim that there is no room for imperatives in rigorous mathematics. Whether this is meant to be a descriptive or normative claim, we demonstrate that analysing the actual writings of mathematicians, particularly written proofs, shows something quite different. Mathematicians use certain imperatives far more frequently than in natural language, and within proofs we find an even higher prevalence of certain verbs.
The implications of this are that mathematical writing and argumentation may be harder to formalise than other linguistic accounts of it suggest. Furthermore, this backs the idea that proofs are not merely sequences of declarative sentences, but instead provide instructions for mathematical activities to be carried out.

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THE IRRELEVANCE OF THE AXIOM OF PERMUTATION

Session 18M

Congress section(s): A2

The axiom of Permutation (A→(B→C))→(B→(A→C)) is valid in many relevant logic systems such as R. Although Permutation has not been particularly problematic I consider that there are good relevantist reasons to distrust this axiom. Thus, I am interested in investigating if Permutation should be relevantly valid.
There has been previous research on the matter. In "Paths to triviality", Øgaard shows how different principles could lead to triviality of naïve truth theories of paraconistent relevant logics. It is important to note that Øgaard's proofs regard rules and not axioms and therefore his results only assess the consequences of having an instance of the rule as part of the theory. Amongst the proofs one can find the combination of the principle of Excluded Middle and the rule of Permutation. In "Saving the truth schema from axioms", Field characterices a logic by adding a conditional that avoid paradoxes such as Curry's to Kleene's logic and the resulting logic does not validate the axiom of Permutation.
Despite the counterexamples in the natural language and Field's results, Permutation still satisfies the usual relevantist properties.

Variable sharing property (VSP): A logic L has the VSP iff in any theorem of L of the form A→B, A and Bshare at least one propositional variable.
Effective Use in the Proof (EUP): In every theorem of the form A1→(...(An→B)...), each Ai is used to prove B.
Permutation also satisfies stronger versions of these properties. However, I think there is a way of understanding the VSP that does not validate Permutation. I want to suggest that one could state a relevantist principle such as the ones mentioned above in which Permutation is not valid. The principle is the following:
Non-Implicative Extremes Property (NIEP): In every theorem of the form X → (Y → Z), X and Z cannot be implicative formulas.
I want to show that NIEP is an interesting relevantist principle that recovers enough axioms to characterize a fairly expressive relevance logic. My plan is to start by explaining the three principles which I will focus on: VSP, EUP and NIEP. I will motivate these principles by showing some results that are valid in Classical Logic but are not relevantly valid. Due to the ambiguity of EUP, I will have to suggest an interpretation with which I will work throughout this paper and I will relate it to NIEP in order to highlight the differences between EUP and NIEP. Afterwards I will focus on pinpointing the logics that exist between B and R to investigate what can be recovered from B taking into account NIEP. The goal is to find a logic characterized by VSP, EUP and NIEP.

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CONSTRUCTING ILLOYAL ALGEBRA-VALUED MODELS OF SET THEORY

Session 19K

Congress section(s): A1

The construction of algebra-valued models of set theory starts from an algebra A and a model V of set theory and forms an A-valued model V(A) of set theory that reflects both the set theory of V and the logic of A. This construction is the natural generalisation of Boolean-valued models, Heyting-valued models, lattice-valued models, and orthomodular-valued models [1, 2, 7, 5] and was developed in [4].
Recently, Passmann introduced the terms "loyalty" and "faithfulness" while studying the precise relationship between the logic of the algebra A and the logical phenomena witnessed in the A-valued model of set theory in [6]. A model is called loyal to its algebra if the propositional logic in the model is the same as the logic of the algebra from which it was constructed and faithful if every element of the algebra is the truth value of a sentence in the model. The model constructed in [4] is both loyal and faithful to PS3 , which is a three-valued algebra and can be found in [4] as well.
In this talk, we shall give elementary constructions to produce illoyal models by stretching and twisting Boolean algebras. After we give the basic definitions, we remind the audience of the construction of algebra-valued models of set theory. We then introduce our main technique: a non-trivial automorphisms of an algebra A excludes values from being truth values of sentences in the A-valued model of set theory. Finally, we apply this technique to produce three classes of models: tail stretches, transposition twists, and maximal twists.
It will be shown that there exist algebras A which are not Boolean algebras and hence its corresponding propositional logic is non-classical, but any sentence of set theory will get either the value 1 (top) or 0 (bottom) of A in the algebra-valued model V(A), where the sub algebra of A having domain {0, 1} is same as the two-valued Boolean algebra. This concludes that the base logic for the corresponding set theory is not classical, whereas the set theoretic sentences act classically in this case. This talk is based on [3].
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FREE LOGIC AND UNIQUE EXISTENCE PROOFS

Session 4M

Congress section(s): A2, C1

Results establishing the unique existence of a thing that satisfies certain properties are essential for the legitimate use of singular terms. A unique existence statement concerning a property F typically consists of two parts: An existence statement E(F) to the effect that there is at least one thing which is F; and a uniqueness statement U(F) that there cannot be two different things which are both F's.
With respect to the classical first-order logic, uniqueness and existence are considered to be logically independent. This result generalizes beyond pure logic: Let T be a theory and F be a property definable in T. Then, if E(F) can be deduced from U(F) relative to T, then it can already be deduced from T, without assuming U(F). That is, even if one provide a proof of existence using uniqueness, this extra assumption is dispensable. Similarly for U(F). One may recourse to these observations to justify using any one of the uniqueness or existence statements while proving the other.
In almost all examples of unique existence proofs in mathematics, neither of the uniqueness or existence statements is used to prove the remaining statement. However, since there are significant exceptions, the logical problems related to unique existence proofs should also be of mathematical interest. For there might be some cases where one cannot legitimately or efficiently use the previously proven uniqueness proposition to prove the existence statement.
Theorems that state unique existence of functions provide good examples for us to see if we should consider restricting the positive application of a uniqueness statement while proving the corresponding existence statement. For a proof of a theorem of this sort, one needs to show that a function satisfying the required property exists and that it is unique (there cannot be more than one such functions). Moreover, the existence part of the proof typically involves, in turn, many unique existence results; to be a function requires for every object in the domain the existence of a unique object to be counted as the value of that function. This is where a question arises: assuming that the uniqueness statement of the function is known, can it be applied while constructing the pairs forming the function? (Or, the graph of the function.) An affirmative answer to this question means approving the use of the uniqueness part of the theorem while proving the existence part, though indirectly. I will argue for the view that, allowing for sets with non-existent elements, Bencivenga's system of free set theory (FST), which he developed in [1] and [2], implies that we should restrict the use of the uniqueness results within an attempt to prove the related existence statements for functions.
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RESIDUALS AND CONJUGATES IN POSITIVE SUBSTRUCTURAL LOGIC

Session 29D

Congress section(s): A2

In substructural, fuzzy, and many other logics, relations of residuation between conjunction-like and conditional-like connectives are central (Ono et. al. 2007). For instance, in the context of frame semantics, relations of residuation between connectives allows for those connectives to be interpreted by a single relation, as has been studied in the context of Gaggle Theory (Bimbó & Dunn 2008).
In Boolean algebras with operators (BAOs), residuation has a second face in the form of relations of conjugation (Jónsson & Tsinakis 1993; Mikulás 1996) – the residuals of an operator are definable in terms of its conjugates, and vice versa, by means of Boolean negation. An immediate result of this is that in BAOs, a collection of operations all conjugated with respect to each other may be interpreted by a single relation in the frame semantics.
This talk concerns relations of residuation and conjugation in a positive context – in particular, in the context of logics extending the positive non-associative Lambek calculus with distributive lattice operations. This logic is the extension of distributive lattice logic by means of a binary operator – sometimes called fusion – with left and right residuals, where fusion is assumed only to be monotone (or, equivalently when the residuals are present, to distribute over (and into) the lattice join). The extension of this logic with which we're concerned is that resulting from the addition of two additional fusion-like connectives, where each fusion is conjugated with respect to the others, and left and right residuals for each additional fusion.
Our concern with this logic is motivated by the ternary relation frame semantics for the Lambek calculus – since the residuals and conjugates of an operator can be interpreted by means of one accessibility relation in BAOs, it is an interesting question whether the same is true in a positive context. Of particular interest here is that adding conjugates, and their residuals, to the language would allow for more expressive power in characterising ternary relation frames – in the language including the conjugates, simple frame correspondents can be found for some classes of frames which have otherwise only been characterised by means of negation. Furthemore, there are interesting connections to the semantics for other substructural and relevant logics.
This talk presents the logic in question as characterised by a class of ternary relation models – then we go on to consider the question of completeness.
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PROCESSES AND MECHANISMS

Session 4H

Congress section(s): B4

I became puzzled by the dearth of mention of processes in the New Mechanists literature. This paper examines what I have found.

The new mechanists tend to characterize mechanisms in analogy to the idea of a machine. But the characterizations they offer of the "machine" idea omit many things to which the term "mechanism" gets applied, such as mechanisms of crystal formation, catalytic action, soil erosion, gene transcription, chemical mechanisms.... One sees mention of such examples in the literature, but they are not mechanisms in the stable "machine" sense and so are not covered by the official characterizations. Such "variably embodied mechanisms" are of explanatory interest just as much as the stably embodied, machine-like ones.
Dictionary definitions of "mechanism" include both the machine-idea and processes more generally that produce what I will call a "phenomenon of interest". A mechanism that produces soil erosion can operate in may places but generally not within the same physical housing as in the case of a clock producing an indication of the time.
To get a more general account I start by characterizing a process as a collection of events that are systematically linked.
We tend to apply the term "mechanism" when we are considering a process type that issues in some phenomenon of interest. As a process/mechanism type, such can operate repeatedly, but not necessarily within some one physical housing.
The narrower notion studied by the New Mechanists uses "mechanism" as a term for a stable physical housing that can repeatedly exhibit the process producing the phenomenon of interest.
Finally, I will start work on a typology of processes/mechanisms. Here is some illustration:
Begin with linear processes, in the A causes B causes C... pattern, with dominos as the stereotypical example. These will contrast with non-linear processes that will be of many kinds: causal loops, homeostatic processes/mechanisms... The simple pendulum clock, that we refer to as a mechanism, provides a simple illustration of how the linear and non-linear can combine in a larger whole. A clock can be thought of as linearly organized. All but the first nodes of the linear chain are a simple series of 60:1 gears. But the first node in this chain is itself a non-linear complex involving interaction between the pendulum-escapement component and the beginning of the drive train. As in this example, linear processes can themselves be complex with individual nodes comprising complex non-linear interactions. Broadly, any of these might repeatedly occur within one stable physical housing, or their physical embodiments can be one offs. Either way, if the process type in question produces a phenomenon of interest, the term "mechanism" applies.
Much of the paper will work to clarify and more carefully apply terms that this abstract has so quickly introduced.
There are similarities and differences between my account and some parts of Stuart Glennan's recent book. A full write-up will also discuss related material from Nicholson, Kuorikoski, Ramsey and doubtless many others."

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DOES THE REALITY OF THE WAVE FUNCTION FOLLOW FROM THE POSSIBILITY OF ITS MANIPULATION?

Session 30K

Congress section(s): C2

There are various approaches to the issue of the reality of such unobservable object as the wave function. I examine whether the issue can be resolved by taking the manipulative criterion proposed by J. Hacking in the framework of his experimental realism (Hacking, 1983, 262). According to Hacking, if we can influence some observable objects by manipulating an unobservable object, then the latter is the cause, which means it is real. Accordingly, if we can influence some existing objects by manipulating the wave function, then the wave function is a real entity. I examine the strengths and weaknesses of the manipulative criterion concerning the wave function.
I consider one of the experiments with 'quantum eraser' and causally disconnected delayed choice. Experimenters 'label' the wave function of a system photon by 'which-way information' with the help of auxiliary entangled photon. Afterward the measurement of the system photon (no interference) this information is erased that appears to be free or random. Thanks to such manipulations, they restore the wave function of the system photon and observe the interference in the measurement again.
It is known that almost all quantum technologies within the second quantum revolution based on the manipulating of the wave function of either single quantum object or entangled ones. For instance, in a quantum computer, by manipulating entangled qubits, you can force them to perform computations. If the wave functions of qubits do not exist, where does a result of the calculation come from? Another example is quantum cryptography.

It would seem that these cases confirm the existence of the wave functions. However, I argue that Hacking's criterion is not a sufficient argument in favor of the reality of the wave functions. First, any laboratory manipulations suggest theoretical loading of unobservable objects. It is possible that in some years the modern quantum theory is found to be a limiting case of some new theory. Then the wave function would be a manifestation of some other fundamental theoretical object. Second, Hacking's experimental realism is based on a causal relationship between events. However, at the quantum level, causality is something unusual. The uncertainty principle, quantum non-locality, measurement problem – all of these lead to a new notion of causality. Sometimes we cannot accurately identify which of the two correlating quantum events is the cause and which is the effect. It means that a concept of causality also depends on theory.
I suppose that manipulation the wave function can only confirm that it represents either a certain real fundamental entity or some real internal structure of the quantum system. It can look like a picture described by ontic or constructive versions of structural realism for the quantum field theory (Cao, 2003; French & Ladyman, 2003).
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META-INDUCTIVE PREDICTION BASED ON ATTRACTIVITY WEIGHTING: MATHEMATICAL AND EMPIRICAL PERFORMANCE EVALUATION

Session 19M

Congress section(s): B2

A meta-level prediction method (as opposed to an object-level prediction method) is one that bases its predictions on the predictions of other prediction methods. We call a (meta-level) prediction method "access-optimal" in a given environment just in case its long-run predictive success rate is at least as great as the success rate of the most successful method (or cue) to which it has access it that environment (where access consists in knowledge of the present predictions and the past predictive accuracy of a respective method). We call a prediction method "universally access-optimal" just in case it is access-optimal in all possible environments. Universal access-optimality is obviously a very desirable feature. However, universal access-optimality is also rare, and we show: (1) There are no 'one-reason' prediction methods (i.e., methods that base each prediction on the prediction a single object-level method or cue) that are universally access-optimal, and (2) none of a wide range of well known weighting methods is universally access-optimal, including success weighting, linear regression, logistic regression, and typical Bayesian methods.
As shown in previous work, there is a prediction method known as Attractivity Weighting (AW) that is universally access-optimal, assuming accuracy is measured using a convex loss function (Cesa-Bianchi & Lugosi, 2006; Schurz, 2008; Schurz & Thorn, 2016). Although AW is universally access-optimal, there are other meta-level prediction methods that are capable of outperforming AW in some environments. In order to address this limitation of AW, we introduce two refined variants of AW, which differ from AW in having access to other meta-level methods. We present results showing that these refined variants of AW are universally-access optimal. Despite guarantees regarding long-run performance, the short-run performance of the variants of AW is a theoretically open question. To address this question, we present the results of two simulation studies that evaluate the performance of various prediction methods in making predictions about objects and events drawn from real world data sets. The first study involves predicting the results of actual sports matches. The second study uses twenty data sets that were compiled by Czerlinski, Gigerenzer, and Goldstein (1999), and involves, for example, the prediction of city population, the attractiveness of persons, and atmospheric conditions. In both simulation studies, the performance of the refined variants of AW closely matches the performance of whatever meta-level method is the best performer at given time, from the short run to the long run.
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SMART SYSTEMS: THE POWER OF TECHNOLOGY

Session 5E

Congress section(s): C8

In 2008 the vision of a Smart City was described as follows: "the goal of such a city is to optimally regulate and control resources by means of autonomous IT systems" (Siemens 2008, p.35). Ten years later concrete implementations of subsystems of such a city already exist as Smart Mobility for the optimal regulation of traffic, Smart Energy for efficient energy management or Smart Health for ambient assisted living. The prerequisite of any smart system is a sensor rich and datafied environment. The data gained are used by predictive algorithms to predict future behavioural patterns and optimize the resources accordingly. The focus is on providing knowledge under conditions of uncertainty in order to know ahead and act before trying to streamline processes towards enhanced efficiency. In the next development stage, the transition from prediction to prescription takes place: future behaviour is not only anticipated but formed. Context-specific, adaptive micro-directives (Casey/Niblett 2015) may be incorporated in future intelligent infrastructures to guarantee optimal service from a systems' perspective and nudge or even coerce the human participants towards the desired behaviour. Thus such prescriptive smart systems manifest power relations demonstrating the power of technology in a Foucauldian way: "power is employed and exercised through a netlike organization" (Foucault 1980). However, in these smart systems humans may not only behave as intended but also act in a subversive way demonstrating that "individuals are the vehicles of power, not its points of application" (ibidem, p.98). Thus, even if these environments restrict human autonomy, they also open up possibilities for undermining such systems. They are "dispositifs" in the Foucauldian sense possessing the dual structure of manifestation of power and the chance of subverting it.
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KNOWLEDGE PRODUCTION IN SOCIAL NETWORKS AS THE PROBLEM OF COMMUNICATIVE EPISTEMOLOGY

Session 11H

Congress section(s): B1, B3, B6, C7

Introduction. The communicative dimension of epistemological discourse is connected with the research of how communication forms influence the production of knowledge. The modern communication revolution is determined by a new social role of Internet technologies, which mediate the social communication of different level and open mass access to any kinds of communication. Development of Internet services of social networks gives users more and more perfect instruments of communication management. These tools give individuals the possibility to develop their own networks

of any configuration despite the minimum information about partners and to distribute knowledge out of the traditional institutional schemes of the Modern. Distribution of social networks has cognitive effect because it ensures the mass users inclusion in the production of informal knowledge. The author believes that Internet content is a specific form of ordinary knowledge, including special discursive rules of production of knowledge, as well as the system of its verification and legitimation.

Methods. The research media influence on cognitive structures of communication is based on M. McLuhan's ideas; the analysis of network modes of production of knowledge is based on M. Granovetter and M. Castells's network approach; the cognitive status of Internet content is proved by means of the concept of ordinary knowledge of M.L. Bianca and P. Piccari. The author's arguments are based on the communication approach which brings closer the categories of social action, the communicative act and the act of cognition.

Discussion. Ordinary knowledge in epistemology is quite a marginal problem. A rather small amount of research is devoted to its development. One of the key works in this sphere is the collective monograph "Epistemology of Ordinary Knowledge", edited by M.L. Bianca and P. Piccari (2015). In this work M.L. Bianca proves the concept according to which ordinary knowledge is a form of knowledge which not only allows to get epistemic access to the world, but also includes development of the models of the world which possess different degree of reliability. The feature of this form is that ordinary knowledge can be reliable and relevant though it has no reliability of scientific knowledge.

The question of how the media sphere changes the formation of ordinary knowledge, remains poorly studied. In the beginning the technical principles of operating content determine the epistemic processes connected with complication of the structure of the message. The environment of ordinary knowledge formation is the thinking and the oral speech. Usage of the text causes splitting of initial syncretism of ordinary knowledge and increasing the degree of its reflexivity and its subordination to genre norms (literary, documentary, journalistic), i.e. initial formalization. Usage of basic elements of a media text (graphic, audio- and visual inserts) strengthens genre eclecticism and expands possibilities of the user self-expression, subjectcentricity and subjectivity of the message.

The dominance of subjective elements in advancement of media content is fixed by the neologism "post-truth". The author defines post-truth as the independent concept of media discourse possessing negative connotations and emphasizing influence of interpretations in comparison to factography. The communicative entity of post-truth comes down to the effect of belief as the personal and emotional relation to the subject of the message. The post-truth combines global with private, personalizes macro-events and facilitates the formation of their assessment for the recipient.

The post-truth as transmission of subjectivity is based on representation of personal subjective experience of world cognition, i.e. its core is ordinary knowledge, on the platform of which personal history, personal experience and personal truth are formed, replacing objective data.

The post-truth does not mean direct oblivion and depreciation of the truth. The emotionally charged attitude acts as the filter for the streams of diverse content in conditions of the information overload. Through the post-truth people also cognize, and, at the same time, express themselves, create identities and enter collective actions.

Conclusion. Communicative epistemology as the methodological project offers new prospects in the research of social networks production of knowledge. According to the author, social networks as the special channel transform ordinary knowledge to informal one.

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HISTORY AND EPISTEMOLOGY OF CLIMATE MODEL INTERCOMPARISON PROJECTS

Session 7B

Congress section(s): C2

Having started almost 30 years ago with the Atmospheric Model Intercomparison Project, climate model intercomparison projects are now at the core of climate research. In particular, the Coupled Model Intercomparison Project (CMIP) has become "one of the foundational elements of climate science" (Eyring 2016). Since its creation in the mid-1990s, it has evolved over five phases, involving all major climate modeling groups in the world. In addition with their role for climate research, these phases have hold a central place in international assessments of climate change - in IPCC reports in particular - providing therefore guidance to decision makers.

With more and more people concerned by the results of its experiments, CMIP has been put at the center of contradictory interests (Taylor 2012). In particular, historically, CMIP has had to combine a role for climate research - to help scientists understand climate - and for society - to assess the state of human knowledge about climate and climate change. Yet, the ability of CMIP to play these two roles led to some debates: both the heuristic value of intercomparison projects and the interpretation of assessments from multi-model ensembles for policy guidance have been questioned by climate scientists and philosophers of science (e.g. Knutti 2010, Lenhard and Winsberg 2010, Parker 2011, Winsberg 2018). Taking into account these debates, we will try to show how intercomparison projects can still be useful both for climate research and for policy guidance.

It is often considered that intercomparison projects help climate modeling research, because (1) the comparison of simulation outputs helps to diagnose the causes of differences between models. (2) the agreement between models makes it possible to confirm robust results. However, these two views have been challenged: (1) has been challenged by Lenhard and Winsberg (2010) because, according to them, complex numerical climate models are facing a strong form of confirmation holism. This holism limits significantly the analytical understanding of climate models, thus makes difficult the interpretation of differences between simulations in climate model intercomparison. (2) has been challenged by Parker (2011) and Winsberg (2018), who have shown that in climate multi-model ensembles, the fact that robustness increases confidence is arguable.

In front of that, we will claim that intercomparison projects nevertheless help climate modeling research mainly because (3) they act as an infrastructure, which helps organize and coordinate climate modeling activity, therefore making climate modeling research more efficient.

In a second time, we will clarify the role of intercomparison projects for decision makers, by proposing a new interpretation of climate multi-model ensembles when they are used for policy guidance. For that, we will first give criteria to determine how a single simulation can be best useful for decision makers. It will lead us to define a numerical model as a personal tool, expressing the subjective but rational voice of the climate modeler(s). With this definition, we will then interpret intercomparison protocols as scientific polls, which serve mainly a synthesis role.

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FROM SUBJECT NATURAL LOGIC TO SCIENTIST LOGIC IN NATURAL SCIENCE: AN EPISTEMOLOGICAL REFLEXION

Session 8K

Congress section(s): B3

Traditionally, epistemology been conceptualized as branch of philosophy as philosopher reflexion about scientific work for the construction of philosophical systems where subject material and symbolic actions are totally different to formal actions. Nevertheless, Piaget dedicated his life to analyze the subject natural logic since psychogenetic perspective. He argue

that subject natural logic and logic of scientist in natural sciences is a matter of fact, while subject whom research about logic science, formalization is a matter of deduction with stablish rules. Then, on the one hand, the subject natural logic and logic of the scientist in natural sciences implies norms that address their actions according to the facts of natural world configuring structural representations coordinating the outcomes of their actions on the natural world constructing is own rules. On the other hand, logics rules are deduced independently from the subject actions on natural world. However, the natural scientists are also subjects that structures their reality based on their own psychogenetic natural logic before they becoming scientists.
If we consider, as Piaget does, that "essences" or intuitions are inseparable from the facts, then it can be argued that there is a close link between form and fact. On the other hand, at this research it is argued that imagination is a rational explanatory resource that mediates between intuitions and concepts. Consequently, the symbolic component plays an important role as a mediator between the fact and its formalization that culminates with the configuration of action rules.
In that manner, the problem is based: how the link of factual issues is established to deductive issues of the subject's norms. Therefore, the objective of this paper is to analyze the normative and symbolic logical components of the fact-deduction and its implications in the experimental-formalization link through psychogenetic method.
To achieve the objective, the present work is based on the exposition of the differences between meta-scientific, para-scientific and scientific epistemologies, which will establish the theoretical concepts of the epistemology of the imagination to describe how the subject-object relationship is understood. Subsequently the discussion on the logic of the subject is opened from a genetic approach to finally propose an epistemological framework on the link between the experimental and deductive components in the construction of scientific knowledge.

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HARMONY, STABILITY, AND THE INTENSIONAL ACCOUNT OF PROOF-THEORETIC SEMANTICS

Session 28A

Congress section(s): A2, A3, C6, C8

Proof-theoretic semantics (PTS) is based on the idea that the meaning of a proposition A should not be explained in terms of the conditions at which A is true, but rather in terms of the conditions at which the truth of A can be recognized. For mathematical propositions, this amounts to equate knowledge of the meaning of A with knowledge of what is to count as a proof of A, i.e. of its proof-conditions.
The explanation of the proof-conditions comes together with criteria of identity of proof, that is of criteria for telling, given two proofs of a proposition, whether they are the same proof or not. The talk focuses on the problem of whether, for certain classes of propositions, such criteria deliver a trivial notion of identity of proof. By this we mean that for a proposition belonging to these classes, there can be at most one proof of it, or, equivalently, that any two proofs of such a proposition are identical.
If identity of proof is not trivial, PTS delivers an intensional account of meaning, that is, it can give substance to the idea that there may be essentially different ways in which a proposition can be true, corresponding to the different proofs of the proposition. On the contrary, if identity of proof is trivial, the set of proofs of a proposition A - what in PTS can be seen as the semantic value of A - is either the empty set, or the singleton containing the (only) proof of A. In this case, PTS would come very close to an extensional semantics in which the semantic value of a proposition is simply identified with its truth-value.
Central to PTS is thus the understanding of proofs as abstract objects, as opposed to the syntactic representations of proofs in formal calculi. Formal derivations should be seen as merely "representing", or "denoting" proofs in the abstract sense. Given an appropriate equivalence relation on formal derivations, one may say that equivalent derivations denote the same proof, and proofs could really be thought of as the abstracta obtained by quotienting derivations modulo equivalence.
One of the most promising accounts of the two central concepts of PTS, called "harmony" and "stability", has been given in terms of certain transformations on derivations in the natural deduction format: reductions and expansions. Reductions and expansions determine an equivalence relation on derivations, and in turn a notion identity of proofs, which, as we show, is trivial for two significant classes of proposition: identity statements and negated propositions. In order to recover an intensional account of the meaning of these propositions, we consider the possibility of weakening either the notion of harmony, or that of stability, or both.
We conclude by discussing the extent to which the proposed framework is coherent with the interpretation of proofs as programs stemming from the Curry-Howard correspondence.

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LOGIC AS METAPHILOSOPHY? REMARKS ON THE MUTUAL RELATIONS OF LOGIC AND PHILOSOPHY

Session 24J

Congress section(s): A3

The project of logical philosophy was announced by Jerzy Perzanowski as an alternative to various metaphilosophical projects.
1. Philosophy or its history?
It is not known exactly what philosophy is about. The self-identification of a philosopher is more problematic than an artist or a scientist. We should distinguish a philosopher from a historian of philosophy. The former sees problems and tries to solve them. Why should he look for inspiration in the past and not in the surrounding reality?
The reference to old masters is common among philosophers. It is not known what motives philosophers cause, nor is it known to what extent knowledge, say Aristotle's concept of the soul is necessary to understand and solve the contemporary mind-body problem? Treating the history of philosophy as "eternal now" means that every reference to a noble name, showing, and perhaps the nth order, "relationships" between classic A and classic B is treated on the basis of philosophy with equal seriousness as an attempt to solve any problem called philosophical. For example, Pythagorean statements about the mysticism of numbers remain a fascinating curiosity, but in no sense are they part of contemporary mathematics, nor do any of the mathematicians have the impression that this state of affairs in any sense impoverishes his discipline. There are no fundamental contraindications for similar treatment of the past of philosophy.
2. Logic towards philosophy.
I will satisfy myself with the conviction that logic is a systematized set of reliable inference schemes.
It is absolutely not problematic to indicate mutual, possible relations between logic and philosophy. Their mutual relations are, so to speak, multifaceted and multi-level at the same time. I choose the hypothetical separateness of the ranges of logic and philosophy - considering a symmetrical relationship with a different distribution of accents:
(1) The relationship between logic and philosophy - with the accent on logic - has three aspects:
(a) the logic of philosophy - aspect of the relation of logic to other philosophical sciences;
(b) logic in philosophy - aspect of the role of logic in philosophy (criticism of philosophical reasoning);
(c) The third aspect of the relationship between logic and philosophy is the logic of philosophy, which treats the philosophy itself - about the way of organizing and connecting its components and factors.
(2) The relation of philosophy and logic - with an emphasis on philosophy - is analogical and also has three aspects.
(a) The philosophy of logic describes, among others, changes in particular philosophical disciplines in contact with logic;
(b) Philosophy in logic contains criticism of logic, treating, among others, about the philosophical entanglements of logical theories;
(c) The philosophy of logic is associated with philosophy in logic, focusing on determining the status, object, and source of logic.
A purely formal representation of the relationship cannot be controversial due to the fact that all possibilities are considered.
Depending on the attitude to the role of logic in philosophy, I will distinguish prelogical, alogical and illogical philosophy.
The prelogical philosophy is those trends in the history of the discipline that explicitly assume in argumentations or refer to decisions known from logic.
Alogical philosophy is one whose theses are explicitly articulated in isolation from logic, but their interpretation in its language does not enforce complex and problematic hermeneutic procedures. Finally, an illogical philosophy, it is one that programmatically shuns the tools of logic and does not succumb to intersubjective interpretive procedures.

Philosophy would be barren if it could determine the universe of all possibilities, indicate the source and method of generating this space, diversify the objects in it, link them and outline the system that unites them into a mechanism. Such a philosophical task makes it a discipline not only related but even subordinate to logic. At the same time, it has the advantage that the realization of such a task in spite of technical difficulties, does not make this task hopeless.
The result of such a plan of research is to define the conditions that logical philosophy should fulfill - that is, a closed-ended philosophy that is deductively and organized in theories in a logical sense. Treating logic as a metaphilosophy is possible under the condition of deductive ordering (that is, organizing philosophy with logic in theories). The product of logic is not only conceptual analysis (though their role cannot be overestimated) but mainly prepared by them (formal) philosophical theories - that is, expressed in appropriate language and related relations, the chains of assertions.

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BERNARD BOLZANO AND THE PART-WHOLE PRINCIPLE FOR INFINITE COLLECTIONS

Session 18B

Congress section(s): B6

The embracing of actual infinity in mathematics leads naturally to the question of comparing the sizes of infinite collections. The basic dilemma is that Cantor's Principle (CP), according to which two sets have the same size if there is a one-to-one correspondence between their elements, and the Part-Whole Principle (PW), according to which the whole is greater than its part, are inconsistent for infinite collections [2].
Contemporary axiomatic set-theoretic systems, for instance, ZFC, are based on CP. PW is not valid for infinite sets. Bernard Bolzano's approach primarily described in his Paradoxes of the Infinite from 1848 [4] relies on PW.
Bolzano's theory of infinite quantities is based on infinite series of numbers. PW leads to a special way of their treatment. They can be added, multiplied and sometimes we can determine their relationship. If we interpret infinite series as sequences of partial sums and factorize them by the Fréchet filter, then all properties determined by Bolzano will hold [6]. We obtain thus a partially ordered commutative non-Archimedean ring of finite, infinitely small and infinitely great quantities, where we can introduce the so-called „cheap non-standard analysis" [5].
The size of collections with regards to the multitude of their elements is another topic. Bolzano rejects CP as a sufficient criterion for equality of infinite multitudes. Further conditions are necessary; Bolzano refers to the need for having the same „determining ground". As to natural numbers Bolzano does not determine explicitly the relationship between their multitude and the multitudes of their subsets. Nevertheless, it is evident how to express these multitudes with help of Bolzano's infinite quantities. This is related to the Bolzano's special notion of a sum [3]. Similarly, infinite series express consistently multitudes of a union, of an intersection, and of a Cartesian product of natural numbers and their subsets. This extended conception of Bolzano is similar in its results to the theory of numerosities [1].
In Paradoxes of the Infinite Bolzano also investigates relationships among multitudes of points of segments, lines, planes and spaces. In this case, the same "determining ground" means for Bolzano the existence of a one-to-one correspondence which is simultaneously an isometry. This part of Bolzano's work could be also interpreted in the theory of numerosities.
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CLASSIFYING FIRST-ORDER MEREOLOGICAL STRUCTURES

Session 3L

Congress section(s): A2

Mereology is the theory (or a class of theories) based on the relation "being a part of". The following are some first-order mereological axioms which can be found in the literature. Three most basic axioms are reflexivity, anti-symmetry and transitivity, that is to say, any mereological structure must be a partial ordering. Strong supplementation says that if x is not a part of y, then x has a part z which does not overlap y, where overlapping means sharing at least one part. Unrestricted fusion, which is an axiom schema, says that for any formula which defines (perhaps with some parameters) a nonempty subset of the domain, there is a member which is the least upper bound of that subset. The theory generated by the said axioms is called General Extensional Mereology (GEM). Intuitively, mereological structures can be classified into three mutually disjoint subclasses: atomic, atomless and mixed. The mixed can be further classified into infinitely many mutually disjoint subclasses according to the numbers of atoms, since any mixed model must either have exactly k atoms, for some k>0, or have infinitely many atoms. For any first-order axiomatizable mereological theory, each of the said subclasses of its models is axiomatizable (note that "atom", "atomic", "atomless" and any finite cardinality are first-order expressible and that infinity can be expressed by a list of infinitely many first-order axioms each of which says that there are at least k members, where k>1). Previously, it has been shown that for GEM, each of the said subclasses of its models is axiomatizable by a complete theory [1]. We can make use of this result to show the decidability of any first-order axiomatizable mereological theory which is at least as strong as GEM. The models of a weaker theory can also be classified in the same way. For instance, consider the following two axioms: (a) finite fusion: any definable (perhaps with some parameters) nonempty finite subset has a least upper bound and (b) complementation: for any member such that some member is not its part, there is a member disjoint from that member such that their least upper bound is the greatest member. It can be shown that we can get a weaker theory by substituting (a) and (b) for unrestricted fusion. Then for such a weakened theory, the subclass of mixed models each of whose members has infinitely many atoms will have two models which are not elementarily equivalent. However, such a subclass can be again divided into two disjoint subclasses either of which is axiomatizable by a complete theory, and from this, we can again show the decidability of the weakened theory. It is interesting to apply the same procedure to an even weaker theory and see what subclass contains models which are not elementarily equivalent and whether there is a definable property to separate those models.
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ORIGINS

Session 11A

Congress section(s): C7

The inference of origins distinguishes the historical sciences from the theoretical sciences. Scientific inferences of origins are distinct in inferring reliably probable pasts. They base their inferences of origins on information transmitted from origins, past events and processes, to present receivers, evidence. They include most obviously the origins of species in Evolutionary Biology, origins of languages in Comparative Historical Linguistics, origins of rock formations and the shapes of continents in Geology, the origins of the universe in Cosmology, the origins of texts in Textual Criticism, original historical events scientific Historiography, and the origins of forms of art and craft like pottery in Archaeology.
This paper analyses the concept of origin, its metaphysics and epistemology as distinct of those of causes. I argue that origins are tokens of types of information sources. Origins are past events that transmitted information that reached the present. Entities in the present that receive that information are receivers. Information preserved in receivers may be used to infer properties of their origins. Origin is a relational concept. As much as a cause can only be identified in relation to its effects and there are no causes without effects, origin can only be identified in relation to receivers and there are no origins without receivers. Origins transmit encoded information signals to receivers. There are many different types of

information signals, transmission channels, and types of encoding: Background radiation travelled from the origin of the universe to scientific instruments today. Species transmit information about their properties and ancestry via DNA through reproduction to descendant species. During transmission, information passes through a period of latency when it is not expressed. Latency can vary in length from the age of the universe in the case of background radiation to the brief moment between sending and receiving an email. Information signals are mixed with varying levels of noise and have different levels of equivocation, loss of signal. Types and tokens of processes of encoding and decoding have varying levels of reliability-fidelity, information preservation at the end of the process of information transmitted from the origins at the beginning. Reliability reflects the ratio of preservation of information in receivers to the transmitted information. Some information is lost during transmission (equivocation) and noise that does not carry information is mixed with the signal. For example, we are all the descendants of "prehistoric" peoples. But the information they transmitted about themselves orally through traditions to contemporary societies was lost in few generations due to equivocation. What we can know about them is through information preserved in material and artistic objects and our DNA. Societies cannot transmit information reliably over centuries without a written form of language that can preserve information reliably. I clarify what origins are and how we come to know them by analyzing the conceptual and epistemic distinctions between origins and causes. This analysis justifies the introduction of origins as a new concept to epistemology and philosophy of science to supplement and partly replace philosophical discussions of causation.

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SCIENTIST AS AN EXPERT: BREAKING THE IVORY TOWER

Session 28B

Congress section(s): B1, B5

Weber considered participation in the "battle of gods" as an unacceptable risk for science. This risk occurs when a scientist (or a lecturer) plays the role of arbitrator in the value debates. Weber argued that science turned into ideology when one uses the authority of science ex cathedra to persuade the audience to follow his ideas. According to Weber, science has nothing to deal with politics since the scientific search doesn't require political legitimization. It is governed by the principle that any knowledge is worthy "for its own sake". This idea is closely connected with the key Weber's principle of "the disenchantment of the world". Weber considered a deep specialization and functional autonomy of social institutes as the key features of the rationalization process. For Weber, the value incompatibility of politics and science is the result of this process as well. That is why Weber insisted that a scientist shouldn't assist politicians in decision making unless he or she wants to be called a "prophet". Nevertheless, the present situation in social science shows that the collaboration of science and politics in a form of expertise is in great demand in the "risk society". Political decisions require the deep expert analysis, which becomes one of the crucial aspects of their legitimacy. Hence, the expert knowledge makes the science-politics borders blurred again. This process has become the matter for epistemological discussions. Here the relevant question is whether the expert knowledge makes scientific research dependent on political agenda (i.e., destroys the autonomy of science). Meanwhile, I argue that a correct understanding of the expert knowledge allows considering expertise as a purely scientific phenomenon (in Weber's sense). It requires a differentiation of scientific and political contexts of expertise, which helps reduce the moral pressure on an expert's work. The differentiation could be analyzed by means of the concepts of "operative closure" and "cognitive openness" provided by Niklas Luhmann. We can advocate the idea of autonomy of science (even in the age of the expert knowledge) if we consider "operative" (methodological) closure and "cognitive openness" (an ability of science as a system to respond to some challenges coming from the other systems) as two interrelated features. I intend to show how Luhmann's analysis of science can help clarify the epistemological status of expertise as a kind of scientific knowledge.

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COMPUTATIONAL ABSTRACTION

Session 10E

Congress section(s): C6

The practice of Computer Science is dominated by various processes or devices of abstraction. Many these devices are built into specification and programming languages. Indeed, they are the mechanisms of language design, and the process of abstraction maybe seen as generating new languages from given ones. Our objective in this paper is to provide a logical analysis of such abstraction.

Contemporary research in mathematical abstraction has been inspired by Frege(1884). However, this work, that generally goes by the name the way of abstraction, has mostly been aimed at classical logic and mathematics where the ultimate goal has been to abstract the axioms of Zermelo-Fraenkel set theory. Little work has been aimed at other foundational frameworks such as type theory, the central carrier of computational abstraction. Our intention is to explore how the way of abstraction may provide a foundational framework for the latter.

Frege observes that many of the singular terms that appear to refer to abstract entities are formed by means of functional expressions. While many singular terms formed by means of such expressions denote ordinary concrete objects, the functional

terms that pick out abstract entities are distinctive in the sense that they are picked out by functional expression of the following forms.

(1) The direction of a line A =The direction of line B iff A||B.

where A||B asserts that A is parallel to B. Inspired by examples such as this, an abstraction principle may be formulated as a universally quantified bi-conditional of the following form:

8x:8y:f (x) = f(y) $ R(x; y)

where x and y are variables of some logical language, f is a term forming operator and R is an equivalence relation.

In general, the abstractionist views such abstraction principles as introducing a new kind of object. For example, (1) introduces the kind of things that are directions. But notice that these abstractions also require a source domain on which the abstraction acts. For example, in the case of directions the domain of lines is presupposed. Such an approach to the development of type theories views abstraction as a way of creating new languages and type theories from existing ones. However, the way of abstraction does not just postulate a given class of types and type constructors. Abstraction offers a dynamic for the introduction of new types and type constructors that allows us to explore the rich and varied notions of type that are to be found in modern computer science. The only restriction is that these new types emerge via the way of abstraction. So that the way of abstraction now takes the following shape.

8x : T:8y : T:(f(x) =T=R f(y)) $ R(x; y)

where T is an existing type, R is a relation determined by some well-formed formulae of the formal language, T=R is the new type formed by elements of the form f(t) where t : T, and =T=R the equality for the new type. This is exactly the set-up required to describe computational abstraction.

The distinctive feature of successful abstraction is that every question about the abstracts is interpreted as a question about the entities on which we abstracted. This yields a conservative extension result for the new theory that provides an implementation of the new theory in the old one.

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MYSTERIANISM AND THE DIVISION OF COGNITIVE LABOUR

Session 7J

Congress section(s): B1, C3, C5

Mysterianism has become widespread in the philosophy of language, mind and science since the 1970s. Noam Chomsky (1976) used the term "impenetrable mysteries", Jerry Fodor (1983) spoke about "epistemic boundedness", Colin McGinn (1989) introduced "cognitive closure". Mysterianism is an epistemic stance claiming that some of the problems science

deals with are beyond human cognitive capacities and therefore will never be solved. In my paper I will try to show that mysterianism is not a form of scepticism, but a display of epistemic defeatism. There are three main problems with mysterian arguments. Firstly, mysterians' argumentative strategies are defective, because they are based on appeal to ignorance. Secondly, there might be boundaries for human knowledge, but they cannot be subject to some kind of philosophical futurology – they will be exposed by normal scientific practice. Thirdly and most importantly, mysterianism is inconsistent with the division of cognitive labour and ignores the social aspect of empirical science. The emergence of language enabled cooperation in the area of knowledge, therefore most of contemporary scientific theories cannot be grasped by a single mind. If we distinguish between individual and collective rationality, the mysterians' concerns about the fundamental explanatory impossibility of certain phenomena will prove to be unfounded.

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IS THE LIAR SENTENCE MEANINGLESS?

Session 9M

Congress section(s): A2

In its simplest version, the Liar paradox involves a sentence which says of itself that it is false. If the sentence is true, then it is false (for that is what it claims), but if it is false, then it is true. So we have a paradox. A common initial reaction to sentences like 'This is false' or 'L is false' (where 'this' and 'L' refer respectively to the sentences where they appear) is to claim that they are meaningless. If that claim could be justified, we would have a simple and elegant solution to the paradox. Unfortunately, there are powerful reasons for thinking that both sentences are meaningful: 1) the fact that we reach a paradox by reasoning about those sentences would be unexplainable unless they had some sort of meaning; 2) there are many contexts in which 'this is false' is not paradoxical (when 'this' is used to refer to another sentence). This is what happens with contingent Liar sentences. 'Everything I've said today is false' is a perfectly meaningful sentence. In most contexts, someone will say something true or false by uttering it. Yet that will not be the case if the person who utters it has uttered no other sentence that day (or has only uttered false sentences). For these reasons, philosophers who claim that Liar sentences are meaningless tend to make a distinction between what we could call "linguistic meaning" (a set of rules telling us how to use a sentence in order to say something true or false) and "content" (the proposition expressed or the statement made in a context in uttering a sentence given its linguistic meaning). Those philosophers claim that Liar sentences are meaningful in the first sense, but not in the second (in paradoxical contexts). Someone uttering a Liar sentence in a paradoxical context fails to express some content.

A common way to justify why Liar sentences lack content in certain contexts appeals to the idea of "ungrounded sentence" (Kripke 1975) or to the idea that, in paradoxical contexts, the truth value of a Liar sentence depends (circularly) on a sentence from which we cannot eliminate that predicate. This idea is present in theories which treat the truth-predicate as a property-ascribing predicate (Beall 2001, Goldstein 2001) and also in theories which treat the truth-predicate as a device for forming prosentences (Grover 1977). The object of this paper is to show that the strategy followed in all these cases declares contentless sentences which are meant to be true in all those theories.

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ARE TRANSPARENCY AND REPRESENTATIVENESS OF VALUES HAMPERING SCIENTIFIC PLURALISM?

Session 10G

Congress section(s): B5, C7

It is increasingly accepted among philosophers of science that values, including so-called nonepistemic values, influence the scientific process. Values may, i.a., play a role in data collection (Zahle 2018), measurement procedures (Reiss 2013), evaluating evidence (Douglas 2009), and in choosing among scientific models (Potochnik 2012). The next question is: under what conditions is the influence of these values justifiable? Kevin Elliott (2017) highlights three conditions, namely value influences should be (1) made transparent, (2) representative of our major social and ethical priorities, and (3) scrutinized through engagement between different stakeholders.

In this paper, I scrutinize Elliott's conditions (1) and (2). The first condition, transparency, is present in many accounts considering how to deal with values in science. Requiring transparency brings a range of benefits, but also has its drawbacks, in particular in relation to fostering scientific pluralism, as I will argue, analysing what the aggregate effects of a transparency demand might be. Elliott's second condition, representativeness, might help us in answering which and/or whose values scientists should (justifiably) use. This condition can be considered exemplary of the view that values used in the scientific process should be democratically legitimate – where "democratically" can obviously be understood in several ways. An alternative view is to consider some values bannable or incorrect, while other values might stand the test of being held accountable to the facts – a view that presupposes some values used in the scientific process are objectively (in)correct. A third view might give scientists space to choose the values they prefer themselves, choices that could be disclosed following a transparency requirement.

Taking into account the different epistemic interests in science we encounter among different stakeholders, I have argued before that we should endorse scientific pluralism. Different groups of stakeholders might be interested in answering different types of questions and scientists might try to answer those questions at a level that is not only cognitively accessible, but also representing the phenomena under question in ways sensitive to the stakeholders' interests. How would this scientific pluralism fare if – following Elliott's condition of representativeness – only "our major social and ethical priorities" are being addressed? Would any space been left for approaches critical of major mainstream approaches? Or would Elliott rather envision a science where representativeness implies an alignment of values between certain scientific communities and certain stakeholders? Would that not eventually result in a number of isolated research approaches (each consisting of a particular scientific community teamed up with likeminded stakeholders) not communicating or engaging any longer with other research approaches as any scientific disagreement might be explained away by a difference in value judgment or in epistemic interest? Questions abound once we interrogate Elliott's conditions of representativeness and transparency in relation to scientific practice as I illustrate using case-studies from economics and political science (both to show how Elliott's conditions would play out in these disciplines as well as to compare Elliott's conditions with the conditions/devices these disciplines have developed themselves in order to deal with values in their discipline).

In short, I aim to provide a thorough analysis of the strengths and weaknesses of Elliott's first two conditions, how they would affect scientific pluralism and what it teaches us about the relations between science and democracy.

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CONSTRUCTIVE DELIBERATION: POOLING AND STRETCHING MODALITIES

Session 13B

Congress section(s): A2

When a group of agents deliberates about a course of action or decision, each of the individual agents has distinct (soft or hard) constraints on what counts as a feasible alternative, evidence about potential alternatives, and higher-order evidence about the other agents’ views and constraints. Such information may be to some extent shared, but it may also be conflicting, either at the level of a single individual, or among the agents. In one way or another, sharing and combining this information should allow the group to determine which set of alternatives constitutes the decision problem faced by the collective.
We call this process constructive deliberation, and contrast it with the selective deliberation that takes place when a set of alternatives has been fixed and the group is supposed to select one of them by means of some decision method such as voting. Whereas selective deliberation has been investigated at length (in social choice theory and game theory), constructive deliberation has received much less attention, and there is hardly any formal account of it on the market. In the first part of our talk, we will investigate this distinction, and discuss the similarities and differences between both processes as they bear on formal modeling and considerations of rationality and equality.
In the second part, we will focus on the static aspect of constructive deliberation and on the role of constraints. We will hence ask how the output, a set of viable alternatives constituting a collective decision problem can be obtained from a given input: a tuple of sets of constraints, one for each agent. We model this input in terms of a neighborhood semantics, and show how the output can be obtained by suitable combinations of two types of operations on neighborhoods: pooling (also known as aggregation or pointwise intersection) and stretching (also known as weakening or closure under supersets). We provide a sound and complete logic that can express the result of various such combinations and investigate its expressive power, building on earlier results by Van De Putte and Klein (2018). If time permits, we will also connect this work to the logic of evidence-based belief (van Benthem & Pacuit, 2011; Özgün et al., 2016) and the logic of coalitional ability (Pauly 2002).
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THEORETICAL VIRTUES IN EIGHTEENTH-CENTURY DEBATES ON ANIMAL COGNITION

Session 14H

Congress section(s): C3

This paper discusses the role of the theoretical virtues (i) unification, (ii) simplicity, and (iii) scientific understanding in eighteenth-century debates on animal cognition. It describes the role that these virtues play in the construction of different theories of animal cognition and aims to establish the relative weight that these virtues were assigned. We construct a hierarchy of theoretical virtues for the biologists and philosophers Georg-Louis Leclerc Buffon (1707-1788), Hermann Samuel Reimarus (1694-1768), and Charles-Georges LeRoy (1723-1789) and Etienne Bonnot de Condillac (1714-1780). Through discussing these virtues and the importance assigned to these virtues by different authors, we can determine how different theoretical virtues shaped and determined the theories articulated by Buffon, Reimarus, Le Roy and Condillac.
Theoretical virtues such as unification, simplicity and scientific understanding have received a lot of attention in the philosophical literature. An important question is how these different theoretical virtues relate and how they are supposed to be ranked. We can imagine questions such as the following: confronted between a choice between a simple theory and a theory that yields unified explanations, do we, other things being equal, prefer simple theories over theories that yield unified explanations? Or do we prefer theories that yield scientific understanding over simple theories? To answer these types of questions requires making a hierarchy of theoretical virtues. In this paper, I do not have the systematic aim of constructing such a hierarchy. Rather, I have the historical aim of showing that eighteenth-century debates on animal cognition can be profitably understood if we analyze the relative weight that different authors assigned to different theoretical virtues.
I will show that within eighteenth-century debates on animal cognition we can distinguish three core positions: (a) Buffon’s mechanism, (b) Reimarus’ theory of instinct, and (c) Le Roy’s and Condillac’s sensationalist position which assigns intelligence to animals. I show that these positions are partly shaped by the theoretical virtues that these authors adopted. Thus, Buffon’s mechanism is shaped by his acceptance of unification as a theoretical virtue, Reimarus’ theory of instinct is shaped by his adoption of a particular virtue of simplicity, whereas Le Roy’s and Condillac’s sensationalist position is shaped by their acceptance of the theoretical virtue of scientific understanding.
I will further argue, however, that the way in which Buffon, Reimarus, Le Roy and Condillac understand different theoretical virtues is also shaped by their theoretical commitments. Thus, for example, Buffon’s mechanism influences the way he conceives of unification. Although the appeal to different theoretical virtues thus partly explains the theoretical position articulated by Buffon, Reimarus, Le Roy and Condillac, the converse is also true: their theoretical positions shaped the way in which they conceived of different theoretical virtues. Finally, I show that the different theories on animal cognition could often appeal to the same theoretical virtues for support. This means that the theoretical virtues are sometimes incapable of necessitating a choice between different theories.

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PYTHAGOREAN ARITHMETIC AS A MODEL FOR PARMENIDEAN SEMANTICS

Session 28H

Congress section(s): A4

The question of the relation of mathematics and logic in ancient Greece has puzzled many historians, who view no connection between Greek geometrical demonstration and logical reasoning as conducted within Aristotle’s syllogistics and Stoic propositional logic [Mueller 1974].
However, Pythagorean number theory, as survived in later sources, has the following distinctive features: i) arithmetical reasoning is conducted over a 3-dimensional “domain” that extends indefinitely in the direction of increase. ii) The monas, denoted by an alpha, is taken to be a designated object, over which an iterative procedure of attaching an alpha is admitted. Thus, numbers are defined as finite suites. Various kinds of numbers can then be defined as suites constructed according to certain rules. iii) Arithmetic is then developed by genetic (recursive) constructions of various finite (plane or spatial) schematic patterns. Thus, Pythagorean arithmetic is advanced as a visual theory of counting over a combinatorial “domain.” iv) Arithmetical reasoning is conducted in the form of mental experiments over concrete objects of combinatorial character (e.g. pebbles). Any assertion about numbers utters a law, which can be confirmed in each case by combinatorial means. v) Arithmetic concerns affirmative sentences stating something ‘positive’ that can be confirmed by means of the construction of the corresponding configuration (deixis). None kind of ‘negative’ sentences is found [Vandoulakis 2010].
On the other hand, in Parmenides’ poem On Nature certain semantic views have made their appearance for the first time: a) only what is true is expressible (meaningful). b) a false statement names or expresses nothing (is meaningless). This leads to a peculiar semantic picture where truth is identified with expressibility (meaningfulness) while falsity is identified with

inexpressibility (meaninglessness). Parmenides' ontological universe therefore turns out to be a positive (negationless) true Being; there are no negative facts in it [Vandoulakis 2015].
Now, the relation between Parmenides' semantic viewpoint and Neo-Pythagorean arithmetic can be expressed as follows: Parmenides' theory of truth could be obtained by reflexion upon Pythagorean arithmetic, if truth is identified with genetic constructability (deixis). In this case, the Being is identified with the universe of all arithmetical constructible ('experimental') truths. In other words, Pythagorean arithmetic might have served as a model for Parmenidean semantics.
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FORKING AND CATEGORICITY IN NON-ELEMENTARY MODEL THEORY

Session 19A

Congress section(s): A1

The classification theory of elementary classes was started by Michael Morley in the early sixties, when he proved that a countable first-order theory with a single model in some uncountable cardinal has a single model in all uncountable cardinals. The proof of this result, now called Morley's categoricity theorem, led to the development of forking, a notion of independence jointly generalizing linear independence in vector spaces and algebraic independence in fields and now a central pillar of modern model theory.
In recent years, it has become apparent that the theory of forking can also be developed in several non-elementary contexts. Prime among those are the axiomatic frameworks of accessible categories and abstract elementary classes (AECs), encompassing classes of models of any reasonable infinitary logics. A test question to judge progress in this direction is the forty year old eventual categoricity conjecture of Shelah, which says that a version of Morley's categoricity theorem should hold of any AEC. I will survey recent developments, including the connections with category theory and large cardinals, a theory of forking in accessible categories (joint with M. Lieberman and J. Rosický), as well as the resolution of the eventual categoricity conjecture from large cardinals (joint with S. Shelah).
(Submitted for the special session on model theory organized by John Baldwin, acronym modthy)

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REASONING ABOUT PERSPECTIVES. NEW ADVANCES

Session 5L

Congress section(s): A2, B1

Very often, our reasonings about the world and about ourselves explicitly involve references to perspectives or points of view. We reason about how some facts become the contents of certain perspectives, about how those contents are related with the perspectives that allow access to them, about how those contents are related with other contents, etc. There are some inferential patterns articulating those reasonings. And despite having an enormous methodological importance, especially in the field of social sciences, but not only, those reasonings have barely been the subject of a deep philosophical analysis.

One possible strategy to understand that sort of reasoning is to use the notion of possible worlds. When an epistemic agent does not have complete knowledge about some area, she considers a number of possible worlds as candidates for the way the world actually is. If p holds at all the worlds that the agent considers candidates, then she can be said to know p. Reasoning about perspectives would deal with facts that may appear only in some of the worlds the agent considers candidates, with facts that perhaps do not appear in any of those worlds and with the attitudes agents can have toward all those facts. However, in order to explain why an agent focuses on some of those facts, but not on other ones, maintaining the attitudes she maintains, we need something more than the notion of possible worlds. Perspectives have to be taken as indexes of evaluation different from possible worlds. There are some important logical approaches that suggest that. This is the case of Antti Hautamäki and Steven Hales. The combination of the usual modal operators with the new operators opens an interesting range of possibilities.
Our approach is compatible with these proposals. But it tries to specify the concrete inferential patterns that articulate our reasoning about perspectives. Moreover, perhaps a "logic" about these sorts of reasonings is not strictly possible. Perhaps only a pragmatic and methodological approach is possible. In any case, our objective is to identify and analyze a relevant set of inferential patterns, assuming the distinction established by Gilbert Harman between reasoning and argument. His aim was to study a sort of reasoning that produces not only changes in belief but also changes in attitudes and plans of action, a sort of reasoning that is at the same time theoretical and practical. In our contribution, we will analyze some very important inferential patterns of our reasoning about perspectives. More concretely, we will identify and analyze in detail a set of ten rules. And we will discuss some relevant connections between our approach and the classic work of Harman.
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AN EMPIRICAL CHALLENGE FOR SCIENTIFIC PLURALISM – ALTERNATIVES OR INTEGRATION?

Session 7J

Congress section(s): B1, B3, C3

Scientific pluralism has become an increasingly popular position in the philosophy of science and in the philosophy of biology in particular. One shared notion among scientific pluralists is that some or all natural phenomena require more than one theory, explanation or method to be fully understood. A pluralist claim might be metaphysical, epistemological or both. There is yet another distinction within pluralist positions which is often overlooked. Some pluralists argue that several theories or explanations should be integrated, to understand particular phenomena (e.g. Mitchell, 2002). Other pluralists rather treat different theories and explanations as alternatives (e.g. Kellert, Longino and Waters, 2006). One question yet remains: Does this distinction address the "nature" of the respective phenomena? And, consecutively: Are there genuine cases of "alternative" or "integrative" pluralism?
In this talk I challenge this perspective and argue that it is not possible to uphold the distinction of alternatives vs. integration. Using "extended inheritance" as a case study, I show how research traditions, rather than the nature of specific phenomena decide whether theories or explanations are treated as alternatives or integratable. In addition, I emphasize that heterogeneity within research fields is often neglected in respect to this question. First, I will introduce the case of small RNA inheritance and propose a pluralist interpretation: Small RNAs are a class of biomolecules that are responsive to environmental stimuli and can transmit this information to subsequent generations. It was recently argued that they operate through a use / disuse paradigm (Veigl, 2017), thus presenting an instance of Lamarckian inheritance. I will advocate pluralism of theories of inheritance. Subsequently, I will ask how to relate Lamarckism and (Neo-)Darwinism: are they alternatives, or integratable?
To answer this question I will introduce an actor-based model for the emergence and decrease of plurality within one scientific field. For the case of "extended inheritance" I point to assymetries within the research field: Proponents of Neo-Darwinism are "singularists" as they only accept one theory. Defenders of "extended inheritance" accept Neo-Darwinism

along with a Lamarckian theory, thus they are “dualists”. Although defenders of “extended inheritance” work with more than one theory and thus create plurality within their research field, they do not have a clear (neither epistemological, nor metaphysical) attitude towards this state of plurality. In conclusion, this talk will raise several problems for scientific pluralism: integration vs. alternatives; singularism vs. dualism and plurality vs. pluralism. I will provide a toolbox to address these issues and assess the trajectories of these problems within the respective scientific disciplines. In so doing, I aim at developing an approach towards scientific pluralism which is responsive towards trends within research fields.

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WHO IS AFRAID OF MODEL PLURALISM?

Session 12G

Congress section(s): B1, C3, C5, C6

In this paper, I diagnose that evolutionary game theory models are used in multiple diverse ways and for different purposes, either directly or indirectly contributing toward the generation of acceptable scientific explanations. The philosophical literature on modelling, rather than recognizing this diversity, attempts to fit all of these into a single narrow account of modelling, often only focusing on the analysis of a particular model. Recently, Cailin O’Connor and James Owen Weatherall (2016) argued that a lack of family resemblance between modelling practices makes an understanding of the term ‘model’ impossible, suggesting that “any successful analysis [of models] must focus on sets of models and modelling practice that hang together in ways relevant for the analysis at hand” (p. 11). Rather than providing an essentialist account of what scientific modelling practice is or should be, covering all the different ways scientists use the word ‘model’, I settle for something far less ambitious: a philosophical analysis of how models can explain real-world phenomena that is narrow in that it focuses on Evolutionary Game Theory (EGT) and broad in its analysis of the pluralistic ways EGT models can contribute to explanations. Overly ambitious accounts have attempted to provide a philosophical account of scientific modelling that tend to be too narrow in their analysis of singular models or small set of models and too broad in their goal to generalize their conclusions over the whole set of scientific models and modelling practices – a feat that may, in fact, be impossible to achieve and resemble Icarus who flew too close to the sun. Nevertheless, many of the conclusions in my analysis will be extendable to other sets of models, especially in biology and economics, but doubt must be cast that any essence of models can be discovered.

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RETHINKING THE GIVEN. SELLARS ON THE FIRST PRINCIPLES.

Session 10H

Congress section(s): B4

When Wilfrid Sellars undertook in his Empiricism and the Philosophy of the Mind the critique of the Myth of the given, he could hardly imagine that after a few years the terms and concepts which he introduced would be as intensely discussed as they are today. Contemporary epistemology not only discusses what the Myth of the given might be, but also it refers to the various mythologies that have emerged around the given. It has even come to regard the Myth of the given as a legend.

Although it is evident that a lot of literature has been generated around the given, there are relatively few studies that have addressed what exactly is the given. Right at the beginning of the above-mentioned work, Sellars points out that the given is said of the sensory contents, the universals, the propositions, the necessary synthetic connections, the material objects, the first principles, the given itself and the real connections. Notwithstanding, the studies which have been done about the given has reduced drastically its extension.
In particular, the given has been fundamentally thought from two aspects: as the ground of empirical knowledge or as the basis of logico-mathematical knowledge. The first line of research is represented by John McDowell. The second, by Robert Brandom. Neither of these two schools of interpretation analyzes the concept of the given assuming the extension that Sellars gave him. The analysis of what is not empirical or logical-mathematical but that is still part of the given seems to be somewhat misplaced.
In this paper, I will deepen into Sellars‘ analysis of the given to introduce the criticism that can be gathered from it in the field or study of the first principles. These can be thought as heuristic principles or as some type of presupposition. Nevertheless, Sellar’s criticism concerns anything that is thought to be admitted or supposed without being the result of a clear inferential process: anything that “must be admitted” with independence of how our concepts are applied (i.e.: the falsity of skepticism, the existence of the outside world, a minimum principle of the substance, etc.) constitutes a form of the given.
Consecuently, methodology, general epistemology of science and metaphysics of science would have to be concerned. These disciplines usually presuppose that the world works in a determined way. This determinated way implies o presuppose what tradition has called ‘first principles’. In this presupposition we find the first principles of which I will take charge. Therefore, the critique of the given focused on the first principles can easily be considered as a criticism of these disciplines.

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AN EXPLANATORY VIEW OF INDIVIDUATING NATURAL KINDS

Session 7I

Congress section(s): C3, C4, C5, C7

I argue that the boundaries of homeostatic property cluster kinds (HPC-thesis by Boyd 1989) can be drawn based on the explanatory power of the producing and maintaining mechanisms. The HPC-thesis has become the standard view of natural kinds in the special sciences. According to the thesis, natural kinds consist of property clusters produced or maintained by homeostatic mechanisms. However, the HPC-thesis has been criticised for not offering non-conventional ways to draw the boundaries of mechanisms (Craver 2009). I argue that a pluralistic view of natural kinds, which is based on the explanatory power of the homeostatic mechanisms, dissolves these problems. In short, mechanistic models with robust explanatory powers are grounded on stable kinds, which in turn enable rich inductive generalizations. However, when different classifications based on mechanisms target the same explanandum, they may have explanatory trade-offs. Hence, the causal structure of the world can be sliced into natural kinds in different ways. In defence of my view, I provide examples of psychiatric classifications, and analyse their explanatory powers based on the contrastive-counterfactual theory of explanation (Ylikoski 2001, Woodward 2003). According to the theory, explanations come with different explanatory virtues that, in turn, can be measured by the explanation’s ability to answer relevant counterfactual what-if-questions.

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DEFERENCE AS ANALYTIC TECHNIQUE AND PRAGMATIC PROCESS

Session 16G

Congress section(s): B5, B7, C5, C7

The goal of the paper is to consider what is determining the deference in cases with inaccurate and untested knowledge. Deferential concept is a sort of concept which people use a public word without fully understanding what it typically entertains (Recanati, F. “Modes Of Presentation: Perceptual vs. Deferential”, 2001). What happens on the level of the everyday use of language? There is a link between social stimulations to certain use of words, social learning, different “encouragements for objectivity”, leading to correcting of everything that is not consistent with the generally accepted use of words and meanings (Quine. Word and Object,1960) and the deference, revealing the chain of these uses, distortions, refinements, leading to some problematic beginning of use of the term.

When a philosopher is performing a conceptual analysis, and affirming the causal relationship does not care about analysis of the cause, but relies on the specialists, we say such a philosopher applies the analytic technique of “Grice’s deference” (Cooper, W. E. “Gricean Deference”.1976). This technique allows the philosopher to be free from any responsibility for explanation the nature of the causes. From this point of view, the philosopher at a certain point in her analysis defers to a specialist in the relevant science, competent to talk about the causal relationships. ‘Deferentially‘ means relying on someone’s thought, opinion, knowledge.

The wellknown post-truth phenomena is interpreted as a result of deferential attitude to information, knowledge and various data concerning reality.

Along with linguistic and epistemic deference and their forms of default and intentional deference (Woodfield, A. Reference and Deference, 2000) (Stojanovic, De Brabanter, Fernandez, Nicolas. Deferential Utterances, 2005) the so called “backfire effect” will be considered. “Backfire effect” named the phenomenon pertaining to the cases when “misinformed people, when confronted with mistakes, cling even more fiercely to their incorrect beliefs (Tristan Bridges, Why People Are So Averse to Facts. “The Society Pages”. http:// thesocietypages. org).

There is a problem, within what approach could be correllated the instances of falsity-by-misunderstanding and cases when speaker openly prefers to use expressions like it makes someone else from the nearest linguistic community, following the custom or authority.

Being a pragmatic process, deference is responsible for the lack of transparency in meta-representations (Recanati, op.cit.). So, what determines deference lies in basic concepts of the theory of opacity, in meta-representations, and in mechanism of the deference in connection with the opacity and meta-representations. The last, but not least, in this sequence is the use of the mechanism of deference to problems with imperfect mastery and unconsciously deferential thoughts (Fernandez, N.V. Deferential Concepts and Opacity).

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A COMPUTATIONAL PRAGMATICS FOR WEASELING

Session 30M

Congress section(s): B2, C5

Probabilistic expressions (PEs) have long been studied in linguistics and related fields, and are noted for their vagueness (Kent 1964). Such expressions include words like ‚possibly‘ and ‚probably‘. They express degrees of belief in propositions. Put differently, they lexicalize judgments about uncertainty. Their semantics proves highly elusive; the central problem being that, even though PEs lexicalize uncertainty, they rarely correspond to precise probabilities. To make things more complicated, there is hard evidence that the scopes of possible interpretation of PEs are governed by probability distributions themselves (Mosteller and Youtz 1990). This gives rise to higher-order probability distributions.

The interpretation of vague language is the purview of pragmatics, which can be studied at the intersection of a plethora of fields, including cognitive science, computer science, linguistics, and philosophy of language. In this paper, the usage and understanding of probabilistic expressions is viewed through the lens of recent developments in computational pragmatics. Specifically, several enriched Rational Speech Act (RSA) frameworks are developed (Frank and Goodman 2012). The RSA framework is a Bayesian, computational model of communication and should be understood as a low-level implementation of the key premise of Gricean pragmatics (Grice 1975) and Relevance Theory (Sperber and Wilson 1996). Put simply, this framework operationalizes the Gricean idea of optimal communication probabilistically. In spite of its relative novelty, the RSA framework has been used to model a vast array of linguistic phenomena: metaphor (Kao et al. 2014), politeness (Yoon et al. 2016, 2017, 2018), scalar implicature (Goodman and Stuhlmüller 2013), and adjectival interpretation (Lassiter and Goodman
2017). Moreover, the framework has also been used to offer solutions to philosophical problems, like the sorites paradox (Lassiter and Goodman 2017).

The setup of this paper is to develop several RSA-style frameworks in order to model the pragmatics of PEs. Such a model is not just of interest to linguists; I also provide a formal treatment of phenomena like plausible deniability and avoiding accountability, which are also studied studied in argumentation theory (e.g., Walton 1996). and cognitive science (e.g., Lerner and Tetlock 1999) respectively. This kind of deceitful use of vague verbiage is colloquially known as weaseling. I therefore argue that an RSA-style model of PEs can easily provide a theory of how rational agents can and should engage in weaseling. From a high-level perspective, rational
weasels should exploit Grice’s principle of cooperation to deceive their interlocutors. It is worth noting that Yoon et al. (2016, 2017, 2018) already applied an RSA model to disingenuous speech, specifically to politeness. In total, I propose six models to think about PEs and situations in which they naturally occur. Moreover, I also provide data from simulations to back up the claims I make about the power of my framework for understanding the pragmatics of PEs.

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THEORETICAL AND METHODOLOGICAL DIFFERENCES IN THE EVOLUTIONARY ANALYSIS OF HUMAN BEHAVIOR

Session 28I

Congress section(s): C3, C5

I am interested in the theoretical and methodological differences displayed by two arguably disconnected programs in the context of the evolutionary and Darwinian analysis of human behavior. Specifically, the contribution I propose is concerned with the philosophical aspects of the debate between evolutionary psychology and human behavior ecology. This topic pertains to the fields of philosophy of science (models for constructing and testing hypotheses) and philosophy of biology (applications of evolutionary biology, problems of adaptationism, and fitness maximization). It has also elements of philosophy of psychology (how to explain human behavior and emotions), philosophy of cognitive sciences (functionalism and information-processing systems), and philosophy of mind (mental modules).
Evolutionary psychologists have strong claims about the origin, architecture, and functioning of human mind (see Tooby & Cosmides 1992; Tooby & Cosmides 2016; Buss 1999). According to them, our behavior is produced by hundreds of psychological mechanisms (massive modularity thesis) that evolved during a period of time that is coextensive with the Pleistocene epoch (1.8 million years to 10,000 years ago). These mechanisms or modules (they are even called “apps” in the literature (e.g. Pinker 2016)) are said to be computational in nature and to respond to those statistically recurrent problems faced by our hunter-gatherer ancestors. The method used by evolutionary psychologists to discover the psychological mechanisms that underlie human behavior (and to find out consequently what they think it is the pan-human nature) is reverse engineering or functional analysis. Since the modules are adaptations designed by natural selection, the behavior they produce is supposed to be functional to the problems of the ancestral world and not necessarily to our current modern problems. So we should investigate to solving which specific problems the still operating modules that compose the human mind were designed for. This analysis is based on the idea that present behavior is not necessarily adaptive. That is why evolutionary psychologists reject fitness maximization as an explanation for behavior.
I contrast the above-described theoretical and methodological tenets with the view defended by human behavior ecology. This latter discipline is not committed with any hypotheses about psychological mechanism or modules. Its attempt to explain human behavior does not take into account such mechanisms. It is just concerned with manifest, present, and measurable behavior (see Downes 2001). Furthermore, human behavior ecologists criticize the massive modularity thesis for lacking relevant empirical evidence (see Smith et al. 2001). The goal they pursue is to explain how behavior changes in predictable patterns in relation to the environment (see Nettle et al. 2013; Cronk 1991). Unlike evolutionary psychology, human behavioral ecology works with the assumption of fitness maximization. This means their practitioners see present behavior as adaptive. That is why they try to construct complex formal models for interpreting present behavior as the optimal response for changes suffered by the environment.
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MATHEMATICAL DEPTH AND EXPLANATION

Session 18J

Congress section(s): C1

Mathematicians often use the term ‘deep’ to signal work of exceptional quality or importance. Recently, Maddy has prompted discussion of this concept in response to her claim that there are objective facts of mathematical depth that guide proper axiom choice. While various ways of understanding the concept of depth appear subjective in nature, there is some promise that mathematical explanation is associated with a kind of objective depth. In this paper, I develop this idea by discussing how mathematical explanation arises not only in the form of explanatory proof, but more generally in mathematical theory, and note several mathematical programs that may be thought of as explanatory. I then focus on Friedman’s project of Reverse Mathematics, which is shown to exhibit some very general explanatory features that are common to both science and mathematics. The framework for Reverse Mathematics is used to illustrate the concept of an explanatory basis, which in turn is used to further develop the idea that explanatory power constitutes a type of mathematical depth that possesses a number of the significant features that have previously been suggested as markers of depth.

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GEOMETRY, PSYCHOLOGY, MYTH AS ASPECTS OF THE ASTROLOGICAL PARADIGM

Session 3G

Congress section(s): B3

The geometrization of logic, undertaken by Wittgenstein in “Tractatus Logico-Philosophicus”, is related to the spatial representation of the complex. The form of the fact is the formula of the complex “aRb”, where “a” is one event, “b” is another one, and “R” is the relationship between them. The geometric form of the complex will be “a-b” or “a ~ b”, where the ratio is expressed as “straight line” or “curve”.
The visualization of life in the form of geometric figures allows comprehending the past in a new way, making the prediction part of the visual image itself, a part of the calculation. The new concept of the picture is a consequence of a new understanding of the experience of life. This is the essence of astrological predictions.
Step-by-step analysis of an astrological picture creation: (1). Initially, based on the calculations, two pictures are created. (2). One picture combines two observations; location of the planets at the time of birth and the current location of the planets. We combined two homogeneous, but chronologically different experiences. (3). The third step is structuring life experience. We are looking for the starting point of the geometric projection. We begin to build the figure of experience. (4). We presented the result to the inquirer and asked him to recall the events on a certain date. He creates a picture of his past. We attempt to include the content of his experience into our picture of the events. (5). Now, making predictions based on the picture itself (and this is computation, since all the elements of the picture are connected by rigid connections), we operate on the experience of the questioner. (6). Astronomical calculations of the points and movements of planets become the rule and bring an intuitive perception of the experience of life. “Experience” is now not only “what was”, but also “what should have been”. The prediction became part of the image itself. Mathematical expectation is the basis of psychological persuasion. According to Wittgenstein, “the cogency of logical proof stands and falls with its geometrical cogency”[1]. Particular interest are the problem of differences in conclusions and predictions and, after all, the transformation of thinking itself in the process of creating a geometrical concept. Astrology, through the ideogrammatical form of the writing, turned out to be related to geometry and mathematics, it became tools for predicting.

Archaic origins of astrological representations, based on the picture, can be found in the most ancient languages - for example, in the Sumerian, where words expressing the meaning of representations about the fortune and fate, derive from the root with the meaning of “drawing” and “mapping”[2]. The hieroglyph denoting the Egyptian god of magic and astrology Thoth corresponded to the Coptic transcription: Dhwty-Deguti. One of the options for translating this name is the word “artist” (W.Goodwin).

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THE SCIENCE WE NEVER HAD

Session 8A

Congress section(s): B1

In this paper, I argue that there are historiographical and philosophical reasons to resist the idea that there have been sciences in the past. Here I draw on the insights from the historians of science. If there were no sciences in the past, it is difficult to see how the history of science could provide evidential support (or falsifications) for the philosophical theories of science. I examine different ways of understanding the relationship between the history and philosophy of science in the situation where the practices of the past cannot be judged as sciences. I argue that among the alternatives there are three main lines along which the philosophy of science may proceed. 1. We can study how science would have been different, had its history been different. 2. We can test philosophical accounts using counterfactual scenarios. The question is not whether an account captures what actually happened but what would have happened, had science proceeded in accordance with the account. 3. We can estimate the possible future developments of science by studying what factors behind the development of science could change either due to a human intervention or due to a change in other area of society. I point out that each of the lines 1–3 requires that counterfactual scenarios are built. Luckily, each of the lines can be shown to be a variation of the structure that is implicit in the explanations in the historiography of science. Moreover, I argue that this general structure if often implicit in more traditional case studies in the philosophy of science, and therefore the lines 1–3 are not too exotic despite the first impression. I conclude that the value of history of science is that it provides the materials to build the counterfactual scenarios.

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NUMBERS AS PROPERTIES, DISSOLVING BENACERRAF’S TENSION

Session 15F

Congress section(s): B4, C1

Generations of mathematicians and philosophers have been intrigued by the question, What are arithmetic propositions about? I defend a Platonist answer: they‘re about numbers, and numbers are plural properties.
I start with the seminal “Mathematical Truth” (1973), where Benacerraf showed that if numbers exist, there is a tension between their metaphysical and epistemological statuses. Even as Benacerraf’s particular assumptions have been challenged, this tension has reappeared. I bring it out with two Benacerrafian requirements:
Epistemic requirement. Any account of mathematics must explain how we can have mathematical knowledge.
Semantic requirement. Any semantics for mathematical language must be homogenous with a plausible semantics for natural language.
Each of the prominent views of mathematical truth fails one of these requirements.

If numbers are abstract objects, as the standard Platonist says, how is mathematical knowledge possible? Not by one common source of knowledge-causal contact. Field (1989) argues that the epistemological problem extends further: if numbers are abstract objects, we cannot verify the reliability of our mathematical belief-forming processes, even in principle. If mathematical truth amounts to provability in a system, as the combinatorialist says, the semantics for mathematical language is unlike those semantics normally given for natural language (‚snow is white‘ is true iff snow is white, vs. ‚2 + 2 = 4‘ is true iff certain syntactic facts hold).
I argue that numbers are properties.
Epistemic requirement. We‘re in causal contact with properties, so we‘re in causal contact with numbers. More generally, because a good epistemology must account for knowledge of properties, any such theory should account for mathematical knowledge.
Semantic requirement. Just as ‚dog‘ refers to the property doghood, ‚2‘ refers to the property being two. Just as ‚dogs are mammals‘ is true iff a certain relation holds between doghood and mammalhood, ‚2 + 2 = 4‘ is true iff a certain relation holds between being two and being four.
Specifically, I say that numbers are what I call pure plural properties. A plural property is instantiated by a plurality of things. Take the fact that Thelma and Louise cooperate. The property cooperate doesn‘t have two argument places: one for Thelma, and one for Louise. Rather, it has a single argument place: here it takes the plurality, Thelma and Louise. Consider another property instantiated by this plurality: being two women. This plural property is impure because it does not concern only numbers, but we can construct it out of two other properties, womanhood and being two. This latter plural property is a pure. It is the number two.
Famously, number terms are used in two ways: referentially (‚two is the smallest prime‘) and attributively (‚I have two apples‘). If numbers are objects, the attributive use is confounding (Hofweber 2005). If they‘re properties, there is no problem: other property terms share this dual use (‚red is my favorite color‘ vs. ‚the apple is red‘).
The standard Platonist posits objects that are notoriously mysterious. While the nature of properties may be contentious, my numbers-as-properties view is not committed to anything so strange.

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PROBLEM REDUCTION AS A GENERAL EPISTEMIC REASONING METHOD

Session 27E

Congress section(s): B2

We introduce a general (epistemic) reasoning method based on problem reduction and show its use and discuss its justifiability in several disciplines.
To introduce our concept we rephrase and extend the question-answer approach of A. Blass [B]. We consider a problem (problem domain) P = (I, S, A) consisting of set of problem instances I, set of potential solutions S and acceptability relation A, where A(i,s) means that a particular solution s in S is acceptable for a problem instance i in I.
Problem reduction occurs e.g. when a problem solution is delegated to an expert, or a client computation asks a server to do a part of a job, or an agent with limited resources asks agent with richer knowledge.
Assume we have two problem domains P1 = (I1, S1, A1) (sometimes called target domain) and P2 = (I2, S2, A2) (sometimes called source domain). We consider a typical scenario: assume we are not able (or it is very complex or very expensive) to solve problem instances from I1. Moreover, assume there is a problem domain P2 where we have methods to find acceptable solutions (efficient, cheaper). If we happen to efficiently reduce problem instances from I1 to problem instances of I2 in such a way that acceptable solutions in S2 can be transformed to acceptable solutions to original problem instance, we are done. There is a wide space for what acceptability can mean. It can be e.g. correct, reasonable, reliable, etc.
Problem reduction (PR). Reduction of a problem P1 to a problem P2 consists of a pair of mappings (r, t): r maps problem instances i1 in I1 of P1 to problem instances r(i1) in I2 of P2 and t maps solutions s2 in S2 to solutions t(s2) in S1 in such a way, that an acceptable (in the sense of relation A2) solution s2 to instance r(i1) is transferred to an solution t(s2) which is A1-acceptable to original problem instance i1. Formally we require: for all i1 in I1 and s2 in S2

A2(r(i1), s2) implies A1 (i1, t(s2)) holds true. (PRi)

Motivated by [Hr] we combine decision and search problems, and assume every set of solutions contains an extra element "nas" = "no_acceptable_solution" and we require the above implication should be valid also for r2 = nas2 and t(nas2) = nas1. This helps us to avoid empty fulfillment of (PRi) implication and to preserve category theoretical character of problem reductions.

Our approach generalizes analogical reasoning [A], in that we show that along of similarity it works also under some quite complementary situations

Following [He] we can read following question answer:

SPIEGEL: And what now takes the place of Philosophy?

Heidegger: Cybernetics.

We will show that our general (epistemic) reasoning method based on problem reduction can be used to understanding cybernetics as modeling of dynamic processes with feedback. This can shed light to Heidegger's answer.

Another application comes from modeling and abstraction in System Sciences. Inspired by Peter Senge [S], we propose the correlation of the organizational analysis' depth with the different type of possible actions (solutions). First reducing problem instances from events analysis to patterns of behavior analysis we reach finally systemic structure analysis (on problem instances side). Finding an acceptable generative action and transforming back along the solution side to responsive action and finally to reactive action closes the use of our back and forth reduction and translation.

We consider also use in the management by objectives model in organizational envisioning and narration. These were justified empirically in a certain degree of acceptance of solution. Our reasoning works also under uncertainty.

Problem reduction itself, as a reasoning method, can be quite challenging (similarly to finding mathematical proofs). Nevertheless we believe that advantages of finding P2, r, t and proving implication (PRi) for solving P1 are worth of these difficulties.

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INTEGRATED HPS? FORMAL VERSUS HISTORICAL APPROACHES TO PHILOSOPHY OF SCIENCE

Session 20L

Congress section(s): B1, B2

While recent decades have seen a thorough-going synthesis of history and philosophy of science—most notably in the form of the ' Integrated History and Philosophy of Science' movement—the gap between history and formal philosophy of science remains as wide as ever. In this talk, I argue that the divide between historical and formal philosophies of science is unwarranted and that by drawing the right lessons from existent literature we can overcome their century-long separation. I start by considering the origin of the supposed dichotomy between history and formalism in philosophy of science in the reception of Kuhn's The Structure of Scientific Revolutions (1962). Drawing on recent literature on the Kuhn-Carnap and Kuhn-Stegmüller connections, I argue that the contrast between Kuhn's approach with that of the antecedent formal-sentential analyses of science (exemplified by Carnap) and the subsequent formal-structuralist analyses (exemplified by Stegmüller) is not as stark—and undoubtedly more nuanced—than it is presumed to be in many contemporary appraisals. Following this, I highlight several aspects of existent formal philosophies of science that, so I argue, make them ill-suited for historicization. In particular, I focus on the school of formal philosophy in which the history of science features most prominently, viz. the Munich-brand of set-theoretic structuralism and its analysis of the 'diachronic structure' of scientific theories as expounded by Stegmüller (1976) and Balzer, Moulines and Sneed (1987). These analyses, I argue, suffer from an overly simplistic approach to the history of science, as exemplified by (i) an overly narrow construal of the science-world relation and (ii) an overly narrow construal of the role of mathematics in science. To overcome these issues, I argue that formal methodologies need to move from considering the structure of scientific theories simpliciter to considering various supratheoretical, contextual elements of scientific knowledge as well.

Finally, I put forth a positive proposal that I take to do just this. To this end, I introduce the metalogical framework of abstract model theory. After briefly delineating my proposal from that of Pearce and Rantala (1983), the first authors to apply abstract model theory to philosophy of science, I sketch how this framework may be used to arrive at a formal picture of science that does justice to the plurality of different knowledge-systems we find throughout history.

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GÖDEL AND CARNAP ON THE IMPACT OF INCOMPLETENESS ON FORMALIZATION AND UNDERSTANDING

Session 29C

Congress section(s): C1

On the basis of the proof of the incompleteness theorem in 1931, Gödel and Carnap both drew conclusions on the open character of mathematics, noting either that "whichever system you chose (...) there is one more comprehensive" (Gödel, On the present situation of the foundations of mathematics, 1933) or that "mathematics requires an infinite series of always richer systems" (Carnap, Logische Syntax der Sprache, 1934, §60). However, such similar formulations concerning the immediate consequences of the incompleteness theorem are misleading because Gödel's and Carnap's respective reactions to incompleteness were actually fundamentally different. In this paper, it will be argued

1) that incompleteness had a deep impact not only on the general issue of the limitations of formalisms but more precisely on both Gödel's and Carnap's conception of formalization and understanding — but in completely different directions;

2) that Gödel's and Carnap's foregoing remarks on the open character of mathematics do not provide a sufficient account on their views on the impact of incompleteness on formalization and understanding (and it will be shown why it is so);

3) that a full account on theirs views concerning this point requires a distinction between kinds of incompleteness, and that this illuminates their divergent conceptions of mathematics and philosophy in a new way.

On the basis of this analysis, it will be shown how a new interpretation of Carnap's reading of incompleteness can be provided.

On Gödel's side, a satisfactory interpretation of his views on the incompleteness of theories and its impact on formalization and understanding requires the distinction between two kinds of incompleteness, which depend on the character of the independent sentences. On the one hand, let us consider for example the Gödel sentence G which Gödel's incompleteness theorem proves is neither provable nor refutable in T (for any consistent and axiomatizable extension T of Robinson's arithmetic); as Gödel himself argues, the arithmetical sentence G may be shown to be true (true in the standard interpretation of the language of T) but this sentence is completely uninteresting from the viewpoint of ordinary mathematics (although it has a tremendous logical significance). On the other hand, let us consider for example a sentence such as Cantor's Continuum Hypothesis (CH), which is independent of a classical theory of sets such as ZFC; CH is mathematically highly significant although it is not known to be true by mathematical standards. The fact that Gödel's reaction to these different kinds of incompleteness was different is highly significant for his views on mathematics, on what he thought about the impact on formalization and understanding, and about the connection between formalization and mathematical practice.

As for Carnap, it is well-known that like other logicians, he tried to circumvent incompleteness by devising a method for formalization to which Gödel's theorem would not apply. His ideas was to have recourse to infinitary methods for the definition of such concepts as "consequence" and "analyticity", which enabled him to prove some kind of Carnapian completeness theorem, to the effect that every logical (including mathematical) sentence in some language L for the reconstruction of science is either analytic or contradictory. This is possible because "analytic" does not reduce du "provable" and "contradictory" does not reduce to "refutable". With this result in mind, what is not easy to understand is the impact Gödel's incompleteness theorem really had on Carnap's philosophy of mathematics in the Logical Syntax of Language and what the actual significance of Carnap's remark to the effect that "mathematics requires an infinite series of always richer systems" is. A new interpretation will be proposed which will show that Carnap's reaction to Gödel's incompleteness theorem and its impact on formalization and understanding has to take into account the connection between formalization and the actual development not only of mathematics but of science in general (and why it is so).

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CAN CONVENTIONALISM SAFE THE IDENTITY OF INDISCERNIBLES?

Session 17H

Congress section(s): B4, C2

In Hacking's 1975 paper, The Identity of Indiscernibles, he argues that spatiotemporal examples, like Kant's two indiscernible drops of water, are inconclusive in establishing or refuting a metaphysical principle like the Leibnizian Principle of the Identity of Indiscernibles [I/I]. This principle postulates that somehow there has to be some difference between two objects, otherwise they are identical to each other and therefore only one object. In cases like Kant's two drops of water, which appear to violate the I/I, Hacking re-describes the possible world until there is only one object left which now obeys the I/I. So, his main thesis is that "there is no possible universe that must be described in a manner incompatible with I/I" (p. 249).
My aim is to show that Hacking's argumentation is based on a Poincaré-like conventionalism. With this underlying framework, it becomes easier to see how Hacking's argumentation works and how it can be properly used regarding physical questions, like how we should deal with seemingly indistinguishable quantum particles. Instead of the prior two drops of water, I will apply his argument to two indistinguishable classical particles. As I will discuss, Hacking's re-description works well for such permutation variant particles with well-defined trajectories. But his explanation fails with regard to quantum mechanical objects, which are permutation invariant and have untraceable trajectories, since they have none. Furthermore, under certain circumstances there is no definite description of a twoparticle state, if they are in a so-called mixed state. In such situations there is no way to give different descriptions of the particle states, which means there only exists a single description with no proper re-description. Confronted with this quantum mechanical situation, Hacking's conventionalist approach cannot hold. Such indistinguishable particles violate the I/I – which Hacking rejects as impossible – and there is no possible world description which re-describes the situation in a way the I/I could be defended.
Therefore, we must conclude that Hacking's (original) argument, that every possible world can be described in such a way that the I/I is preserved, holds only for classical particle systems, but not for quantum mechanical ones. Nevertheless, there still seems to be a possibility to defend Hacking's claim against the quantum mechanical challenge by going beyond quantum mechanics. If we take conventionalism seriously, it is challenged by quantum descriptions of mixed states. But following Hacking's line, we can consider whether Quantum Field Theory, which provides a different description of bosonic particles by treating them as field excitations instead of indistinguishable particle states, could be seen as a possible way to re-describe a given system. This would imply that there is in fact a possible re-description of states where quantum mechanics fails to give satisfying explanations. Furthermore, the I/I could be saved again along with Hacking's argument.

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UNFALSIFIABILITY AND DEFEASIBILITY

Session 3I

Congress section(s): B1, B2

Popper's falsifiability criterion requires sciences to generate falsifiable predictions, and failing the criterion is taken as a vice of a theory. Kitcher (1982) rejects the criterion by arguing that there is no predictive failure to be derived from scientific theories in Popper's sense, and thus Popper-style falsification of a scientific theory cannot be fulfilled. We aim at reconstructing Kitcher's argument on the unfalsifiability of a scientific theory based on the defeasibility of scientific inferences, and further indicate how the unfalsifiability can aid scientific discovery. The reconstruction shows that the unfalsifiability of a scientific theory is a virtue rather than a vice of the theory.
Our main argument proceeds as follows. First, we reorganized Kitcher's (1982: 42) argument for the unfalsifiability of scientific theories, and indicate that his main argument is based on that no theory can logically derive a conditional prediction in the form of "if P then Q", since any such conditional is incompatible with the scientific practice that, in case of P but not Q, we can always appeal to some extra intervening factor (e.g. some other force than those under consideration) to explain why it is the case that P and not Q.
The second step is to indicate that Kitcher's argument is unsatisfactory, since a conditional in the form of "if P then Q" is incompatible with the practice of appealing to extra intervening factors only if the conditional allows the inference pattern modus ponens (e.g. the material implication and the counterfactual conditional), that Q logically follows from that if P then Q and that P. But the literature has shown that not all sensible conditionals enjoy modus ponens. But the literature has shown that not all sensible conditionals enjoy modus ponens. Furthermore, Kitcher's argument is puzzling from two aspects: his argument makes it unclear how a theory can be used to generate a prediction, and it is also unclear how the appealing to extra intervening factors works in scientific practice.
Finally, to respond to the two puzzles but still hold the main objective of Kitcher's argument, we propose that a conditional prediction to follow from a theory is the defeasible conditional "if P then defeasibly Q", from which, given P, Q only defeasibly follows but not logically entailed. This proposal in turn is shown to be compatible with the the practice of appealing to extra intervening factors. We also formalize the practice of appealing to intervening factors by a defeasible inference pattern we call the inference from hidden factors, which represents a way how we can learn from mistaken predictions without falsifying scientific theories. Our proposal is defended by indicating that the defeasible inference patterns can capture good features of scientific reasoning, such as that we generate prediction from a theory based on our partial ignorance of all the relevant facts, and how we may change our prediction when evidence accumulates.
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ON SOCIAL REALITY: TAKING THE ENTERPRISE AS AN EXAMPLE

Session 7K

Congress section(s): C7

This paper will analyze the social reality by taking the enterprise as an example. As the enterprise is a social reality which exists in a collective way, rather than an individual way, the analysis starts from a controversial issue that whether one can

accept the group as a social reality. The point at issue is actually the argument between methodological individualism and methodological holism.

Historically the Old Institutional Economics (OIE), for example Thorstein Veblen, has strongly advocated the holism. However, when the New Institutional Economics (NIE) grows up, it turns to the individualism. Malcolm Rutherford concludes that "just as holism is the professed methodology of the OIE, so individualism is the professed methodology of the NIE". [1] The shift in methodology of economics illustrates the methodological individualism has a strong position in Western social sciences camps, from economics to philosophy and sociology.

In theory, the methodological individualism can take the superiority in terms of a hypothesis that seems cannot be refuted, that is only individuals have intentions, not groups. The methodological individualism intends to reduce the "collective intentionality" to "individual intentionality" plus something else. However, this hypothesis was rejected by a number of philosophers, such as John R. Searle and Raimo Tuomel. Searle denied the possibility that collective intentionality can be reduced to individual intentionality, and explained the collective intentionality as the "intentionality in the form of the first person plural as much as we can have it in the form of the first person singular". [2] Tuomel argued that "many social and collective properties and notions are collectively man-made", and "sociality is in many cases created through collective acceptance". [3, p. 123] According to him, "we-mode collective commitment may be based on subjective commitment (involving only personal normative thoughts) and beliefs about others, or it can be interpersonal commitment (involving appropriate interpersonal norms or normative thoughts), or it can be objective commitment (viz. commitment in the public space based objective norms, epistemically available to anyone)." [3, p. 133]

Searle and Tuomel had greatly contributed to the studies on the social reality, but their focus is on the relationship of exchange and intentionality. It leads that Searle's example is money and Tuomel's is squirrel pelt which has played a money role in Finland. When turning to the interpersonal relationships in production, this paper will choose the enterprise, the cell in the modern production, as an example.

In general, this paper believes that the characteristic of the enterprise as a social reality in three respects. First, the institutional reality of contract. Various communities, such as the enterprise, family, and state, all have institutional characteristic. As far as the enterprise is concerned, it is widely acknowledged a nexus of contract in economics. Accordingly, the enterprise basically is the institutional reality of contract. Second, the reality of role structure. The enterprise is a collective consisting of individuals (or human being).When an individual becomes a member of the enterprise collective, he or she will be allocated to a specific post with the relevant rights and obligations. Then an individual turns into a special "role" in the community, and, in this sense, the enterprise is necessarily a "reality of role structure". Third, the reality of physical facilities. The traditional approach to reality analysis always focuses on consciousness, language, and their relationship, instead of the "material facilities", "material basis", and "material condition". However, without the physical office and production facilities, such as the factory building, machine and equipment, the enterprise cannot carry out the material production.

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A THREE-VALUED PLURALIST SOLUTION TO THE SORITES PARADOX

Session 6M

Congress section(s): A2

Many philosophers believe that the three-valued approach to the sorties paradox is a wrong approach to solve the paradox. There are two main objections to the three-valued approach in the literature. One of them focuses on the fact that the approach is truth-functional and thereby cannot do justice to the phenomenon of penumbral connection. The other says that the three-valued approach falls foul of 'the problem of higher-order vagueness' by imposing sharp cut-off points on a sorites series where there should be none. Disagreeing with this popular and negative opinion, the author proposes and endorses a solution that he calls 'three-valued pluralism' to the age-old sorites paradox. In essence, it is a three-valued semantics for a first-order language with identity with the additional suggestion that a vague language has more than one correct interpretation. Unlike the traditional three-valued approach to a vague language, so the author argues, the three-valued pluralism can accommodate both the phenomenon of higher-order vagueness and the phenomenon of penumbral connection when equipped with 'suitable conditionals'. Specifically, the author shows how to define a definite operator within this pluralist framework to reply to Williamson's objection to the three-valued approach in (2004) and (2007), and shows how to define a conditional operator within this framework to accommodate the phenomenon of penumbral connection. The author also shows that the three-valued pluralism is a natural consequence of a restricted form of the Tolerance Principle ((RTP): If it is correct for a subject S to classify x as a member of F+ (or F−(on an occasion o and y and x are 'very similar' in F-relevant respects for S on o, then it is also correct for S to classify y as a member of F+ (or F−) on o, so long as, after it is so classified, the overall difference in F-relevant respects between any member of F+ and any member of F− remains salient for S on o.) and a few related ideas, and argues that (RTP) is well-motivated by considerations about how we learn, teach, and use vague predicates. Finally, the author compares his proposal with Raffman's recent proposal in (2014).

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THE CIRCULATION OF EPISTEMIC VALUES BETWEEN MATHEMATICAL CULTURES: THE EPISTEMIC VALUES PURSUED BY J.-L. LAGRANGE IN HIS TEACHING OF ANALYSIS AT THE ECOLE POLYTECHNIQUE

Session 29G

Congress section(s): B3

In the context of the scientific and education reforms that took place during the French Revolution, J. –L. Lagrange (1736-1813) participated in many activities such as the reform of system of measurement units, and the teaching of analysis at the newly founded Institutions, notably the Ecole Normale of the year III and the Ecole Polytechnique. His teaching at the Ecole Polytechnique, which lasted from 1795 until 1799, gave rise to two important publications on analysis, that is, Théorie des fonctions analytiques (first appeared in 1797) and Leçons sur le calcul des fonctions (first published in 1801). In these two works, Lagrange advocates to ground the Calculus on his theory or method of "derived functions". In the introductions of his books, Lagrange first examines the methods employed by past practitioners, including the methods of Newton, Leibniz and their followers. Through taking a close look at Lagrange's historical treatment in these two publications, this talk aims at clarifying the purpose that Lagrange assigned to this historical narrative. I will argue that in his books on analysis, Lagrange pursues four epistemic values, that is, generality, rigor, clarity and simplicity and that his pursuit of these values can be correlated with the reasons he gives to reject all the conceptualizations by his predecessors. More importantly, my claim is that these four values brought together epistemic values that were prized in the 18th century practice of analysis (generality, simplicity, clarity), and a value for which Lagrange found inspiration in his reading of ancient Greek texts, that is, rigor. This can be related with Lagrange's special interest in Greek mathematical writings. The key point is that, as I will further argue, these four epistemic values played an essential role in Lagrange's determining which method should be used to deal

with Calculus. How Lagrange put forward rigor and practiced this epistemic value in the context of his own practice will be at the core of my presentation. This circulation of a key epistemic value from one context to another will have a key impact on future research in analysis. Indeed, I will emphasize that Lagrange's view had an impact on his successors who taught analysis at the Ecole Polytechnique and in particular initiated the rigorization of analysis before August Cauchy (1789-1857). For example, Joseph Fourier (1768-1830) took special care to proceed in a rigorous way in the teaching of an elementary course of analysis that he too delivered at the Ecole Polytechnique between 1795 and 1798.

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METHODOLOGICAL INDIVIDUALISM AND HOLISM IN THE SOCIAL SCIENCES

Session 6F

Congress section(s): C7

The debate of individualism and holism has been a central issue in the philosophy of social science for decades. The paper will focus on methodological individualism and holism in the social sciences. On the one hand, methodological individualism (thereafter MI), especially the method of game theory, is dominant in social inquiries; on the other hand, many schools such as network theory, structural sociology, sociological realism, and neofunctionalism in sociology insist methodological holism (thereafter MH).
Main debates between MI and MH includes the dispensability debate (whether holist explanations are indispensable in social sciences) and the microfoundations debate (whether holist explanations may stand on their own or should always be supplemented by the underlying individual-level micro foundations).
Borrowing researches in the philosophy of mind, Keith Sawyer proposes nonreductive individualism (NRI) which accepts that only individuals exist, but rejects methodological individualism. Using arguments from the philosophy of biology, the author tries to revisit the dispensability and microfoundation arguments by analyzing social causation, translatability of social concepts, intertheoretic reduction.
Firstly, social causations supervene on individual level causations, just like James S. Coleman's boat model, because the latter may falsify the former but not visa versa. Secondly, according to Philip Kitcher's multiple realizability argument in the philosophy of biology, the many-many relationship between social and individual concepts may support the untranslatability of social concepts. Lastly, since approximations are so common in social sciences as well as in biological sciences, , if there is non-singular limit as Robert Batterman describes, some social theories cannot be derived from individual level theories.
The author suggests ontological individualism but methodological pluralism which tries to integrate MI and MH. Ontologically, the social phenomena express causal power through the individual level, so the microfoundations are always helpful to understand and study social phenomena. But epistemologically, it is often difficult to translate and derive, because of multi-realization and approximation problem, so we must accept epistemic nonreductionism of social theories and social causation.

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KNOWLEDGE, REASONING TIME, AND MOORE'S PARADOX

Session 9M

Congress section(s): A2, A3

One way to increase knowledge is to activate the reasoning organs to perform a series of inferences. These inferences will bring out the hidden information in the possessed knowledge of the reasoners, such that they can then access these pieces of information and utilize them. However, this temporal aspect of the reasoning knowledge is normally implicit in our daily conversation, and ignorant in the traditional possible-world-semantics treatment of epistemic concepts. In this paper, we argue for the naturality and importance of the concept of reasoning-based knowledge, in contrast with information-based knowledge, and provide two epistemic logical systems which incorporate formulization of this reasoning-based concept of knowledge. These systems will be discussed and compared. Then I will demonstrate an application of the systems, with a detour to discuss Moore's paradox from the reasoning-based knowledge point of view.

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A PHENOMENOLOGICAL ANALYSIS OF TECHNOLOGICAL INNOVATIONS

Session 16D

Congress section(s): C8

Inspired by Martin Heidegger, the phenomenological analysis has become one of the mainstream approaches in philosophy of technology, and promoted hugely our understanding of the place of technology in the society. So far, however, such studies has been characterized by static nature to a large extent, and there is virtually no phenomenological analysis of technological innovations. For instance, the central questions in post-phenomenologist Don Ihde's investigations are what role technology plays in everyday human experience and how technological artifacts affect people's existence and their relation with the world. According to him, there are four typical relations between human and artifacts, i.e., embodiment relation, hermeneutic relation, alterity relation and background relation. These relations are those between the "given" user and the "given" artifact under the "given" life world, taking no account of the creation of artifacts and dynamic interaction among the user, new artifacts and the world. It is true that Heidegger himself discussed the "present-at-hand" state which could lead to further "thematic investigation", a very beginning of the innovative practices. In this way, his analysis certainly suggests the emergence of innovative practices. However, his focus was mainly put on the structure of the routine practice, and the very essence of innovative practice was largely put aside.
In this paper, the author attempts to develop a phenomenology of technological innovations to make up for the above shortcomings. It is presented that, ontologically, technological innovations drive and lie in the existential cycle of the human beings. Technological innovations stem from the rupture of the lifeworld and the "circular economy". With such ruptures, all the basic elements of the social practices, i.e. the human, the natural, the institutional and the technological, would be brought to light, and the innovation is the very process of transactions among various stakeholders who dispute each other and try to resolve these disputes. In this process, through a series of mangles and experiments, technological problems will be firstly defined, then gradually be resolved, eventually a new collection of human and nonhuman formed and the lifeworld renewed. Therefore, if the technology is the root of human existence, the technological innovations, based on the related traditions as the ready-made things, is the dangerously explorative journey to create new possibilities for human existence. The essence of human existence lies in technology (being) as well as in innovations (becoming), especially technological innovations. To break the solidification of society, the fundamental means is to conduct technological innovations. So it is absurd to simply deny technologies and innovations and the only choice is to do technological innovations proactively and responsibly. Responsible innovation means not so much letting more prescient and responsible people into the innovation process to supervise the less prescient and irresponsible innovators, but rather to help broaden the innovation vision and to share such increasingly weighty responsibilities through public participation and equal dialogue.

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PROOF SYSTEMS FOR VARIOUS FDE-BASED MODAL LOGICS

Session 30E

Congress section(s): A2

We present novel proof systems for various FDE-based modal logics. Among the systems considered are a number of Belnapian modal logics introduced in [2] and [3], as well as the modal logic KN4 with strong implication introduced in [1]. In particular, we provide a Hilbert-style axiom system for the logic $BK^{\Box}$ and characterize the logic BK as an axiomatic

extension of the system BK^{FS}. For KN4 we provide both an FDE-style axiom system and a decidable sequent calculus for which a contraction elimination and a cut elimination result are shown.

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BRIDGING BETWEEN BIOLOGY AND LAW: EUROPEAN GMO LAW AS A CASE FOR APPLIED PHILOSOPHY OF SCIENCE

Session 14H

Congress section(s): C3

Laws regulating the permissibility of producing and releasing genetically modified organisms (GMOs) into the environment address a multitude of normatively loaded issues and frequently lead to heated public debate. Drafting new legislature as well as interpreting and operationalizing current GMO law draws on knowledge from both (applied) biology and the study of law.

The European Directive 2001/18/EC regulates the deliberate release of GMOs, such as genetically modified crops in agriculture. Its legal definition of GMO depends on the interpretation of the vaguely formulated phrase “altered in a way that does not occur naturally” (Bobek, 2018). However, this phrase decides which organisms do or do not fall under the regulatory obligations of European GMO law, with far reaching implications for what is planted on our fields and served on our plates.

I provide a framework for interpreting the European GMO definition on an outcome-based approach, by identifying two main issues that challenge its straightforward application to organisms bread by new breeding techniques:

(1) First, three contradicting concepts of naturalness can be distinguished (following Siipi, 2008; Siipi & Ahteensuu, 2016) and the decision between those is necessarily based on values.

(2) Second, a theory of biological modalities is required for the operationalization of natural possibilities (following Huber, 2017).

Once these conceptual issues are solved the GMO definition can be operationalized for regulatory practice.

This case study on the GMO Definition in European law shows how history and philosophy of science can contribute to bridging across the disciplines: Note that legal methods alone do not suffice to interpret the GMO definition in the context of new technologies, because there are no legal precedents and no comparable instances in the legal body in the case of (radically) new scientific developments. For this reason, lawyers call on experts from biology and biotechnology to draw on scientific ontologies, emphasizing the importance of science for policymaking (cf. Douglas, 2009). On the other hand, also methods from biology alone do not suffice to operationalize the GMO definition, since ontological choices do not only depend on empirical evidence but also on value judgments (Ludwig 2014, 2016). Instead, HPS is the go-to-discipline for the clarification of conceptual issues in multidisciplinary normative contexts.

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MODELS OF TRUTH THEORIES

Session 27I

Congress section(s): A1

The commitments of a theory can be roughly understood as what the theory ‚really says.“ In our research, we are specifically interested in truth theories. There are several ways found in literature to define the situation in which one theory of truth Th_1 ‚says more“ than another theory Th_2. In particular, requiring that Th_2 is simply contained in Th_1 (i.e., proves no more theorems than Th_1) may often be viewed as an overly crude measure.

In our talk, we will discuss the notion of the semantic strength of truth theories. Let Th_1, Th_2 be two theories of truth over Peano Arithmetic PA (i.e., theories obtained by adding to PA a unary predicate T(x) which is assumed to satisfy some axioms which are likely to hold for the truth predicate, like compositionality). We say that Th_1 is semantically stronger than Th_2 if for every model M of PA if there exists an expansion (M,T) satisfying Th_1, then there also exists an expansion (M,T‘) satisfying Th_2. In other words, if in a model M we can distinguish a set of sentences which satisfies the axioms of Th_1, then there is a set of sentences (possibly a different one) satisfying the axioms of Th_2.

There are some obvious links between semantic strength and other methods of comparing truth theories. First of all, if in Th_1 we can define some other truth predicate satisfying the axioms Th_2 leaving definitions of arithmetical operations unchanged (in which case we say that Th_2 is relatively definable in Th_1), then Th_1 is semantically stronger than Th_2. Furthermore, if Th_1 is semantically stronger than Th_2, then every consequence of Th_2 in the language of arithmetic (or, in general, in the language of the base theory), can also be proved in Th_1 (so Th_1 is stronger than Th_2 in the sense of arithmetical consequences).

In our research, we tried to fine-tune the comparison of strength between theories of the same arithmetical consequences by investigating their models. It turns out that all known natural theories whose semantic strength we managed to understand are actually comparable with one another, even if it is not initially clear from the description of the theory.

Probably the most striking example of this comparability phenomenon is that CT^-, the theory of purely compositional truth without induction for the formulae containing the truth predicate is semantically stronger than UTB, a theory of Tarski biconditionals for arithmetical formulae with full induction for the truth predicate.

Another aspect of philosophical interest are theories of truth which are semantically conservative. A theory Th is semantically conservative over its base theory (in our case, PA) if for every model M of PA, we can find a subset T of its domain such that (M,T) satisfies Th. This means that the theory Th does not put any restrictions whatsoever on the shape and structural properties of the underlying model of the base theory. It does not ‚say“ more than the base theory in the sense of semantic strength. We will discuss some examples of well-behaved theories which are semantically conservative over PA as well as some striking classical non-examples, including CT^-.

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PEGGING LEVELS

Session 7K

Congress section(s): C7

Any serious discussion of policy—say of further and higher education—is unlikely to progress far without someone asking, ‘What do you mean by...?’ or saying, ‘We need some definitions for our report’. Indeed, without some degree of agreement about what an item under discussion is, the discussion is unlikely to be productive. It will miss issues—or fudge them. Educational policy and organisation necessarily utilises ‘levels’—higher education is ‘higher’ than further education, level 4 is ‘higher’ than level 3, etc. But there are no litmus tests of levels. Instead, open-textured language is used to ‘define’ them. Sometimes that language reflects a consensus amongst those who write or speak the words and those who read or hear them. Sometimes the words obscure deep disagreements. Sometimes disagreements emerge over time as social changes erode or shake the previous consensus.

Failure to appreciate such difficulties undermines the utility of much educational debate whether amongst policy-makers or those who teach. Too often will participants 'talk past each other'. Too often will they fail to identify and engage with the real issues.
In this context, the paper will explore the following:
1. the problems of once-and-for-all definition of evaluative terms, using examples from the work of W B Gallie, J L Austin, Ludwig Wittgenstein, Ronald Dworkin, Daniel Dennett, David Miller
2. the Humean insight that reason is ever 'the slave of the passions' and modern developments in neuro-psychology and neuro-philosophy that adopt a two-stage approach to the process of evaluation—distinguishing the rapid and automatic from reflective (and often ex post justificatory) reasoning (sources here include the work of Jonathan Haidt. Thomas Scanlon and Daniel Kahneman)
3. the notion of 'lexical priming' through shared experience
4. the distinction between words that are necessarily clear (e.g. triangle) and words that are more open-textured (e.g. triangular, reasonable, analytical
5. the UK 'official' definitions of levels
6. the notion of 'vocationality' as an indicator of that which is merely 'further' rather than 'higher'
7. Bloom's taxonomy, 'soft' and 'hard' skills
8. learning by doing"

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A DEFENCE OF PLURALISM OF CAUSALITY

Session 7D

Congress section(s): B2, B4

The purpose of this talk is to defend a pluralism of causality. It will be expected that for natural sciences and humanities different types of causal relations are appropriate. However it can be shown that even inside physics different causal relations are necessary. A model is presented which distinguishes five types of causal relations.
The model distinguishes: Causal factors which are neither necessary nor sufficient; three sufficient causes and a necessary cause. All causal relations are irreflexive, asymmetric or not-symmetric, and the cause is earlier than the effect.
Causal Factors (CF): Amazon has hundreds of tributary rivers. A change of the temperature of one of them is a causal factor for the temperature of Amazon at its fall into the Atlantic. CF is not transitive, continuous or discrete.
Sufficient Cause 1 (CS1): This causal relation is not symmetric and not transitive. Such situations occur when described by the General Gas-Law, i.e. in thermo-dynamical cases on the phenomenological and descriptive macro-level.
Sufficient Cause 2 (CS2): According to Classical Mechanics and Special Relativity the causal relation there is asymmetric, transitive and continuous. This is a second important type of sufficient cause (CS2) which happens in dependencies of events described by dynamical laws.
Sufficient Cause 3 (CS3): In a quantum jump the sufficient cause is not transitive. An energy-input by a photon (A) is capable to change electron's position to an excited state (B) and event B is capable to emit a photon by jumping back (C). But A is not capable to produce C. Moreover the causal relation is asymmetric.
Necessary Cause (CN): The events of the past light cone (Special Relativity) are necessary causes for those of the future light cone. The earlies members of an ancestry-tree are necessary causes for the later members. CN is asymmetric and transitive.
On the metalevel of scientific causal explanation laws of nature (dynamical or statistical) are necessary "causes" for the Explanandum.
Theories of causality having only one type of causal relation (for example CS2) as Dowe (2000) and Salmon (1984, 1994) cannot be accepted as the examples for CF, CS1 and CS3 above show.
The basic logic RMQ (Weingartner 2009) for the causal relations is a 6-valued decidable propositional logic (3 values for truth, 3 values for falsity). It has two concepts of validity, material and strict where all theorems of classical two-valued propositional logic are at least materially valid. The strictly valid theorems of RMQ meet criteria of relevance. RMQ includes a modal system with 14 modalities. The proposed model RMQC is a logic of five causal relations which is an extension of RMQ by adding the five 2-place operations CF, CN, CS1, CS2, CS3 between propositional variables representing states of affairs or events.
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IDEALS, IDEALIZATION, AND A HYBRID CONCEPT OF ENTAILMENT RELATION

Session 27C

Congress section(s): A1

The inescapable necessity of higher-type ideal objects which more often than not are "brought into being" by one of the infamously elegant combinations of classical logic and maximality (granted by principles as the ones going back to Kuratowski and Zorn), is, it may justly be argued, a self-fulfilling prophecy. Present-day classical mathematics thus finds itself at times clouded by strong ontological commitments. But what is at stake here is pretense, and techniques as multifarious as the ideal objects they are meant to eliminate have long borne witness to the fact that unveiling computational content is all but a futile endeavor.
Abstract entailment relations have come to play an important role, most notably the ones introduced by Scott [6] which subsequently have been brought into action in commutative algebra and lattice theory by Cederquist and Coquand [3]. The utter versatility of entailment relations notwithstanding, some potential applications, e.g., with regard to injectivity criteria like Baer's, seem to call for yet another concept that allows for arbitrary sets of succedents (rather than the usual finite ones), but maintains the conventional concept's simplicity.
In this talk, we discuss a possible development according to which an entailment relation be un- derstood (within Aczel's constructive set theory) as a class relation between finite and arbitrary subsets of the underlying set, the governing rules for which, e.g., transitivity, to be suitably adjusted. At the heart of our approach we find van den Berg's finitary non-deterministic inductive definitions [2], on top of which we consider inference steps so as to give account of the inductive generation procedure and cut elimination [5]. Carrying over the strategy of Coquand and Zhang [4] to our setting, we associate set-generated frames [1] to inductively generated entailment relations, and relate completeness of the latter with the former's having enough points.
Once the foundational issues have been cleared, it remains to give evidence why all this might be a road worth taking in the first place, and we will do so by sketching several case studies, thereby revisiting the "extension-as-conservation" maxim, which in the past successfully guided the quest for constructivization in order theory, point-free topology, and algebra.
The intended practical purpose will at least be twofold: infinitary entailment relations might com- plement the approach taken in dynamical algebra, and, sharing aims, may ultimately contribute to the revised Hilbert programme in abstract algebra.
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ON ENGINEERING DESIGN. A PHILOSOPHICAL INQUIRY

Session 16D

Congress section(s): C8

In my work, I would like to explore the area of engineering design through the philosopher's glass. First, looking at the whole process of engineering design - as described by Pahl and Beitz [4] - as a perfect combination of ancient techne and episteme [7], what is an art or craft and what is theoretical knowledge. In this part, I will try to build a bridge over Aristotelian thought and contemporary discourse in engineering design. Second, focusing on the so-called conceptual and embodiment design I would like to explore the notion of representation. In particular, I would like to use the work of Roman Ingarden on the works of art and use his analysis to name and interpret elements of, what engineers often call, 3D models or 3D representations. Third, I would like to recognize the usefulness of the idea of 'directions of fit'[1,6] in the area of manufacturing, like it is presented in [2], and try to apply this idea to the area of Computer-Aided Design.
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'TAXONOMIC FREEDOM' AND REFERENTIAL PRACTICE IN BIOLOGICAL TAXONOMY

Session 20F

Congress section(s): C3

Taxonomic names serve vital epistemic and communicative roles as identifiers for taxonomic groups. Yet most taxonomic names are poor vehicles for communicating taxonomic content – names don't wear their meaning on their sleeves. For names to have meaning, they must be associated with fallible and subjective judgments about the proper circumscription of the taxonomic groups they refer to. So while the practices of naming and taxonomizing are principally distinct, they closely depend on each other. With a twist on Kant's famous dictum, one could say that "nomenclature without taxonomy is empty; taxonomy without nomenclature is blind."
This clear distinction – yet intimate connection – between nomenclature and taxonomy is enshrined in the major codes of taxonomic nomenclature as the principle of taxonomic freedom: the codes only govern the application of names, they don't lay down principles for taxonomic practice. What counts as a 'good' taxonomic judgment is not codified but is left to taxonomic science and the individual taxonomist. In recent years, the principle of taxonomic freedom has come under attack from various side in scientific journals, on taxonomic mailing lists, and in the popular press. It has been argued that in contemporary taxonomic practice, taxonomic freedom all too often results in taxonomic free-for-all. Names based on poor-quality taxonomic hypotheses make their way into databases and end up misleading taxonomists and non-taxonomic end users, including policy makers on conservation efforts. This raises the question whether taxonomic freedom should continue to be a central principle, or whether it should be restricted to allow for additional quality assurance mechanisms on taxonomic judgment. Or would this be fundamentally unscientific and do more harm than good to taxonomy? In this talk, I will provide a closer philosophical look at recent arguments for and against the principle of taxonomic freedom.

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CAN WE ADD KAPPA-DOMINATING REALS WITHOUT ADDING KAPPA-COHEN REALS?

Session 26B

Congress section(s): A1

I will discuss the question under which circumstances forcings which add a kappa-dominating real (i.e., an element of the generalized Baire space kappa^kappa that is eventually above all ground model elements) also add a kappa-Cohen real. Using an infinite game of length (only) omega, we show that this is indeed the case for a large class of forcing notions, among them all Laver type forcings on kappa^(<kappa). Moreover, this class contains all tree-like forcings which add a kappa-dominating real as their generic branch. However, it remains unclear whether all "nice" (e.g., <kappa-closed, or <kappa-distributive) forcings belong to this class, or whether there exists a counterexample, maybe under very strong large cardinal assumptions.
In any case, the results show that the situation on kappa is very different from the classical setting on omega where it is easy to add (omega-)dominating reals without adding (omega-)Cohen reals, e.g., by ordinary Laver forcing, or by ordinary Mathias forcing, etc.

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PROBABILISTIC AGENT-DEPENDENT OUGHTS

Session 10L

Congress section(s): A2

In this paper we extend the probabilistic STIT logic of [1] in order to develop a logic of obligation which accounts for an agent's beliefs about what others will do and what the agent him- or herself can achieve. The account is notable in that it incorporates harms into a probabilistic STIT framework, an endeavor which to our knowledge has not been done before.
One of the main aims of the paper is to develop an account of obligation which is decision-theoretic [2] (i.e. concerned with how an agent goes about trying to act for the best, and not with what really is objectively best), agent-dependent (i.e. based on the agent's limited information, reasoning abilities, and actual alternatives), and action-guiding (i.e. provides a unique conclusion to the question of what the agent ought to do).
We begin by presenting the probabilistic XSTITp logic developed in [1]. In doing so we highlight that the XSTITp logic is concerned with subjective probabilities, with what some particular agent believes is likely to happen. This is crucial for our purposes, as our aim is to model obligations from the decision-theoretic agent-dependent perspective, and therefore it is necessary that the probabilities being employed be those assigned by the agent whose choice we are examining, and not

some god-like omnipotent observer. With Broersen's framework for the probabilistic XSTITp in place, we then move to our method of modeling expected harm.
We simplify our discussion by conceiving of "expected harm" as nothing more than the expected number of some concrete and discrete harms caused by some action. (More specifically, we conceive of "expected harm" as the expected number of deaths a certain action will bring about, as we aim to apply our account to the ethics of war and conflict.) This allows us to assign clear values to "harm", while also avoiding the more subtle debates about comparisons between different types of harm or between individuals. Moreover, in many areas of ethical concern (and most especially in the ethics of war and conflict), there is some discrete and concrete harm (usually number of deaths) that is generally taken to be lexically prior to all others, and is often used as a proxy for evaluating the total harm caused by some action. With our discussion limited to concrete and discrete harms, we then present a function for mapping future harms at some particular state to a notion of expected harm which relies on both the choice of the agent in question and the expected choices of all other agents within the model. This function allows one to formally model how an agent's expectations about his and others' choices will impact on his beliefs concerning the expected harms of the actions.
With our method of calculating expected harm in place, we then move onto developing a ranking of actions given some goal G. We begin by presenting Broersen's "chance of success function", where the chance of success that some G is realized is the sum of the chances the agent assigns to next possible static states validating G. Using this, and given some goal G, we can then define a deontic ranking of actions where one action is preferred to another just in case the chances of success for the former are at least as high as those of the latter (with respect to G) and the former has a lower expected harm than the latter. Put simply, one action is preferable to another if it will be at least as likely to be successful in securing our goal and will not cause more harm than the alternatives. From this ranking we then define the notion of optimality, which maintains that an action is optimal just in case there is no other alternative which has an equal or higher chance of success and is less harmful. We conclude by showing that this notion of optimality tracks to core obligations from, most notably, the ethics of war and conflict (via the principle of necessity), but also to other ethical domains as well. Moreover, the simplification we make regarding how "harm" is to be understood allows one to easily employ this overall framework to other domains of ethical inquiry where there is some discrete outcome that is taken to be morally central.
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SETTING LIMITS TO CHANG'S PLURALISM

Session 5J

Congress section(s): B1, B6

Hasok Chang has raised some serious questions about the extent to which there needs to be consensus in science. At its core, Chang's project is normative. He argues that there are many benefits to pluralism. I argue that Chang's pluralism overlooks the importance of consensus in science. I advance an account of consensus in science that will aid us in better understanding the constructive roles and limitations of pluralism in science.
Chang argues that progress in science has been undermined and developments have been delayed because scientists have frequently been too quick to abandon a theory for a new competitor theory (see Chang 2012). Chang illustrates this with a number of case studies, including what he regards as the too-hasty abandonment of the phlogiston theory in the late 18th Century. He maintains that some insights contained in the phlogiston theory needed to be rediscovered years later.
Chang argues that we need to foster pluralism in science. The type of pluralism that Chang defends is broader than theoretical pluralism. Chang is concerned with what he calls "systems of practice" (Chang 2012, 253). He presents two families of arguments in defence of his pluralism: arguments that appeal to the benefits of tolerance, and arguments that appeal to the benefits of interaction between competing theories or scientific practices. Chang explicitly relates his defense of pluralism to Thomas Kuhn's endorsement of theoretical monism (see Chang 2012, 258).

I argue that Kuhn's account of science allows for a certain degree of pluralism. Kuhn came to argue that the locus of consensus in a specialty community is the scientific lexicon that scientists employ in their research. A scientific speciality community is characterized by its use of a scientific vocabulary that those working in the specialty employ in their research. Kuhn was quite insistent that the scientists in a specialty must share a lexicon. The lexicon is what makes effective communication possible.
Such a consensus, though, is compatible with significant differences between the scientists working in a specialty. In fact, such differences play an integral part in Kuhn's account of scientific change. Kuhn was insistent that exemplars are open to multiple interpretations. It is their flexibility, their ability to be applied in a variety of different ways to different problems, which make them so useful to scientists in their normal research activities. Second, Kuhn argued that the values that scientists appeal to when they are choosing between competing theories are also open to multiple interpretations (Kuhn 1977). Again, this flexibility was crucial, according to Kuhn. It was the key to understanding the success of science.
I argue that lexical monism is compatible with Chang's pluralism of systems of practice.
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COUNT-AS CONDITIONALS, BACKGROUND CONDITIONS AND HIERARCHY OF CONSTITUTIVE RULES

Session 8H

Congress section(s): C7

Searle's theory of institutional facts seems to be of vital importance in understanding social reality. According to his summary, the following "rather simple set of equivalences and logical implications" hold (2010, p.23):
institutional facts = status function → deontic powers → desire-independent reason for action.
Status functions are functions people, things, processes, and so on have, not in virtue of their "sheer physical features" but in virtue of certain status they are recognized to have, and deontic powers are things like "rights, duties, obligations, requirements, permissions, authorizations, entitlements, and so on" (2010, p. 9). In simple cases, institutional facts are generated in accordance to the constitutive rules of the following form (Searle 1969, pp. 51-52, 1995, pp. 28, 41-51):
X counts as Y in context C,
where the term Y stands for the relevant status which the status function accompanies.
Recently several attempts to capture the logic of count-as conditionals have been made in the deontic logic literature. For example, Jones and Sergot (1996) includes the following principle as one of the logical principles for the logic of count-as (Jones and Sergot, 1996, p. 436).
$(A \Rightarrow_s B) \rightarrow ((B \Rightarrow_s C) \rightarrow (A \Rightarrow_s C))$,
where the expression "$\Rightarrow_s$" is used to represent the special kind of conditional such that the sentence "$A \Rightarrow_s B$" intuitively means that A counts as B in the institution s.
The importance of having such a logic is of course clear, but if we are to analyze the logic of social reality, it seems that we need to go further for at least two reasons. First, even if an entity of type X counts as Y in context C, another entity of type X may fail to do so in other contexts. Thus, we need to be able to talk about background conditions that characterize C. Second, constitutive rules can be hierarchically structured, and so an entity e of type X which counts as Y in a context c of type C may further count as Z in that context if any entity of type Y counts as Z in context D and c is of type D as well. The Purpose of this paper is to show how the logic of such phenomena can be captured in Channel Theory developed in Barwise and Seligman (1997).
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MULTI-MODAL MU-CALCULUS WITH POSTFIX MODAL OPERATOR ABSTRACTING ACTIONS

Session 26I

Congress section(s): A3, C1, C6

With respect to Hennessy-Milner Logic ([2]) classical for abstract state concepts, taking the meaning of a formula as a state set, my talk in CLMPS 2015 ([3]) was concerned with the denotation of actions where the actions (as functions) are implemented at the state and state transitions are affected by the applied actions. The purpose was to look for the meanings of actions accompanied with state transitions.
In my works (Logic Colloquium 2016 and 2017), multi-modal mu-calculus is formulated, where a prefix modality for communication, a postfix modality for terms or propositions (as action constructions of functional or logic programming), a negation denoting incapability of interaction (virtually with human), truth and propositions, the classical negation, and a least fixed point operator “mu” may organize formulas (denoting state sets in the transition system of the calculus, extending modal logic interpretations). It is for a representation of human-machine interaction that a postfix modal operator, as well as the prefix modal one, is defined with consistency to fixed point operator (mu-operator). Intuitively, the communication (from human) is to be demonstrated in prefix modality, while the action (of corresponding machinery) is to be represented in postfix modality. As a next step to abstracting actions, the postfix operator is to contain a class of propositional formulas (whose form is a “set” containing a conjunction of literals preceding both an implication and a succeeding literal). The form may be sometimes of use in programming. Its model is closely related to refutation procedures as program implementations even in “3-valued logic”.
In this talk, from an algebraic view, Heyting algebra expressions of the three elements (from 3-valued domain of the bottom, medium and top) are re-examined to model the propositional formulas of the above form. As regards the postfix modality for actions (which are to be programmed), algebraic and constructive views are necessary into its abstraction: The sequence of actions includes intelligence and the selection is relevant to judgement, such that a concatenation for the sequence, and an alternation for the selection may be essential in state machinery with respect to an algebraic structure “semiring”. It suggests that algebraic views on postfix modality in the calculus are to be made clearer.
However, it conceives difficulty in the sense that the associated mapping with the above set (the one containing conjunctions of literals where each conjunction is followed by an implication and a literal) might not be always monotonic and thus fixed point denotation of the associated mapping is not so easily obtained. The usage of van Gelder et al. ([1]) may be effective, not only classically but also for any given expression set of this case that the interpretation of negation is not Boolean. Then actions in postfix modal operator are modelled into the transition system of the multi-modal mu-calculus (as an extension of modal mu-calculus), where the evaluation of algebraic expressions regarding postfix modality may be the key problem.
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WHAT IS THE COMMON CONCEPTUAL BASIS OF GÖDEL EMBEDDING AND GIRARD EMBEDDING?

Session 31C

Congress section(s): C1

It is well-known that via Gödel translation the theorems of intuitionistic logic (IL) can be soundly and faithfully embedded into classical modal logic CS4: the formulae of IL are provable iff their translations are provable in CS4. Recently, several authors have elucidated much more specific details of the embedding by adopting various new systems of sequent calculus. Employing a system of G3-style multi-succedent sequent calculus for IL and Troelstra and Schwichtenberg’s G3-style sequent calculus for CS4 [1], I constructed another proof-theoretic proof of the embedding.
On the other hand, it is also well-known that there is another very interesting embedding of IL, i.e. Girard’s embedding of IL into classical linear logic (CLL). I invented a new translation (different from that of Girard) of the IL formulae into the CLL formulae and by adopting the same multi-succedent sequent calculus for IL as mentioned above, proved the soundness and faithfulness of this translation. Girard’s translation interprets Conjunction and Disjunction as additive and Implication as multiplicative, whereas our translation interprets all of them as multiplicative. In interpreting sequents, Girard attached to the all the formulae occurring in the antecedent of a sequent the modal operator !, while in our translation, although we similarly attach ! to the all the formulae occurring in the antecedent of a sequent, we attach ? to those in the succedent of a sequent (note that we employ the same multi-succedent sequent calculus for IL). Our translation, one might say, is more coherent in that the connectives employed in the translation are all multiplicative. It shows that the additive aspects of IL connectives can be represented by multiplicative connectives together with ! and ? (i.e. Weakening and Contraction).
Let us compare Gödel embedding and Girard embedding (and also our embedding) more closely. In the former, Box dominated by S4 axioms plays a central role, while in the latter (also in our embedding) ! do that (note that in our embedding also ? dose an important role). From a proof-theoretical view point ! behaves completely similarly with S4 Box (except that ! is also dominated by Weakening and Contraction). Besides Weakening and Contraction, why does ! obey the same inference rules as S4 Box?
In this presentation, we will take up this question and attempt to clarify the common conceptual basis sherd by Gödel Embedding and Girard Embedding. Among philosophers of logic, it seems to be generally regarded that the content (i.e. meaning) of a formula occurring in a sequent is different between LL and IL. While in the latter, it is nothing but a ordinary constructivist proposition (e.g. what a mathematician attempts to prove constructively), in the former it is a certain data token of some data type represented by the formula. By clarifying the common basis mentioned above we hope that we can make their relationship more intelligible.
[1] A. S. Troelstra and H. Schwichtenberg. Basic Proof Theory. Cambridge University Press. Second edition. 2000.

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UNDERSTANDING CAUSAL REASONING IN NEUROPHYSIOLOGY

Session 28G

Congress section(s): C3

Neuroscientists value the research that provides some causal understanding of the targeted system. In order to achieve that, they perform causal reasoning, a reasoning type of activity that aims at producing and/or evaluating causal claims about their targeted system. When they perform their causal reasoning within a specific context, they need to employ some standards to guide and justify their causal reasoning. But what are these standards? How we as philosophers analyze and evaluate them?
The questions get more complicated when you take the evolution and heterogeneity of neuroscientific practice into consideration. First, structures and standards for good experimental paradigms are co-evolving with technological innovation. For example, in neurophysiology, after the invention of the technique that allows geneticists to genetically

modify neurons to express light-sensitive ion channels, a good experimental paradigm in neurophysiology often involves the component of using this genetical technique. Second, it is common in current neuroscience that, for a given set of experiments, it might combine various types of techniques from different areas. Each set of techniques brings in different methodological standards that may or may not be relevant to the success of causal reasoning.

These evolving and heterogeneous aspects of neuroscientific practice pose a particular challenge to the philosophers who aim to provide a normative framework in order to understand how causal reasoning in neuroscience works. We need a normative framework that accommodates these evolving and heterogeneous aspects. One way to meet the challenge is to reduce or subsume the heterogeneous practices under a single category/concept of mechanistic causal explanation that is flexible enough to accommodate the heterogeneity (Craver, 2007; Craver and Darden, 2013). Another way to meet the challenge is to adopt the framework of modest scientific pluralism on causal pattern explanations (Potochnik, 2017).

In this paper, I will first present a case study from neurophysiology. I will use the following methodology to analyze the case study: (1) delve into the details of the heterogeneous practices, (2) identify instances of causal reasoning, (3) analyze what the relevant standards are in use, (4) perform some literature analysis to assess how the community of neurophysiologists, in fact, evaluate the relevant standards. I will then apply both Craver's framework and Potochnik's normative framework to the case study. I aim to adjudicate which framework provides better conceptual tools for evaluating the success of the identified instances of causal reasoning. I will conclude that Potochnik's framework does a better job than Craver's one with respect to the case study from neurophysiology.

To that end, the paper will proceed as follows. In Section 2, I will present a case study to identify three instances of causal reasoning in neurophysiology and the relevant standards in use. In Section 3, I will argue that the conceptual tools from Craver's framework are insufficient to complete the evaluative task. In Section 4, I will show that the conceptual tools from Potochnik's framework adequately assist the evaluative task and help generate a better philosophical understanding of the evolving and heterogeneous aspects of causal reasoning in neurophysiology.

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UNDERDETERMINATION AND EMPIRICAL EQUIVALENCE: THE STANDARD INTERPRETATION AND BOHMIAN MECHANICS

Session 17G

Congress section(s): B4

The problem of underdetermination in general is that if two or more different theories are empirically equivalent, then we have no empirical reason to believe one but not the other. I define empirical equivalence as two or more theories or formulations are empirically equivalent with respect to all possible data (either predictive or non-predictive), within or outside its own domain of application. I take empirical equivalence to be a broader notion than predictive equivalence. An underdetermination is also seemingly present between the standard interpretation of quantum mechanics, the standard interpretation and a non-local hidden variables theory, Bohmian mechanics. They make the same predictions for cases such as statistical correlations in EPR-type experiments, interference in a double-slit experiment, quantum tunneling, etc. But the two are contrary theories because they differ in important ontological aspects. For example, the standard interpretation obeys indeterministic laws and claims a nonexistence of particle trajectories, while Bohmian mechanics obeys deterministic laws and claims an existence of particle positions and trajectories (Cushing, 1994, p. 203)[1]. It is important to note that quantum phenomena do not directly tell us what the world is like, so we need to give our interpretation of it. The two theories are commonly referred to as interpretations of quantum mechanics, but they are in fact two different theories. A detailed discussion on this particular case of Bohmian mechanics and the standard interpretation provides new understanding to the underdetermination problem that the scientific realist faces. Unlike it is often suggested that the two theories are empirically equivalent (and if they are, we will face a problem of underdetermination), I deny that belief and my argument is three-folded. (a) The two theories are not predictively equivalent when we restrict our discussion to the domain of non-relativistic quantum mechanics, where Bohmian mechanics makes predictions about particles having trajectories and has gained empirical support for it [2]. (b) The predictions for domains outside non-relativistic quantum physics are relevant empirical evidence if they flow from the fundamental part of a quantum theory and those predictions may be also confirmed or disconfirmed by theories or empirical findings in other domains. The two theories, Bohmian mechanics and the standard interpretation are not empirically equivalent when we consider their implications for the relativistic domain. (c) I also argue that there is non-predictive evidence that provides empirical support for Bohmian mechanics but not for the standard interpretation. A coherence with empirical data about macroscopic systems, such as a walker system that consists of an oil droplet and the wave it creates exhibits quantum-like phenomena and resembles the Bohmian wave-particle model, gives an empirical reason to favor Bohmian mechanics [3]. This empirical support cannot be accommodated in a theory to be entailed by it.

Footnote:

[1] See Cushing, J. T. (1994) "Quantum mechanics: historical contingency and the Copenhagen hegemony".

[2] See Kocsis, S., Braverman, B., Ravets, S., Stevens, M. J., Mirin, R. P., Shalm, L. K., & Steinberg, A. M. (2011) "Observing ∃the average trajectories of single photons in a two-slit interferometer".

[3] See Faria, L. M. (2017) "A model for Faraday pilot waves over variable topography".

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HIGHER-ORDER IDENTITY IN THE NECESSITISM-CONTINGENTISM DEBATE IN HIGHER-ORDER MODAL LOGIC

Session 24L

Congress section(s): A2

Timothy Williamson (2013) defends necessitism, the thesis that everything exists necessarily—(NE) □∀x□∃yx = y, which is valid in the models with constant domains for standard quantified modal logic S5 (QML-S5). Still, several contingentist's quantificational modal logics with variable domains (CQML) have been constructed, including Yang's (2012) universally free modal logic (CQML-Y), taking as the underlying system a free logic with specified syntactic stipulations on the occurrences of names and free variables in de re contexts. If we introduce a new predicate 'Cx'('x is chunky', meaning that the object x is either abstract or concrete) and add to QML-S5 two axioms (i) ∀x◇Cx (the 'ontic-chunky constraint'), and (ii)∀x□(Fx → Cx) (the 'predicative-chunky constraint'), to get a necessitist's quantificational modal logic (NQML), a mapping between CQML-Y and NQML can be then established in that a sentence sN is a theorem in NQML if and only if its translation sc is a theorem of CQML-Y. Thus, quantified modal logic has no authority to decide between necessitism and contingentism.

Williamson further argues that higher-order modal logic favours necessitism over contingentism by comparing both in terms of their interaction with plausible candidate comprehension principles. In particular, an unrestricted modal comprehension principle would generate the higher-order analogues of the thesis for necessitism, (NE2) □∀F□∃G(F ≈ G), where ≈ abbreviates some higher-order analogue of the identity predicate of first order. Intuitively (NE2) involves higher-order identity of first-order predication. But Williamson takes it as an assertion about the necessity of an equivalence relation, characterized in terms of co-extensiveness. An unrestricted modal comprehension principle, (Comp-M)∃X□∀x(Xx ↔ A), then 'creates an awkward logical asymmetry between the first and higher orders for contingentism; typically, contingent objects have non-contingent haecceities'.

Nonetheless, Williamson's defence is unconvincing in two aspects. Firstly, one cannot claim that 'F ≈ G' is equivalent relation, rather than identity, simply because the later may bring in the sort of ontological commitment of higher-order quantification. To be fair to both sides, no more ontological commitment beyond first order should be allowed. Also, Quine's ontology-ideology distinction would no longer work. Williamson's appeal to the so-call metaphysical commitment seems ungrounded. I shall call 'the predicative commitment' the totality of whatever the higher-order variables range over, which would function in a way similar to the assignments for first order variables in Tarski's formal semantics. The second one is concerned with unrestricted comprehension principles. Williamson argues that with these principles, identity holds even for non-chunky objects, which accordingly, have haecceities. But this is unjustified. We may impose a further constraint, namely, ∀X□∀x(Xx → Cx)—a higher-order analogue of the predicative-chunky constraint. Equipped with this, a more promising comprehension principle would be like this: (Comp-MC)∃X□∀x((Xx ∧ Cx) ↔ A)), which claims that there must be some universal property that every chunky object has, and haecceity is one of them, i.e. ∀y□∃X□∀x((Xx ∧ Cx) ↔ x = y). The contingentists would accept this.

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FRAGMENTED AUTHORITARIAN ENVIRONMENTALISM, NATIONALISM, ECOLOGICAL CIVILISATION AND CLIMATE CHANGE IN CHINA

Session 7B

Congress section(s): C2

The complexity and urgency embedded within climate change are appealing for more deliberations in different social, political and philosophical contexts. This paper is going to reflect on how climate change has been constructed under and also shape, what I framed as, fragmented authoritarian environmentalism in China, to argue that more changing social reality need to be included in the discussion of the philosophy of climate change, and to broaden the discussion of green modernity based on political and communication strategy.
In this paper, I demonstrate there has appeared a clearer form of fragmented authoritarianism in environment, rather than a non-participatory totalitarianism in China, through its governance of climate change. The nationalism supported and framed by the central stated in regards to climate change has intensified the ideology of authoritarianism, while the initiative notion of ecological civilisation has reinforced the national interest and at the meantime advanced it beyond, promoting cosmopolitan responsibility for everyone and therefore offer more spaces for different actors and interactions in the whole regime.
Nationalism, or more accurately the aggrieved nationalism, is the main mental root that constructs the Chinese scepticism of climate change. Though be subdued and not as the same scale as in some Western countries, Chinese scepticism of climate change exists, mostly regards climate change as a plot made by the developed world, and has been strongly influenced by the autonomous central authority with its “official” stances and attitudes. On one hand, this government-leading conspiracy theory could be seen as identity politics which simulates and consolidates the social cohesion and trust of the central political authority; on the other hand, it has managed to guide the public opinion to steer by the epistemic distrust to scientific knowledge of climate change which is often seen in troubled international negotiations and interest conflictions. Also, as the scepticism has been largely disappeared from the scene along with China’s stance on the international arena, and the national objective in building an ecological civilisation which includes many human-environment issues has been raised up. This nationalism based political and discourse strategy is shifting people’s perception of distributive justice and restitution of climate change from national dignity, to national soft power and cosmopolitan responsibility. Critically, climate change, along with many other environmental issues, has lowered the entry for more actors beyond the government to influence policy making and implementing, and thus facilities a more fragmented authoritarianism in environment-society relationship. This fragmented authoritarianism framework would supply the space and power for stabilization and change in Chinese context, but also encounter its intrinsic contradiction. In order to examine the prerequisite for building the cosmopolitan solidarity across individual and social boundaries on a global scale to tackle climate challenge, this paper will also make effort in discussing the vulnerability, inequality and maladaptation by looking at cases referring to climate change policy implementation within the political philosophy background of China.

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DEFINITION AND FACULTIES OF LIFE IN MEDIEVAL ISLAMIC PHILOSOPHY

Session 13H

Congress section(s): C3, C4

It has always been problematic to give a concrete definition and identify a principle, when it comes to life, however, unlike the death. Although life itself is the main difference between biological organisms and lifeless (or inanimate?) things. At first sight, it is visible that between the eighteenth and the twentieth centuries there are forty-three books dedicated in discussions on the origin or definition of life, with a quarter of which has been published in this millennium. The increase in these numbers indicate that the debate on solving life puzzle has been popular among scientists and philosophers. How was this situation in medieval times? Did philosophers and physicians in the medieval Islamic world compose books, which give a definition of life and its faculties?
In this study, after giving few definitions of life (and that of death) from recent scientific literature, I will try to go back into the medieval in order to investigate how life and its faculties were considered in a book of kalam from fifteenth century. Composed by Sayyid al Sharif al Jurjani (d. 1413) who was appreciated as an authority in the Ottoman Empire, Sharh al Mawaqif had been copied; read, commented frequently, which shows its popularity among Ottoman philosophers, and theologians. This book is an explanation of al-Mawaqif fi Ilm al-Kalam, written by Adud al-Din al-Idji (d. 1355). I will also pursue by citations from Ibn Sina (d. 1037) -known as Avicenna- through his famous book al-Qanun fi al-Tibb (Canon of Medicine) where he discussed the Vegetal type and Animal type of life through the performance of some actions. As the eminent symbol of Peripatetic Tradition in Islamic Philosophy and Medicine, I will try to compare different considerations on life and death by Kalamic and Philosophic Schools.
Starting from biology, shifting towards kalam and philosophy, I will try to show whether or not we can find philosophical instruments which may inspire us to solve the life puzzle, today.
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REVISITING THE TWO MAJOR STATISTICAL PROBLEMS, STOPPING-RULE AND THE CATCH-ALL HYPOTHESIS, FROM THE VIEWPOINT OF NEO-BAYESIAN STATISTICS.

Session 31F

Congress section(s): C1

Statistics has been not only an indispensable tool for scientific inquiry but also the field of intense controversies among researchers and philosophers (Sober 2008). In the histories of the statistics and the philosophy of statistics, a fundamental conflict between Frequentist (or error statistician) and Bayesian theory had long been the interpretation of probability. These days, however, some Frequentist (or error statistician) does not cling to frequentist interpretation of probability nor Bayesian statistician subjective interpretation adopting frequentist properties to justify procedures (Mayo 2018; Gelman et al. 2013, 2018). It shifts attention from the interpretation of probability to the role and/or objective of the statistic. Here in this presentation, I will review the relationship between two schools of statistics, Frequentist and Bayesian, from the nature of an assessment.
First, I revisit the stopping-rule problem and the catch-all problem, well-known criticism respectively against Frequentism and Bayesianism. In general, any assessment procedure needs both the target and information for making an assessment. While we can update or change the latter, we cannot change the former because it spoils the objective. On this view, the two problems are parallel. For frequentists, the target of assessment is the data: while we evaluate the given data under any

hypothesis, increase in data size post hoc is not permitted. It is the source of the stopping-rule problem. For Bayesianism, the target is the hypothesis: while we evaluate the given hypothesis under any data, changing the set of hypothesis post hoc is not permitted. It is the source of the catch-all problem. Both, the increase in data size and the emergence of novel hypothesis, do happen in the scientific practice and did happen in its history. It means, I argue, any of Frequentism and Bayesianism are not enough for scientific practice.

Second, I consider an apparent solution to the catch-all problem from the perspectives of Bayesian statistics, where Pr(D) is substituted by the marginal likelihood of the model, the likelihood averaged by prior distribution over the parameter space. However, another problem, I argue, emerges since it requires a too strong assumption that we have the true model at hand, rarely the case in scientific practice. Again, the problem is we have no room for changing the set of hypothesis or model.

Third, and finally, I examine the role of Bayesian statistics in practice and point out that it involves assessment of both data and hypothesis as the target of the probe. I show the Bayesian statistics could offer not just a pragmatic eclectic but positive reason to adopt since it avoids the two fundamentally parallel problems.

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UNDERSTANDING METABOLIC REGULATION: A CASE FOR THE FACTIVISTS

Session 11K

Congress section(s): B1

Factive scientific understanding is the thesis that scientific theories and models provide understanding insofar as they are based on facts. Because science heavily relies on various simplifications, it has been argued that the facticity condition is too strong and should be abandoned (Elgin 2007, Potochnik 2015). In this paper I present a general model of a metabolic pathway regulation by feedback inhibition to argue that even highly simplified models that contain various distortions can provide factive understanding. However, there is a number of issues that need to be addressed first. For instance, the core of the disagreement over the facticity condition for understanding revolves around the notion of idealization. Here, I show that the widely used distinction between idealizations and abstractions faces difficulties when applied to the model of a metabolic pathway regulation. Some of the key assumptions involved in the model concern the type of inhibition and the role of concentrations. Contra Love and Nathan (2015) I suggest to view these assumptions as a special sort of abstraction, as vertical abstraction (see also Mäki 1992). Usually, it is the idealizations that are considered problematic for the factivist position because idealizations are thought to introduce distortions into the model, something abstractions do not do. However, I show that here abstractions distort key difference-makers (i.e. type of inhibition and the role of concentration), much like idealizations do elsewhere. This seemingly further supports the nonfactivist view, since if abstractions may involve distortions then not only idealized models but abstract models as well cannot provide factive understanding. I argue that this is not the case here. The diagrammatic model of a metabolic pathway regulation does provide factive understanding insofar as it captures the causal organization of an actual pathway, notwithstanding the distortions. I further motivate my view by drawing an analogy with the way in which Bokulich (2014) presents an alternative view of the notions of how-possibly and how-actually models. The conclusion is that, at least in some instances, highly simplified models which contain key distortions can nevertheless provide factive understanding, provided we correctly specify the locus of truth.

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CHEMICAL REACTIVITY: CAUSALITY OR RECIPROCAL ACTION?

Session 7L

Congress section(s): C2

A chemical reaction is every interaction between molecules, ions or radicals in which chemical bonds are generated or broken, giving rise to new molecules. The initial molecules are called "reactants", and the produced molecules are called "products". Most reactions occur in solution; hence the characteristics of the solvent molecules and their interactions with the other participating molecules should be taken into account.

Chemical reactions are directed towards the production of a higher relative concentration of the most stable species. It is possible to infer which the most stable species is by contrasting the forces of the broken bonds and the formed ones, and the energy associated with them. Reactions are represented by means of energy diagrams or reaction profiles, in which the change of the potential energy during the reaction progress is drawn. The "reaction coordinate" represents the degree of progress in which the reactants become products.

Given the standard representation of reactions, it is often considered that they occur in a causal way. Since chemical reactions processes are traditionally conceived in terms of a causal framework –the relation involved in chemical reactions is understood as causality–, the link between the species is interpreted as successive. Such processes are viewed as if, firstly, the reagents interact and, after a while, they cause the appearance of the products. It is common to find this simplification in the lexicon of chemists as well as in textbooks. But if we intend to address the issue philosophically, wondering about the very nature of the chemical reactions, we can ask for the reasons for maintaining a causal picture beyond the context of teaching and professional practice.

In opposition to the widely accepted causal interpretation, in this paper we will argue that chemical transformations can be more appropriately elucidated within a framework rooted in the category of reciprocal action, inspired in the Kantian notion. While causality is marked by succession, reciprocal action must be interpreted in terms of simultaneity. When the mechanisms involved in chemical reactions are analysed, it is necessary to take into account the interactions with the solvent, the formation of intermediary reactants, the productions of parallel reactions and simultaneous reactions, the formation of dynamic equilibrium, and so on. It is also important to bear in mind that all participants (no matter how low their concentration is) are relevant in the kinetics and thermodynamics associated with the reaction. Our main purpose is to discuss whether the Kantian category of reciprocal action is philosophically fertile to account for the nature of chemical reactions.

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NON-CAUSAL EXPLANATIONS IN QUANTITATIVE LINGUISTICS

Session 18J

Congress section(s): C7

For most linguists, there is a general concurrence that there are only two major plausible explanations in linguistics – formal explanation, typical for generativism and, broadly speaking, functional explanation, typical for not only cognitive linguistics (for current debates see Newmeyer, 2017, Egré, 2015, Haspelmath, 2004). Here, formal explanation is typically derived from internal structure, namely the system, of the grammar of the language, and functional explanation is mainly derived from the external non-linguistic, and so non-systemic, needs of speakers.

Although both of these explanations are considered to be non-causal, only the functional one is of interest for us, because we agree with Givón's criticism (1979) as to the non-predictiveness, and thus mere desriptiveness, of formal explanation. Functional explanation in its more specific form assumes its role in quantitative linguistics (QL, see Köhler 2012 and 1986). In the philosophy of science there is still a lively debate concerning the possibility of the existence of separate non-causal models of explanation (for current debates see Reutlinger, 2018, Kostic, 2016, Huneman, 2010 etc.). Acordingly, it is crucial to study the usefulness of non-causal explanations for conceptualizing explanatory practices in the social sciences and humanities.
QL is a major useful tool for this. Accordingly the goals of this paper are:
First, to uphold the non-causality of functional explanation used in QL.
Second, to use another non-causal type of explanation – topological explanation (see Huneman, 2010) – for QL.
Third, to decide which of these two types of non-causal explanation is more productive for QL.
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EVOLVING THEORIES AND SCIENTIFIC CONTROVERSIES: A CARRIER-TRAIT APPROACH

Session 15H

Congress section(s): B6

It is not without problems to account for the evolution of scientific theories. Reconstruction of theoretical content is typically carried out using static and atemporal conceptual and modelling spaces, but many of the historically important scientific theories are far from being easily delineable entities, and a scientist's theoretical position can respond to new data, literature, and the criticisms received. Especially challenging are scientific controversies, where the debated issues are complex, the exchange involves several participants, and extends over long periods. Famous examples include the Methodenstreit, the Hering-Helmholtz controversy or the debates over Newton's or Darwin's views. In these cases controversies lasted for several generations, and polarisation is a recurring trait of the exchanges. The reconstructions and evaluations of the exchanges also exhibit heterogeneity and polarisation. Cultures of reading, representing, interpreting, and evaluating the theory suggest that some scientific theories are manifolds.
What are the suitable frameworks that help the study of theories, theory-acceptance and the often co-occurring process of opinion-polarization? The talk offers a permissivist carrier-trait framework to study theories, and an artifact-human-artefact knowledge-mobilization process. The theory picked for analysis is Newton's optical theory, a highly successful scientific theory, but one that cannot be easily reduced to equations or formulas, and one that gave rise to opinion polarization. Instead of assuming some type of content (a propositional structure, a conceptual space, or a mathematical object) to reconstruct the theory, and thus provide a paraphrase to stand for the theory, I look at traits that are delineable when studying the carriers of a theory. In a deliberately broad definition, carriers are scientific representations, parts thereof, or composites of them, targets of an interpretation-process. A carrier is an external (non-mental) representation, akin to some speech act, yet it can be a whole book, or just a part of a diagram or sentence. A trait is a distinctive or distinguishable feature, corresponding to some act of making distinctions between carriers.
The reconstruction focuses on innovative aspects (novel traits) of theories that become conventionalized: items introduced to the lexicon (neologisms), some of the mathematical idealizations, and novel diagrammatic traits of the theory. The perspective helps to map strands of uptake (including polarisation of opinions), and trait-analysis can show that multiple readability (ambiguity) of carriers facilitated heterogeneous uptake and the spread of competing views.

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CAUSAL MINIMALITY IN THE BOOLEAN APPROACH TO CAUSAL INFERENCE

Session 3B

Congress section(s): B1

In J.L. Mackie's (1974) influential account of causal regularities, a causal regularity for an effect factor E is a statement expressing that condition C is sufficient and necessary for (the presence or instantiation of) E (relative to a background or causal field), where C is in general a complex Boolean formula involving a number of factors. Without loss of generality, we can put C in disjunctive normal form, a disjunction of conjunctions whose conjuncts express presence or absence of factors. Since C is supposed to be sufficient and necessary for E, each conjunction therein expresses a sufficient condition. Mackie's requirement is that such a sufficient condition should be minimal, in the sense that no conjunction of a proper subset of the conjuncts is sufficient for E. If this requirement is met, then every (positive or negative) factor that appears in the formula is (at least) an INUS condition: an Insufficient but Non-redundant part of an Unnecessary but Sufficient condition for E. Mackie's minimality or non-redundancy requirement has been criticized as too weak (Baumgartner 2008), and a stronger criterion is adopted in some Boolean methods for causal inference, which have found interesting applications in social science (e.g., Ragin and Alexandrovna Sedziaka 2013; Baumgartner and Epple 2014). In addition to minimization of sufficient conditions, the stronger criterion requires that the disjunctive normal form that expresses a necessary condition should be minimally necessary, in the sense that no disjunction of a proper subset of the disjuncts is necessary for the effect. In this talk we identify another criterion of non-redundancy in this setting, which is a counterpart to the causal minimality condition in the framework of causal Bayes nets (Spirtes et al. 1993; Pearl 2000). We show that this criterion is in general even stronger than the two mentioned above. Moreover, we argue that (1) the reasons for strengthening Mackie's criterion of non-redundancy support moving all the way to the criterion we identified, and (2) an argument in the literature against the causal minimality condition for causal systems with determinism also challenges Mackie's criterion of non-redundancy, and an uncompromising response to the argument requires embracing the stronger criterion we identified. Taken together, (1) and (2) suggest that the Boolean approach to causal inference should either abandon its minimality constraint on causal regularities or embrace a stronger one.
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PROCESS, NOT JUST PRODUCT: THE CASE OF NETWORK MOTIFS ANALYSIS

Session 26H

Congress section(s): C3, C9

It has been ubiquitous for scientists to study complex systems by representing these systems as networks and analyzing properties of these networks. Since this network approach allows scientists to focus on all relevant elements in a system and their interactions, in contrast to individual elements in isolation, networks are particularly useful in understanding complex systems, for example, cellular networks studied in systems biology and social networks studies in sociology.
The network approach has drawn attention from philosophers. For example, recent papers have considered the question of how network explanations relate to mechanistic explanations. Some philosophers like Levy and Bechtel (2013) and Craver (2016) view network explanation as a desirable extension of mechanistic explanation, while others like Huneman (2010) and Woodward (2013) reject this view, emphasizing novel features presented in network explanations. However, these discussions are all about the products, namely, explanations generated by the network approach.
I will argue that a focus on explanations is insufficient, especially for understanding how the network approach deals with complex networks. My main example is the network motifs analysis, which has been a focus of some recent discussions (Levy and Bechtel 2013; Craver 2016; Brigandt, Green and O'Malley 2017).
Networks are webs of nodes and edges. When a few-node pattern recurs in different parts of a network much more frequently than expected, this pattern is called a network motif in this network. The general occurrence of network motifs in real networks has been taken to indicate some deep mechanisms governing the evolution of real networks, for example natural selection, and the ultimate task of the network motifs analysis is to reveal these mechanisms.
By analyzing the process of the network motifs analysis, I argue:
1. What lies at the heart of the network motifs analysis is the fact that a pattern's being identified as a network motif depends not only on its own properties, but also on its relationships to the target network and the random networks.
2. This dependence on the target network and random networks is not reflected in the explanations, since these explanations are results of modelling network motif patterns independently of the target networks and the random networks.
As a result, an explicit focus on the process is indispensable for understanding how exactly the network approach deals with complex networks, which cannot be captured by focusing on explanations themselves.
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WHEN ARABIC ALGEBRAIC PROBLEMS MET EUCLIDEAN NORMS IN THE 13TH CENTURY – A CASE STUDY ON THE SCIENTIFIC INNOVATION BY THE TRANSFORMATION IN CROSS-CULTURAL TRANSMISSION

Session 31G

Congress section(s): B3

The transmission of sciences from Arabic world to the Medieval Europe is an established historical fact. The present article is not merely presenting one example of the fact but discussing a way in which the cross-culture transmission of knowledge resulted in the production of new knowledge. The case taken is the reaction of a 13th mathematician, Jordanus de Nemore, to Arabic algebraic problems. In short, he changed them in such a form that we won't see its Arabic relatives unless we summarize his propositions by means of symbolic equations. What was remained and what was lost for the sources after the transmission? My general answer is that only the problems and some solutions, only if understood abstractly, were remained. The form taken to presenting the problems, as well as the demonstrations for them, along with the practical roots and the epistemological foundations of them, were lost. The substitution provided by Jordanus were the Euclidean way of doing mathematics: mathematical knowledges were organized as demonstrated propositions in a deductive system. What's more, Jordanus not only followed this principle, but also modeled Euclid's forms (terms, sentence patterns, structure of propositions, demonstrations). I suggest that the above modifications were an effect of the cultural environment in which Jordanus worked. Although the research on his identity turned to be in vain, by comparison of his works to what were taught and circulated in the universities of the time, we can reasonably assume that he worked in a university environment, where Euclidean mathematics and alike dominated the contents and norms of mathematics. Thus, this is a case that an actor in the adopting culture tried to incorporate an alien knowledge by transforming it according to the dominating norms. Jordan's action did result in an innovation in science. Firstly, the convention of representing numbers by letters was one of the key steps for the development of algebra. More importantly, the analysis method of Greek mathematics enhanced the analytical feature of algebra. However, it seems that this innovative work didn't play its role until several hundred years later, which, in my opinion at least, can be interpreted by the multiplicity of mathematical traditions (also cultures) in the Medieval Europe. But this hypothesis would be discussed in another article.

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MUTUALLY INVERSE IMPLICATION INHERITS FROM AND IMPROVES ON MATERIAL IMPLICATION

Session 17M

Congress section(s): A2

The author constructs mutually-inversistic logic with mutually inverse implication ≤-1 as its core. The truth table for material implication correctly reflects the establishment of A being a sufficient but not necessary condition of B. The truth table for material equivalence correctly reflects the establishment of A being a sufficient and necessary condition of B. A≤-1B denotes A being a sufficient condition of B, its truth table of establishment combines the two truth tables: The first, third, and fourth rows of both truth table are T, F, and T respectively, so the first, third, and fourth rows of the truth table of establishment for ≤-1 are T, F, and T respectively; the two truth tables differ on the second row, so the second row of the truth table of establishment for ≤-1 is n (n denotes "need not determine whether it is true or false").
After an implicational proposition has been established, it can be employed as the major premise to make hypothetical inference. In classical logic, the affirmative expression of hypothetical inference is made in this way: both A materially implying B and A being true is the fourth row of the truth table for material implication, in which B is also true. The author argues that this is incorrect. There is a fundamental principle in philosophy: human cognition is from the known to the unknown. There is a fundamental principle in mathematics: the evaluation of a function is from the arguments to the value, if we want to evaluate from the value to the argument, then we should employ its inverse functions. In order to mathematize human cognition, we let the known be the arguments, let the unknown be the value, so that the human cognition from the known to the unknown become the evaluation of the function from the arguments to the value. The truth table for material implication is a truth function, in which A and B are the known, the arguments, A materially implying B is the unknown, the value, therefore, it can only be employed to establish A materially implying B from A and B. After A materially implying B has been established, it becomes known, becomes the argument. While in the generalized inverse functions of the truth table for material implication, A materially implying B is the known, the argument, therefore, we can employ its generalized inverse functions to make hypothetical inference. Following this clue, the author constructs two generalized inverse functions for the truth table of establishment for ≤-1, one for the affirmative expression of hypothetical inference, the other for the negative expression of hypothetical inference.
Mutually inverse implication is free from implicational paradoxes.

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MUTUAL MISUNDERSTANDING IN SIGNALLING GAMES

Session 18K

Congress section(s): B2

It is a platitude that the relationship between a sign and the meaning it represents is arbitrary and based on convention. While W. V. Quine (1936) argued that this notion implies a vicious circle. Conventions depend on agreements, and, in order to make an agreement, we have to be able to communicate with each other through some kind of primary sign systems. While the emergence of sign systems is the very thing we want to explain.
David Lewis, whose PhD was advised by Quine, challenged Quine's argument in his dissertation (1969). He argued that conventions emerge from social interactions between different agents and formalized the process as signalling games. Signalling systems in which information is communicated from the sender to the receiver in a signalling game are strict Nash equilibria in the game. Then, the problem of the emergence of meaning becomes the problem that how to converge to and maintain strict Nash equilibria in signalling games. The solutions provided by Lewis are common knowledge and salience. However, Brian Skyrms (1996, 2004) argues that Lewis' solution cannot escape Quine's critique. Instead, Skyrms proposes an evolutionary dynamic approach to the problem. The dynamic analysis of signalling games shows that signalling systems spontaneously emerge in the interacts between senders and receivers. Common knowledge and salience are unnecessary.
It is believed by many philosophers that Lewis-Skyrms signalling game theory brings fundamentally new insights to questions concerning with the explanation of meaning. As a result, studies of signalling games are prosperous in recent years. Nevertheless, this paper does not intend to discuss the technical problems of signalling game but concerns with the epistemic aspect. The question the paper tries to discuss is that whether the selection and maintenance of a strict Nash equilibrium in a signalling game mean the establishment of a signalling system.
In the case of a signalling game at a strict Nash equilibrium, the receiver plays the act proper to a state the sender perceives. In other words, the act causally maps the state correctly. According to the evolutionary approach to signalling games, strict Nash equilibria in a signalling game equal to signalling systems. That is to say when the causal relationship between the act and the state is established, a signalling system emerges. However, the causal relationship can be established without signalling systems in the case of mutual misunderstanding. There may be two orders of mutual misunderstanding in signalling games: the first-order between senders and receivers and the second-order between the observer and the signalling game s/he observes.
Mutual misunderstanding is the result of the absence of common knowledge between the sender and the receiver in a signalling game, and the observer of a game and the players in the game. Therefore, signalling games by evolutionary dynamics are insufficient to reject the common knowledge. The source of mutual misunderstanding is a long-standing confusion in information studies: confusing signal sequence and pragmatic effects of information with informational content. In the case of signalling game, philosophers take the success conditions of acts as the success conditions of communication while the mutual misunderstanding argument shows that they are different. In order to avoid mutual misunderstanding without appealing to common knowledge in signalling games, the distinction between signals, informational content and acts should be made clear first.
The configuration of signalling game and the evolutionary approach to it will be introduced in section 2. Section 3 analyzes a Chinese folk story, Magical Fight, as an exemplar of mutual misunderstanding. The story shows that although the interacts between two players in the magical fight are successful for both the players and audiences, there is no effective communication between the players due to no common knowledge sharing by them. Section 4 argues that the magical fight is a signalling game in which two orders of mutual misunderstanding happen. Possible objections to the mutual misunderstanding argument are considered in section 5. Section 6 investigates the source of mutual misunderstanding in the studies of signalling game: possible mismatches between signals, informational content and acts.

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ELEMENTS OF CONTINUITY IN THE CIRCULATION OF MATHEMATICAL KNOWLEDGE AND PRACTICES IN CHAPTER "MEASURES IN SQUARE" IN MATHEMATICAL WRITINGS IN CHINA

Session 28J

Congress section(s): B3

My aim in this talk is to analyze the process of circulation of a mathematical method, as it is evidenced by its occurrence in Chinese mathematical works from different periods which rely on one another.
The earlier occurrence of this method is found in The Nine Chapters on Mathematical Procedures (thereafter, The Nine Chapters), which is one of the most important mathematical works from China. We place its completion date in the form handed down somewhere between the first century B.C.E. and the first century C.E. One chapter of this book was entitled "measures in square (fangcheng)," and it deals with what, in modern terms, are systems of linear equations, even though important differences between the two exist.
A 13th-century scholar Yang Hui (fl. 1261 CE) wrote, under the title Mathematical Methods Explaining in Detail The Nine Chapters, (thereafter, Mathematical Methods), a commentary on The Nine Chapters relying on both the book and former commentaries on it. His commentary testifies to continuities as well as breaks with respect to his base text. Yang Hui commented on different mathematical procedures from the base text, including the "procedure of the positive and negative".
The second source I focus on is Great Compendium of Mathematical Methods of The Nine Chapters with Analogies (thereafter, Great Compendium), a book that a 15th-century scholar Wu Jing (fl. 1450 CE) composed relying on the text of Mathematical Methods. Wu Jing inherited the concepts and practices related to the "method of the positive and the negative", which Yang Hui had introduced in Mathematical Methods. However, in relation to the culture in the context of which he wrote, and in particular the emphasis he placed on the value of uniformity of procedures, Wu Jing modified the way of solving problems in this chapter of The Nine Chapters.
My case study on the basis of one mathematical procedure in Mathematical Method and Great Compendium shows one of the differences between the scientific cultures to which the two mathematical writings belong respectively. And taking advantage of this difference, I could reveal the circulation of mathematical knowledge and practices from different periods.

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AN ATTEMPT TO DEFEND SCIENTIFIC REALISM

Session 18G

Congress section(s): B4

In today metaphysics, the debate between realist and antirealist is of central importance with respect to the truth value attributed to our best scientific theories. Scientific realism is a realism regarding whatever is described by our best scientific theories and it aims at dealing with the following questions: i) can we have compelling reasons to believe in the (approximate) truth of our scientific beliefs? ii) which are the criteria used to attribute truth value to scientific theories?
On the one hand it seems fair to admit that scientific realism is the best option to embrace in order to give a definitive answer to these questions; but on the other hand, scientific realism seems hard to defend because, unlike antirealism, it has to bear the burden of proof.
In our presentation we aim at presenting Stanford's antirealism position put forward in his Exceeding our grasp: Science, history, and the Problem of Unconceived Alternatives (2006), and then at giving some realist's replies inspired us by Chakravartty (2008), Magnus (2010) Forber (2008), Godfrey-Smith (2008), Ruhmkorff (2011), and Devitt (2011).

Besides the two main antirealist arguments (i.e. the “empirical underdetermination of theories by data” (EU); and the “pessimistic induction” (PI) also labelled “pessimistic meta-induction” (PMI)) there is a new challenges offered by Stanford in 2006 which has been labelled “the problem of unconceived alternatives” (PUA) or the “new induction” over the history of science (NI) which combines (EU) with (PMI) suggesting that:
P1) science is subjected to the problem of unconceived alternatives: plausible alternatives are not conceived, thus our choice is not the best or the true one;
P2) recurrent, transient underdetermination maintains that by looking at the history of science there were scientifically plausible alternatives to the past accepted theories that were not conceived; some of them were accepted later, while the theories actually accepted at that time were later shown to be false;
C1 (new induction) our theories are not different from past ones. Thus, by induction, there are some plausible alternatives to our current theories that are not even imagined and entertained;
C2) there are no compelling reasons to believe in our best theories.
Towards this latest antirealist argument there are many realist replies which aim at showing that Stanford’s argument is inappropriate and it diverts the attention from the main realist claim, namely the induction over scientific theories in Stanford becomes an induction over scientists and their cognitive abilities in exhausting the set of all plausible alternatives. Assuming that PUA is similar to PI, we are going to prove that a possible reply to the classic PI can be also used as a reply to PUA because they are both based on a historical induction.
First, a principle of continuity can be established between the different formulations of a theory in order to see which elements of the theory are retained over its historical development. This allow to save at least a partial version of scientific realism.
Secondly, Stanford’s argument does not work as a real argument against scientific realism because relies on a distinction between community level proprieties and individual level properties; in fact, Stanford appeals to the cognitive limits of scientist’s without focusing the attention on the real realist’s claim: the comparison between the content of our best scientific theories and the physical reality they aim at describing, explaining and predicting.
Third, the cognitive limits of past scientists would not necessary be the limits of future theorizers because history teaches us that science has been getting better and better. Let us just take for example the last century physics which has undergone to an exponential growth if compared with the centuries before.
Finally – as Devitt suggests – to undermine the PUA challenge we can appeal to the methodological and technological improvements shown in our scientific life. In fact, this version (with respect to the classic realist reply, namely that there is a difference in the breadth, precision, novelty, or other important features of the predictive and explanatory accomplishment of past and present theories) explains why present theories are more successful and hence removes the whiff of ad-hoc-ery.

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INTERLOCKING MODELS VALIDATING ENGINEERING MEANS-END KNOWLEDGE

Session 8D

Congress section(s): C8

A substantial part of the knowledge developed by engineers in commercial laboratories and universities of technology consists of means-ends knowledge (MEK), or (intersubjective) know-how. This knowledge concerns the question which engineering interventions to apply to achieve a pre-specified technical goal within a concrete well-identified engineering context. Engineers need and produce a lot of it. For instance, Nancy Nersessian and Christopher Patton recognize know-how as one the three types of outputs during their ethnographic investigation of biomedical laboratories, besides artifacts and descriptive knowledge (DK) (2009: 728/730). In the rest of their chapter, however, the relation between these three remains somewhat underdeveloped. For methodological purposes, I take Wimsatt’s (2007) re-engineer philosophy perspective, and propose to put engineering MEK as an irreducible and equally important candidate of an engineering project beside DK. Besides their differences in goals, MEK and DK have many incompatible characteristics. To mention a view: the first is value-laden on object-level, intentional, valued for its context dependency and defies truth-values whereas none of these apply to DK. The differences in characteristics raises the question how, in contrast to DK, MEK is validated. In this paper, I show that engineers often refer to models to validate MEK, besides applying f scientific knowledge and carrying out practical experiments. To illustrate this claim, I discuss the way in which William Froude prescribed how to use scale models to predict the resistance of a hitherto non-existing ship hull. It turns out that Nersessian and Patton’s technical term of interlocking models is of great help to disentangle the intricate question of model-based MEK validation. Moreover, not only models are indispensable for the development and validation of MEK, but also MEK plays a crucial role in the development of models in an engineering project. As such the identification of MEK in engineering practices provides extra insights into the complicated processes of Model-based reasoning as well.

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