CLMPS 2011
Volume of Abstracts

Division of Logic, Methodology and Philosophy of Science of the International Union of History and Philosophy of Science

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Volume of Abstracts
CLMPS 2011

Special Topic
Logic and Science Facing the New Technologies

14th International Congress of Logic, Methodology and Philosophy of Science
Nancy, July 19–26, 2011 (France)

Contents

Editorial Note

The *Volume of Abstracts* contains the abstracts of plenary and invited lectures, special symposia, as well as contributed papers and symposia of regulary regristered participants of the Congress, such as they were submitted by the authors. We apologize in advance for errors which could have occured while converting the files in LaTeX.

For practical reasons we chose to include only short abstracts. Longer ones can be found on the Congress Website: http://www.clmps2011.org/en/editorial.html and on the provided CD.

Official Program

I – Plenary Lectures

DLMPS—Tarski's vision and ours

Tue. 19/07
2:00 – 5:00 pm

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The first International Congress of Logic, Methodology and Philosophy of Science took place in 1960. So this present Congress in Nancy is an occasion for us to review the first fifty years of DLMPS Congresses and cast an eye forwards to the Division's second fifty years. The form of DLMPS was the result of several influences. In the aftermath of the Second World War, the International Council of Scientific Unions (ICSU) gave itself the task of setting up and coordinating International Scientific Unions; the administrative form of DLMPS comes mainly from its ICSU connection. The Division's sphere of interest was the result of a tug of war between philosophers of science (notably Ferdinand Gonseth) and logicians (notably Evert W. Beth and Alfred Tarski). The historians report that the logicians had the better of the fight, largely through Tarski's dominance. Today we should ask whether our administrative form still makes good use of the possibilities, and whether our inherited sphere of interest is appropriate for the present situation in logic and philosophy of science.

Retrocausality—what would it take?

Thu. 21/07
8:30 – 10:00 am

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Some writers argue that retrocausality offers an attractive loophole in Bell's Theorem, allowing an explanation of EPR-Bell correlations without "spooky action-at-a-distance." This idea originated more than a decade before Bell's famous result, when de Broglie's student, Olivier Costa de Beauregard, first proposed that retrocausality plays a role in EPR contexts.

The proposal is difficult to assess, because there has been little work on the general question of what a world with retrocausality would "look like"—what kinds of considerations, if any, would properly lead physicists to conclude that we do live in such a world, and what that would mean, in terms of the structure and use of physical models. In this talk I begin with a brief discussion of these general issues, with the aim of bringing the more specific question as to whether

quantum theory implies retrocausality into sharper focus than has hitherto been possible.

Against this background, I then consider the suggestion (made by Costa de Beauregard himself, amongst others) that time-symmetry counts in favour of retrocausality in the quantum case. I show that this is true under some assumptions about quantum ontology but not others. In the remaining cases, the most that can be said is that it remains an open question whether the quantum world is retrocausal, even if we assume time-symmetry. Contrary to conventional wisdom, however, there seems to be little justification for the standard assumption that the quantum world is *not* retrocausal. At present, this assumption seems to be dogma, not science.

Engineering technical artefacts and scientific instruments

Sat. 23/07
8:30 – 10:00 am

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In this paper a comparison is made between engineering and scientific approaches to the design and construction of artefacts. The paper aims to show that the engineering of artefacts involves a separate body of knowledge, thus undermining the idea of engineering as being just applied natural science. This will be discussed on the basis of a case study, the test fusion reactor ITER that is currently being built in France.

Magnetohydrodynamics (MHD) is the science of conducting magnetized fluids. The theory combines Maxwell's equations with the gas dynamics equations for the evolution of density and pressure. MHD allows physicists to understand elements of solar physics, the development of the earth magnetic field and the behaviour of fully ionised gasses (a.k.a. plasmas). In nuclear fusion research hot plasmas are confined by magnetic fields. MHD explains that in fusion plasmas at specific locations resistive instabilities (tearing modes) can develop. These modes are important from both a scientific and a reactor performance perspective.

The key notions of magnetohydrodynamics (MHD) will be explained. Then two instruments for MHD research will be compared. Both instruments are based on 140 GHz electro-cyclotron (EC) waves. The 2D ECE system is designed and built for scientific research in resistive MHD modes. The in-line ECE is a prototype of a real time control system for these modes, and designed and built for operational purposes (the suppress these modes and to increase the fusion power). The requirements and design of, and implementation of these systems on the small German tokamak TEXTOR will be discussed. If we were to bring these instruments to maturity for installation on ITER additional requirements emerge, associated with safety, licensing, lifecycle considerations, waste-management and operational availability.

On the basis of this comparison it will be argued that a specific body of engineering and design knowledge is involved in the second case. This knowledge is required to deal with complex trade-offs and cannot be derived from the natural sciences. The requirements for the mature scientific instrument, on the

other hand, are only performance driven and similar to the typical requirements associated with prototypes. Finally, the design process seems firm and well established. External factors or the requirements can change, and this will affect the object of design, but not the design process.

"THE SOUL OF THE FACT": POINCARÉ AND PROOF

Mon. 25/07
8:30 – 10:00 am

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I argue that for Poincaré the uninteresting part of proof is rigour, and that the interesting part is the role a proof plays in understanding a piece of mathematics.

IS THERE A GENERAL CONCEPT OF PROOF?

Tue. 26/07
8:30 – 10:00 pm

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Notions of formal proofs are usually proposed as attempts to capture truth or provability within a certain domain. More seldom does one try to capture the epistemic process by which we get to know that a sentence or proposition is true. The lecture will try to do that, and will in this way approach questions concerning a general notion of proof for a language.

Affiliated plenary lecture

Fri. 22/07
8:30 – 10:00 am

WHAT THE DECISION THEORIST COULD TELL THE BAYESIAN PHILOSOPHER

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Bayesianism is a pervasive doctrine in current epistemology, philosophy of science and statistics. There are three main Bayesian tenets, i.e., that (i) empirical claims are to be evaluated by their probability values, (ii) evidence for or against them is to be weighed through probabilistic conditioning, and (iii) decisions are to be made as prescribed by the expected utility rule or the equivalent conditions on preferences. Arguments for (i) and (ii) may be classified as pragmatic or non-pragmatic, depending on whether the justification strategy also involves (iii) or puts it aside. Furthermore, pragmatic arguments may rely more or less heavily on the apparatus of decision theory: some are pretheoretic, like the Dutch Book argument, while others take the advanced form of a representation theorem, as in Savage's *Foundations of Statistics.* This lecture is concerned only with pragmatic arguments, and it will take the line, already present in today's Bayesian philosophy, that they cannot win the day if they fall short of a proper representation theorem. However, the lecture will unconventionally emphasize that Savage's classic does not yet deliver the appropriate result and that more sophisticated decision-theoretic work must be carried if one is to establish (i), (ii) and (iii) jointly. One major complication we will emphasize stems from event-dependence of utility values; it calls for a much richer framework of preferences than in Savage. An even more dramatic departure we will propose is to disconnect (i) and (ii) from (iii) by replacing expected utility by a more general, and arguably more appropriate, rule of decision under uncertainty. Despite the Pandora's box of initially unnoticed problems, not all of which are resolved in the state of the art, we will strongly maintain that the detour by decision theory is unavoidable if one is to justify Bayesianism pragmatically.

II – Special Sessions

Special Session 1

Wed. 20.07
4:35 –7:05 pm

What is an algorithm?

Chair: Helmut Schwichtenberg

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Abstracts

What is an effective algorithm?

Nachum Dershowitz
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I will discuss Gurevich's axiomatic definition of classic, sequential algorithms, and the fact that, by adding a postulate regarding the finite representability of initial states, one can formally prove the Church-Turing thesis. More generally, I will address the question of what about a process makes it "effective".

What's an algorithm?

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We plan to address a variety of issues:

0. Algorithms vs. computable functions
1. Is it possible to define algorithms? The answer is in fact obvious.
2. What kind of entities algorithms are.
3. When are two algorithms the same?
4. Why bother to define algorithms?
5. Axiomatic definition of algorithms.

PANEL DISCUSSION ON "WHAT IS AN ALGORITHM?"

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There are three issues that I will introduce and discuss (very briefly):

1. Relativization, uniformity and effectiveness of algorithms. Any formal definition of algorithms must take into account and make precise the following three features of algorithms:
   a. Relativization: an algorithm computes a function f : $A \to B$ "from" (or "relative to") specified primitives—i.e., there are no, non-trivial "absolute" algorithms. (And for this discussion I will confine myself to the case where the primitives are (partial) functions on A, B.)
   b. Uniformity: an algorithm uses "the same procedure" to compute $f(x)$ for all inputs in A.
   c. Effectiveness: the computation of $f(x)$ is "effective", except for the "calls to the primitives".
2. Recursion and computation. It is possible to formulate a version RCT of Church's Thesis for "recursive" (rather than "computable") functions, always from specified primitives. There is a relation between RCT and the classical CT of course, since the classes of recursive and computable functions coincide on the natural numbers relative to minimally rich primitives, but even then, the two theses are not "intensionally identical". I will give an argument for RCT which (I believe) is easier to defend than the standard CT.
3. Applications. CT grounds the derivation of (absolute) undecidability results for interesting mathematical problems, in fact these were the first (and for a long time the only) applications of logic to mathematics. I will give some examples of applications of RCT to the derivation of "absolute lower bounds" for (various) complexity measures on interesting decidable problems , especially in number theory and algebra.

Special Session 2

Thu. 21.07
2:15 – 7:05 pm

Quantum Information—Conceptual issues and new technological developments

Chair: Dennis Dieks

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Abstracts

Simulation, computation, and physics – What can we learn about the world?

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We shall discuss modern notions of computation, simulation and (quantum) physics, and their mutual roles in our effort of understanding Nature. As part of this discussion, we shall describe the scheme of projective simulation, which we recently introduced in the context of learning.

Einstein and Bohr meet Alice and Bob

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The debate between Bohr and Einstein about the conceptual tensions of quantum mechanics was primarily about the problem of completeness (the 'hidden variable' problem) and the measurement problem (the problem of Schrodinger's cat). The Bohr-Einstein debate came to a head in 1935 with the Einstein-Podolsky-Rosen paper, in which entanglement emerged as a key feature distinguishing quantum mechanics from classical mechanics. After the Kochen-Specker theorem and Bell's reconsideration of the completeness issue about thirty years later, the focus of attention shifted to the nonlocality and contextuality of quantum mechanics. For most philosophers interested in quantum mechanics, completeness and the measurement problem remained the central conceptual issues. By contrast, there has been an information-theoretic turn in the physicist's understanding of quantum mechanics. Wheeler's question 'Why

the quantum" has two sides: Why is the world quantum rather than classical, and why is the world quantum rather than superquantum? The second question has turned out to be more fruitful. Progress in answering this question has led to the recognition of a profound structural difference between the information-theoretic properties of classical, quantum, and superquantum theories, and consideration of the new technological possibilities that arise by considering what sorts of information-theoretic tasks Alice and Bob can perform by exploiting entangled states. I discuss this information-theoretic turn, and consider to what extent the issues between Bohr and Einstein have been resolved.

The invasion of physics by information theory

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One of the slogans of quantum information science is that "information is physical", by which it is meant that the sorts of information-processing tasks that can be achieved in nature depend on our best physical theory thereof. Indeed, this field of research has revealed that living in a quantum world provides significant advantages for many computational, communication and cryptographic tasks. However, another good slogan for the field is that "physics is informational": taking an information-theoretic perspective on physical theories can deepen our understanding of them and contribute to their development. Such a perspective has already had a great impact on the foundations of quantum theory and has recently begun to spread to the rest of physics. In other words, we are beginning to see information theory being used to solve problems that are primarily of interest to physicists rather than computer scientists. This talk will review some examples of this phenomenon, in particular the application of information-theoretic tools to the problem of determining the consequences of dynamical symmetries.

Bell's Theorem and EPR correlations: The issue, the triumph of the scientific method, misinterpretations, and practical applications

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EPR paper and Bell's theorem addressed issues that seemed to be of interest only for philosophically biased scientists, or philosophers themselves. The initiators of the debate, hoped for a different final result than the implications of Bell?s theorem (either realism cannot hold or one might have non-locality, or both). Thus implications went far beyond expectations. These developments can now be considered as the very beginnings of quantum information science. Violations of Bell's inequalities are helpful in detecting entanglement, define problems in which quantum processes can reduce communication cost, allow quantum cryptography. Bell-type reasonings influence our understanding of the quantum speedup in computations, and set new limits in information theory.

Special Session 3

Fri. 22.07
10:30 – 1:00 pm

Mathematics and the new technologies

Chair: Benedikt Löwe

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Formal mathematics and mathematical practice

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Gödel's completeness theorem together with standard first-order formalizations of the foundations of mathematics imply that every mathematical theorem can in principle be derived in a strictly formal proof calculus. Formal mathematics is currently witnessing a tremendous expansion due to the development of theoretical ideas and of powerful computer tools. There are now formal proofs of many key theorems including the prime number theorem, the four color theorem, or the Jordan curve theorem. Formal mathematics systems still require expert users who are prepared to supply a considerable overhead of mathematically uninteresting formal subleties. This contrasts with the informal, intuitive, and often vague and incomplete style of argumentation that is considered to be "natural" in mathematics. Whether formal mathematics will enter common mathematical practice will depend on the degree of "naturalness" of future systems. I shall discuss the potential for naturalness in some components of formal mathematics systems: input and output languages, proof styles, automatic theorem provers. I shall then speculate about applications in everyday mathematics.

The Internet: New technology in old bottles?

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From its early days on, the Internet has been invested with utopian and dystopian narratives. Analysts and proponents alike have emphasized the Internet's transformative power and technological might, typically combined with a focus on online activities in isolation. Recent social science scholarship has

contrasted this perspective by considering, more broadly, how the Internet has become embedded in everyday life, conceiving of it as being but one technology among others that afford communicative connectedness and the exchange of information. The proposed presentation will focus on a scientific community that has used the Internet abundantly early on: theoretical particle physics, as observed in an ethnographic study at CERN, the European Organization for Nuclear Research in Geneva (Switzerland). I will discuss how theorists make use of the Internet both as a communication and an information technology, paying attention to established practices, their interaction with Internet affordances and the effects thereof. I will argue that the Internet differentially resonates with central elements of scientific cultures. This explains why Internet applications become more or less firmly engrained in a specific scientific culture, a process in the course of which they may also be considerably transformed.

MATHEMATICS IN THE CLOUD: THE WEB OF PROOFS

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The use of computers in mathematics has now become a familiar feature in mathematics, although perhaps not accepted by everyone. Probably the best known cases concern number crunching (where the number of cases to be examined can be of superhuman size), programs searching for patterns in sets of data of similar size, proof rewriting and verification and the creation of mathematical on-line libraries. In this lecture I will look at the Polymath project that was initiated by Timothy Gowers, resulting in a proof of the density Hales-Jewett theorem (see [1], where most relevant papers are collected). In particular I want to have a closer look at the claim that this project has changed the way mathematics is done and, if the claim does hold, what the implications are for the philosophy of mathematics.

References

[1] Imre Bárány and Jozsef Solymosi. *An Irregular Mind (Szemerédi is 70).* Number 21 in Bolyai Society Mathematical Studies. Springer, 2010.

III – International Union of History and Philosophy of Science

Joint Commission Symposium

Mission of the Joint Commission of IUHPS

Joint Commission (JC) of the IUHPS is established to realise a unique task. That is to help strengthening the interaction in research between its two Divisions, recently the DHST and the DLMPS. This goal is promoted by organizing an event where mutually interesting topics for both Divisions are discussed by invited speakers. Composed of the presidents and general secretaries of DHST and DLMPS and an by them elected chair, Imre Hronszky, the recent Joint Commission choose a topic that mirrors the high recent interest in history and philosophy of technology issues, in issues in which cognition is explored in interaction with a broader societal and philosophical (action theory, ethics) context. Together with the strong interest of the recent clmps in technological topics the Joint Commission Symposium has to forward research in this direction.

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1 –Understanding through making

Thu. 21/07
10:15 am – 1:30 pm

From synthetic chemistry to synthetic biology: The revival of the verum factum principle

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The recently emerged synthetic biology differs from received biotechnologies, such as metabolic engineering, by emphasizing the creation of living beings rather than their mere modification. Apart from the technological use of the products, it is promised that the creation will also improve our basic understanding of life. Various epistemic claims have been made that revive the classical verum factum principle: from bold claims such as "What I cannot build, I cannot understand" to more modest statements according to which the creation of living beings brings about some important understanding. Such claims are frequently justified by the analogy between today's synthetic biology and 19th-century synthetic chemistry.

In this paper I scrutinize both the epistemic claim of synthetic biology and its analogy to synthetic chemistry. I will do so by first reconsidering the verum factum principle in philosophy and its various well-known applications to 19th-century synthetic chemistry, which requires a historical and methodological analysis of the chemical structure theory. By exploring the analogy to synthetic biology, I will argue that essential features are missing here, such that it is difficult to uphold the verum factum principle. Moreover, because synthetic biology explicitly gives up traditional epistemic goals, in order to distinguish itself from established fields, its epistemic ambition becomes questionable.

References

[1] Joachim Schummer. *Das Gotteshandwerk: Die künstliche Herstellung von Leben im Labor*, Frankfurt: Suhrkamp, 2011.

Synthetic biology: The construction of a discipline with interdisciplinary contents

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As she raised the question, "What does synthetic biology have to do with biology?", Evelyn Fox Keller argued that synthetic biology does not aim at understanding organisms as they have evolved, and rather tries to remake these organisms for human uses. While the engineering culture certainly distinguishes synthetic biology from traditional biology, another not less important feature differenciates this emerging field: its transdisciplinary dimension. Synthetic biology combines knowledge from a large number of disciplines, including molecular biology, engineering, mathematics, chemistry, and physics. This paper will raise the issue of the the unity and consistence of this emerging field.

Emerging technosciences and changing relations between technology

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The goal of nanobiotechnology, i.e., of the use of nanotechnology to access biological systems, is to employ technology to influence and shape living systems to a greater degree. It even offers the perspective of artificial life being created by means of synthetic biology. Decisive for scientific and technological progress is the combination of knowledge about molecular biology and genetic techniques with the new opportunities offered by nanotechnology.

Synthetic biology and systems biology have recently turned into a vibrant field of scientific inquiry full of technological expectations. The combination of engineering with biology promises to make it possible to fulfill many of the goals expected of nanotechnology in an even easier fashion: while nanotechnology involves the development of materials and machines at the nanoscale synthetic biology builds on the insight that nature already employs components and methods for constructing machines and materials at very small scales.

The knowledge gathered by nanobiotechnology can be used *to produce new* functions in living systems by modifying biomolecules or the design of cells, or designing artificial cells. The traditional self-understanding of biology, which is molded by the natural sciences and which aims at *understanding* vital processes, is reinterpreted by synthetic biology as a *new invention* of nature and as the

creation of artificial life on the basis of our knowledge about "natural" life. This transforms biology into a technical science that has the dual strands of cognition and design characteristically for the technosciences.

Against this background the presentation will cover the following issues:

- analyze the changing self-understanding of biology by looking at the language used in synthetic biology - which can be shown to be transferred from classical engineering disciplines such as electro or engine engineering
- conceptualize terminological challenges concerning the definition of life if living systems are more and more being described in technical language
- consider the changing relation between technology and nature, in particular life which might be technicalized in analytic and synthetic respect to a far extent
- identify methodological challenges (control and prediction issues) emerging from the technical use of self-organizational mechanisms taken from nature.

2 – Coevolution of technology and society

Thu. 21/07
3:00 – 7:00 pm

How to modulate coevolution of technology and society?

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It may seem obvious that technology and society co-evolve, but implications are not always drawn, also because there is not enough understanding of the actual dynamics. There are historical and sociological aspects, and philosophical and action perspectives. This makes co-evolution of technology and society, and its modulation, an important topic for a symposium at a IUHPS meeting.

Historically, technology development becomes a separate activity from the 18th century Enlightenment and early industrial revolution. A division of labour in assessing and "controlling" technology in society evolves later, and has become institutionalized in our days, with government departments of trade and industry pushing for new technology, and other government departments like social affairs and environment attempting to regulate.

Another historical development is that the co-evolution becomes more reflexive, particularly after the Second World War. The rise of science policy, and then technology and innovation policies, are indicators, but also the institutionalization of technology assessment beginning in the early 1970s.

One can ask whether such policies and assessment exercises make a difference to the dynamics of development. Perhaps little if one thinks in terms of steering to achieve a specified objective (although this can happen, as the program for civil nuclear energy showed—until it faltered). But if one thinks of modulation,

i.e. attempts at change from within, and with process rather than product objectives, one can see changes occurring. The causality is complex, of course.

Further distinctions have to be made, in particular the difference in dynamics between "advanced technologies" like genetic engineering and nanotechnologies, and "sociotechnical systems engineering" with geo-engineering as a recent ambitious example (definitely, a technology of hubris rather than humility). Reflexivity in the sense of policy and assessment will be different, and there is now also broader reflexivity in the sense of societal hopes and concerns (and occasional debates). This can have effects when actors (technology promoters) anticipate on possible societal responses. This is happening now, at least at the symbolic level, in nanotechnology.

Grand narratives, local minds and natural disasters: Community responses to tsunami

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This paper examines the philosophical issues connected with the collapse of grand narratives in local minds with reference to the Tsunami disasters that hit India in 2004 and Japan in March 2011. It is argued that faced with the onslaught of a natural event, local communities relapse to their traditional, non-scientific ways of analyzing the problem. This may not be a desirable approach, given the fact that natural disasters like Tsunami are risks that defy prediction. It takes re-configuring of narratives to gain the trust of local communities in modern technologies. This calls for a post modern turn that involves the evolution of new narratives that are based on experiences of the victims with disaster coping mechanisms.

Extreme natural disasters like the Tsunami have tormented human communities. Efforts to prevent and manage the fall-out of natural disasters have also been the major pre-occupation of larger human community formations, notably nation states. It was not until the advent of modernity that grand narratives emerged on sustainable approaches to warding off natural disasters. These grand narratives zeroed on to 'modular technology solutions' to prevent the disastrous effects of natural calamities. Earthquake proof buildings, bridges and strong concrete sea walls along the shore lines were part of modular technology solutions. These structures were deemed to be robust and universal that could straddle space—across landscapes and human communities and entailed their distinct patterns of land use in vulnerable areas. In the process, these modular technology solutions advanced to handle the tsunami problems re-configured the traditional mental-scape of local communities in relation to 'place'. However the trial of destruction wrought by the tsunami waves that hit Sendai in March 2011 destroyed not only human lives, houses, infrastructure and nuclear power establishments but also faith in robust technological solutions that were touted as the best damage control mechanisms.

In India, a country which had not heard of the tsunami until the monster waves struck its coastal areas in the Southern region, there were no modular preventive technology solutions in existence. However faith in advanced disaster

forewarning systems lay shattered. The series of warnings about possibility of more tsunami waves following the one that initially struck the affected coastal areas turned out to be 'untrue'. The sprouting of alternative 'local community' theories on the 'why' of tsunami and the 'how' of facing it were rooted in the traditional epistemes of astrology and geomancy.

Indeed the common factor that binds tsunami victims of India and Japan lay in its two fold local response—namely disillusionment with the 'technology dream of modernity' and the effort to cognitively and/or physically redefine the problem in terms of local idioms. This is best reflected in new literary works and new local texts of social knowledge that arise in the wake of the disaster.

This paper examines the philosophical issues connected with the collapse of grand narratives in local minds. It is argued that a post modern turn that builds new narratives on the foundation of newly engendered local community knowledge and experiences is the best way to move beyond the disillusionment with modernity. The new, altered narrative arises from the foundations of re-configured mental-space resulting from the disaster.

Fri. 22/07
10:15 am – 2:15 pm

Realworld simulation: A conceptual tool for technoscientific field sciences

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The complexity of ecology is mainly due to the fact that ecological objects are construed according to different modes of description. A number of analytically useful and distinctive concepts will be discussed that are to sharpen our understanding of what technoscientific objects are and how they can be distinguished from scientific objects. Most notably will be focused on the notion of real word simulation. The object in question is an artificial water catchment that is a constructed natural site. The "Hühnerwasser – Chicken Creek (DFG-Project SFB/Transregio 38)" serves to analyze the "structures and processes of the initial ecosystem development phase in an artificial water catchment". Basically, it is an isolated artificial sand heap, with an altitude difference of approximately 10 m and a small lake at the deepest part of the site. The object is situated on the area of an abandoned pit mine in Eastern Germany. To bring forward the notion of the real world simulation it is of particular interest that in a certain sense the artificial water catchment simulates its own behaviour in that it monitors its performance. It is a kind of field experiment that abolishes the carefully maintained spatial separation between an experimental system and the natural system, which it is supposed to represent. This raises the problem of how to critically assess findings from this real world simulation, and thus of how to adequately characterize the vantage point of description.

The study on real world simulation and on more general aspects of developing a descriptive pattern for technoscientific objects is part of a German-French DFG/ANR research project on "The Ontology and Genesis of Technoscientific Objects".

Design between science and art. Historical remarks

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Important contributions have been made by the research in engineering design recently. The focus is on the understanding (describing, explaining, prescribing, predicting) and planning process and the result of the imaginary devising and outlining of novelty. This revolves around the conditions of the possibility of "design". There is a broad and interesting discussion with different points of view, strategies of problem solving and directions of answer continuing the same discussion in the history of engineering sciences too. One of the main-questions has been: "Is the design process more a rational, formalized or more an 'intuitive' process?" or more generally "Is design science or art?". (This leads to the current question "Is it possible to generate a general theory of design?") Answers to this question have been different both in history and recently. This difference is related to the understanding of engineering on the one hand, to the understanding of science, art, heuristics, ways of problem-solving on the other hand (role of values and evaluation in design, types of thinking in design, relationships between logic—in a large scale—and heuristics, influence of language, visual thinking and non-verbal knowledge and its representation—for example sketches, drawings, models, and the teachable and learnable in design). The design process is a "mix" of different components (science *and* art). This will be first shown. Second, some historical remarks on the development of "design theory" (above all in Germany) will be given.

Group cognition within living lab research and innovation, the cycle of experiential knowledge

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[No abstract available]

The use of the technological paradigms conception in history of technology and theoretical economics

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We somehow are determined to believe that historians and philosophers (of science and technology) can enrich their work mutually. But going into details uncovers great difficulties and makes one to believe that the processes may be very complex. Kuhn explains how much did he learnt from historians of science

and was certainly convinced about the validity of his conception for history. But The Structure, that "highly schematic sketch" (Kuhn 1990) was not expected by Kuhn himself to be directly utilised by historians of science. The paradigm conception initiated changes of perspectives in different disciplines. One of them was introducing technological paradigms in the early 80s. Historian of technology Edward Constant II outlined a technological paradigm for using it in his research on the story of introducing turbojets. Rachel Laudan, while realizing some demarcationistic thinking, identified a role of technological paradigms for philosophy and history of technology. She took into account the autonomy of technological cognition allegedly being the base for some autonomy of technology dynamics in the socio-economic environment. Giovanni Dosi exemplified a double approach in which the conception of a technological paradigm was used both for providing for an empirical case (of matching?) when applied to the semiconductor industry and for introducing the concept to the evolutionary economic theory to account for the autonomy of technological dynamics. It was expected to help understanding both the "Schumpeterian phase" of dynamics and the stylized fact of cumulativity experienced in some part of technological dynamics. The analogy is utilized in economy in a goal-oriented way, one time is application of Kuhn, then "matching" Kuhn to economist commonsense. While referring to it in the phases mentioned, especially the selection among paradigms themselves is non-Kuhnian. Carlota Perez introduced the idea of a techno-economic paradigm. Technological and techno-economic paradigms became standard elements in some theories of economic dynamics.

The presentation assesses the processes of the story. It also compares it to the potential the use of paradigm thinking would have meant if Kuhn had been read through the eyes of Margaret Masterman concerning the role of analogical thinking in the self-organizing preparadigmatic dynamics. It equally compares the above processes to the potential the use of paradigm thinking would have meant if paradigm dynamics had been read through the interpretation of Joseph Rouse. The latter focused on the differences between a representalist and a view that can be named practice perspective (both conceptual and material) in paradigm dynamic: i.e. looking at technologies as first of all being able to realize something. Beside assessing the early period of the story in the first half of the 80s the presentation deals with developments in the 21st century too, by focusing on the ideas of Perez in Technological Revolutions and Financial Capital and the more recent applications of the techno-economic paradigm perspective by Giovanni Dosi, Christopher Freeman and others.

Roundtable: Responsible development of nanotech?

- Facilitator: Imre Hronszky
- Participants: Arie Rip, Armin Grunwald, Elena Pariotti (Italy)

IV – Ordinary Sessions

A. Logic

A1. Mathematical Logic

Invited Lectures

Logic, structure and complexity

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There is an intricate interplay between the computational complexity of algorithmic problems, their logical definability, and structural properties of their instances. Connections between computability and logical definability played an important role ever since the beginning of computability theory in the 1930-40s. The area of descriptive complexity theory, which emerged in the 1970s, is concerned with connections between computational complexity and logical definability. While it is long known that favorable structural properties of the input instances can greatly help in the design of efficient algorithm, only recently such structural properties have been systematically studied in the context of descriptive complexity theory. After a general introduction to the theme, in my talk I will report on recent results giving a logical characterisation of the class of all polynomial time decidable graph properties on classes of graphs defined by topological properties such as planarity.

Comparing classes of structures

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Classifying objects up to some important notion of sameness is important in all branches of mathematics. One important notion of sameness is isomorphism. We compare isomorphism problems for different classes of countable structures using Borel embeddings, and effective embeddings. We can say that certain classes, such as linear orderings, graphs, and fields of any fixed characteristic, lie "on top". We also say, for certain classes such as number fields, or real closed Archimedean ordered fields, exactly which classes lie below.

Beyond the Turing degrees: non-diagonalizability and universal randomness

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Computability theorists use the Turing degrees to measure the complexity of infinite binary sequences. The complexity of other objects can often be measured by coding them as sequences. However, the Turing degrees may prove insufficient to capture the complexity of objects with no canonical code. For example, a countable graph G can be *presented* as a subset of $\mathbb{N}^2$ representing the edge relation, but different subsets of $\mathbb{N}^2$ will present the same underlying graph, each corresponding to a different labeling of the vertices by natural numbers. While each presentation has Turing degree, there need not be a least Turing degree presentation. A striking example was given by Slaman [4] and Wehner [5]: there is a graph that has presentations of every non-computable Turing degree, but no computable presentation. Clearly, no Turing degree captures the complexity of such a graph.

A similar phenomenon occurs in computable analysis. Say that $\lambda\colon \mathbb{N}\times\mathbb{Q}^+ \to \mathbb{Q}$ is a *representation* of a sequence $S \in [0,1]^{\mathbb{N}}$ if for all $n \in \mathbb{N}$ and all rational $\varepsilon > 0$ we have $|\lambda(n,\varepsilon) - S(n)| < \varepsilon$. Note that a representation can be canonically coded as an infinite binary sequence, so it has Turing degree. But different representations of S will have different Turing degrees, and as before, there need not be a least Turing degree representation. This was shown by Miller [3], who introduced the *continuous degrees*, an extension of the Turing degrees that captures the complexity of elements of $[0,1]^{\mathbb{N}}$ and of any computable metric space.

The key to showing that the Turing degree are insufficient for sequences of reals was the existence of a sequence $S \in [0,1]^{\mathbb{N}}$ that lists every real in $[0,1]$ that is computable from (every representation of) S. Such a sequence is not *computably diagonalizable*. Although every representation of S computes a real $x \in [0,1]$ not in S, no single such x is computable from every representation. Sequences that are not computably diagonalizable can be constructed using a classical topological fixed point theorem for multivalued functions on $[0,1]^{\mathbb{N}}$. Furthermore, such sequences characterize the continuous degrees that are not Turing degrees: they do not have Turing degree and any member of any computable metric space that does not have Turing degree is computably equaivalent to a sequence that is not computably diagonalizable.

Levin [2] gave an interesting example of objects that have continuous degree but not Turing degree, well before the introduction of the continuous degrees. A *neutral measure* m on $2^{\mathbb{N}}$ has the property that *every* infinite binary sequence is random (in essentially the sense of Martin-Löf) relative to m. We can think of a measure on $2^{\mathbb{N}}$ as a sequence of reals, in particular the measures of the basic clopen sets, so a measure has continuous degree. Day and Miller [1] observed that neutral measures cannot have Turing degree. On the other hand, every *PA degree* (i.e., a degree of a complete consistent extension of Peano arithemtic) computes a neutral measure. These facts help us understand neutral measures (e.g., we can show that every neutral measure has atoms of PA degree), but they also let us use neutral measures as tools. Day and Reimann recently used

them to show that if A has PA degree and B is a computably enumerable set not computable from A, then $A \oplus B$ computes the halting problem. This result takes place entierly within the context of the Turing degrees, and yet it follows naturally from the study of objects whose complexity transcends the Turing degrees.

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THE PROPER FORCING AXIOM

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The Proper Forcing Axiom is a powerful extension of the Baire Category Theorem which has proved highly effective in settling mathematical statements which are independent of ZFC. In contrast to the Continuum Hypothesis, it eliminates a large number of the pathological constructions which can be carried out using additional axioms of set theory.

GÖDEL'S FUNCTIONAL INTERPRETATION OF CLASSICAL ARITHMETIC AND ANALYSIS

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Gödel's (dialectica) *functional interpretation* is an interpretation of proofs in Heyting arithmetic into the quantifier-free calculus $\mathbf{T}$ [4]. Given a proof of A in Heyting arithmetic, Gödel's functional interpretation provides an algorithm to transform that proof into a sequence of terms $t \in \mathcal{L}(\mathbf{T})$ and a proof of $|A|^t_y$ in $\mathbf{T}$, where $|A|^x_y$ is a quantifier-free formula over the free-variables of A plus the two sequences of fresh variables x and y. So we have

$$\mathbf{HA} \vdash A \qquad \overset{\exists t}{\Rightarrow} \qquad \mathbf{T} \vdash |A|^t_y.$$

The interpretation is faithful, in the sense that $\exists x \forall y |A|^x_y$ implies the original formula A, assuming the axiom of choice, the principle of independence of premise for universal formulas, and the Markov principle (see [1]). If we denote by $\mathbf{HA}^*$ the extension of Heyting arithmetic with the principles above we have, in particular, that

$$\mathbf{HA}^* \vdash A \qquad \Leftarrow \qquad \mathbf{T} \vdash |A|^t_y.$$

Given that proofs in Peano arithmetic can be faithfully translated into proofs in Heyting arithmetic via the so-called negative translation $(\cdot)^N$, i.e.

$$\mathbf{PA} \vdash A \qquad \Leftrightarrow \qquad \mathbf{HA} \vdash A^N,$$

composing the two we obtain a functional interpretation of Peano arithmetic into $\mathbf{T}$.

Gödel's interpretation was quickly extended to analysis by Spector [5], by showing how countable choice can be given a dialectica interpretation into the system $\mathbf{T}$ extended with the schema of bar recursion. Bar recursion can be thought of as an extension of primitive recursion on the well-founded (linear) order of the natural numbers to recursion on arbitrary well-founded *trees*.

In this talk we will survey these classic results from a new perspective. Namely, we observe that when interpreting a classical theorem of the kind

$$A \equiv \exists x^X \forall y^Y P(x,y),$$

with $P(x,y)$ quantifier-free, the dialectica interpretation of (the negative translation of) A extracts from a classical proof of A a *selection function* $\varepsilon : (X \to Y) \to X$, in the sense of [2, 3], satisfying

$$|A|^\varepsilon_p \equiv P(\varepsilon p, p(\varepsilon p)).$$

Given the recently discovered connection between selection functions (and their product) and the calculation of optimal strategies in sequential games, we show how this gives new insight into how classical logic, arithmetic and analysis are given computational meaning via Gödel's dialectica interpretation.

During the talk, the interpretation of (1) pure classical logic will be illustrated by the interpretation of the drinker's paradox, (2) classical arithmetic will be illustrated by the principle of finite choice, and (3) classical analysis by the interpretation of full countable choice. Our running example will be that there is no injection from the Baire space to the natural numbers, i.e.

$$\forall H^{\mathbb{N}^{\mathbb{N}} \to \mathbb{N}} \exists f, g (f \neq g \wedge Hf = Hg).$$

Although the proof of this seems to involve classical logic and countable choice, at the end of the talk we will show how the dialectica interpretation extracts from this proof a program that computes f and g satisfying $f \neq g$ and $Hf = Hg$ for any given H (in models of $\mathbf{T}$ plus bar recursion).

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Solving equations by quadratures using Model Theory

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Some of the current research in Model Theory can be viewed as an extension of the principle of "solving equations by quadratures". I will illustrate this point of view in the case of expansions of the real field, where notions such as o-minimality and model completeness have led to the discovery of various so-called preparation theorems.

Martin's Conjecture and countable Borel equivalence relations

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While the last decade has seen some remarkable progress in the theory of countable Borel equivalence relations, many of the basic problems concerning universal countable Borel equivalence relations remain open. In this talk, by considering of some of the consequences of Martin's Conjecture on degree invariant Borel maps, I will offer some evidence that these problems are genuinely difficult.

Contributed Papers

Constructing a proof-tree: An investigation on composition of derivations

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The practice of composing fragments of derivations, which is necessarily implicitly exploited in the process of building a proof-tree, does not receive any formal treatment in the standard version neither of Sequent Calculus nor of Natural Deduction. Alternative versions of these systems will be proposed, where this practice is embedded into explicit operations of "substitution".

Approximating Beppo Levi's "principio di approssimazione"

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We try to recast in modern terms a choice principle conceived by Beppo Levi, who called it the Approximation Principle (AP, henceforth). Up to now, there almost was no discussion about Levi's contribution, due to the quite obscure formulation of it the author has chosen. We briefly reconstruct the historical and philosophical surroundings of Levi's proposal, and then propose our own interpretation of AP. This is discussed with respect to the supposed faithfulness to Levi's original intention. Finally, an application of AP to a property of metric spaces is presented, with the aim of showing how AP may work.

On constructive set theories with operations and related problems

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We consider constructive set theories with an additional (partial combinatory) structure of operations, which were investigated by the authors since 2004. This research parallels similar investigation on the classical side by Feferman and Jäger. In this paper we study extensions with operations, which internally represent description operators, unbounded set quantifiers, local fixed point operators, transitive closure and unique decoration of graphs. We investigate the proof theoretic strength of the resulting systems varying from that of Peano arithmetic to that of impredicative full second order arithmetic and the second order μ-calculus over arithmetic.

Lorenzen games for full intuitionistic linear logic

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This note discusses game semantics, in the style of Lorenzen, for Full Intuitionistic Linear Logic, following Blass and Rahman.

Copies of classical logic in intuitionistic logic

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Intuitionistic logic is obtained from classical logic by dropping the law of excluded middle. Intuitionistic logic is weaker than classical logic, but constructive. Surprisingly, classical logic can be embedded in intuitionistic logic by the so-called negative translations. Their images are copies of classical logic in intuitionistic logic.

All the negative translations usually found in the literature have the same image (modulo intuitionistic equivalence), that is the same copy of classical logic. This leads us to conjecture that the copy of classical logic is unique. We refute this conjecture and present other copies.

Properties of universes in realizability models for intuitionistic set theory and its corollaries

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We consider the universe of sets for models for intuitionistic set theory from [1] and [2] and proved that for every partial recursive function $(prf)f$ there exist the set x from Δ (our universe) so that the given $prff$ is not its function of extensionality. Corollary. There does not exist $prff$ such as it is the extensionality function for every set from our universe. We use the Corollary for extending of our result from [3] about independent uniformization from strong Church Thesis in intuitionististic set theory to the extensionality.

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On Kripke frames and arithmetical interpretations for QGL

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In 1976, Solovay [2] proved the arithmetized completeness theorem for a propositional modal logic GL of the provability. In 1984, Montagna [1] proved that QGL which is a natural extension of GL to the predicate modal logic is not Kripke complete and that Solovay's theorem does not hold for QGL. We prove that (1) the Kripke validity does not imply the validity according to arithmetical interpretations, (2) the intersection of these two validity does not characterize QGL, and (3) Montagna's conjecture does not hold for a particular case.

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Two-level version of sequential logic: completeness and consistency aspects

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Following step method of Kolmogorov and Markov, and ideas of sequential logic, we propose *non-axiomatic* two-level sequential (not formular) calculus M, representing provably complete and provably consistent formalization of Cantor's set theory. *Non-axiomatic* construction is very important here, because by virtue of paradoxes (Russell paradox, etc) and Gödel incompleteness theorems such formalization is impossible on *axiomatic way.*

This result is obtained by using of non-logical algorithmic undecidable (with the law of excluded middle) sequential calculus of Church Lambda-conversion and deductive sequential Gentzen constructions without postulated logic cut rule and with two postulated Lambda-cut rules, introduced by the first author. The proofs of completeness and consistency for M follow directly from the construction of the calculus M and its derivations.

A generalization of Dugundji theorem

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In 1918, Lewis proposed a hierarchy of modal systems with the intention of deal with the concept of strict implication, without proposing a semantics for these systems. But in 1940, Dugundji shows that no system between $S1$ and $S5$ of Lewis may be finitely valued.

However, with the emergence of non-classical modal logics, the theorem must be updated. Thus, we show that no system between $S1$ and $S5$, whose propositional fragment is between the Henkin's Implicative Calculus and Propositional Classical Calculus, can be characterized by finite matrices.

Dynamic justification logic

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Justification logic is an epistemic framework that provides a way to express explicit justifications for the agent's belief. Public announcement logic is a framework for reasoning about the belief change caused by public announcements. In this talk we present dynamic justification logics of public announcements that allows us to reason about evidence brought about and changed by public announcements. We provide axiomatizations and epistemic semantics for dynamic justification logics. Moreover, we study the belief dynamics of such logics and we elaborate on the relationship between the traditional modal logic approach and the justification logic approach to public announcements.

On Piaget-like monoids: Monoids for logics

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We introduce and examine an algebraic tool for analysing and comparing logics. This tool stems from some ideas introduced by Jean Piaget for classical propositional negation. Piaget-like monoids extend these ideas to the case of other unary symbols (e.g. modalities). These monoids provide condensed information about a logic, much as eigenvalues (or eigenvectors) give some information about matrices. As such, they can be employed for analysing, and to some extent, comparing logics. Comparing logics is not an easy task. We hope that Piaget-like monoids contribute to simplifying the task of comparing logics by using algebraic methods.

Semantics for Tarskian consequence operations

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In 1930 Tarski introduced the concept of consequence operation. It can be viewed as a syntactical concept. Following some ideas of S. Surma, we develop a general framework for different semantical systems. The key idea is to ascribe, via model mappings, to every set of formulas the set of its models. A sentence A follows from a set T iff every model of T is also a model of A. We give necessary and sufficient conditions which such a model mapping must satisfy, for being an adequate semantics for Tarskian consequence operations. Finally concepts of the theory of consequence operations are defined semantically.

A2. Philosophical Logic

Invited Symposium

General Proof Theory

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By the end of the last century Saunders Mac Lane wrote:

> So "proof" is the central issue in mathematics. There ought to be a vibrant specialty of "proof theory". There is a subject with this title, started by David Hilbert in his attempt to employ finitistic methods to prove the correctness of classical mathematics. This was used essentially by Gödel in his famous incompleteness theorem, carried on further by Gerhard Gentzen with his cut elimination theorem. In 1957, at a famous conference in Ithaca, proof theory was recognized as one of the four pillars of mathematical logic (along with model theory, recursion theory and set theory). But the resulting proof theory is far too narrow to be an adequate pillar;... [1]

General proof theory (the term is due to Dag Prawitz) should lead to the proof theory Mac Lane was looking for. It addresses the philosophically-looking question "What is a proof?" by dealing with technical questions related to normal forms of proofs, and in particular with the question of identity criteria for proofs. It follows Gentzen more than Gödel, and in doing that it deals with the structure of proofs, as exhibited for example by the Curry-Howard correspondence, rather than with their strength measured by ordinals.

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Abstracts

Proof theory and presentation of algebras

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Recent work of Matias Menni illuminates the problem posed by my 1994 Pontignano work on constructive proof theory via adjointness. The class of models considered there stems ultimately from Hans Lauchli's 1967 intuitionistic completeness result (which paradoxically is valued in a Boolean topos) as expanded

into the hyperdoctrines discussed in my 1967/68 lectures and publications. Anders Kock's 1970 work on proof bundles suggested the closer study of those special hyperdoctrines determined by an elementary topos E with the attributes of X forming the category E/X; the poset reflection PX of that category retracts adjointly by a Heyting homomorphism to the much smaller poset TX of subobjects of X. The latter is representable by an object omega of E itself; when does the former proof-theoretic power set P have an analogous representation? Essentially only in the very special cases similar to those considered by Lauchli, emphasizing his striking insight that nonetheless intuitionistic logic is faithfully represented even in such P. The E-constructive proofs (which are truly constructive in examples of E considered by Phil Mulry, Peter Freyd and others) are just the morphisms in the categories E/X; what they prove are entailments between predicates in $E(X, \Omega)$ or equivalently inclusions between the supporting subobjects in $T(X)$. The adjoint retraction $PX \to TX$, even in the general case where it cannot be internalized, can be collapsed to a equivalence by taking account of another important aspect of the practice of proving. Namely, the discovery of an arrow $A' \to B$ will suffice to prove that the image of A is contained in the image of B provided there is a covering $A' \to A$. The discovery of $A' \to A$ itself can be regarded as an analysis of the hypothesis A. Formally, this reduction of P modulo the further existence of spans of coverings, reduces Proof P to Truth T, in an illuminating way.

Remarks on recent categorical proof theory

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Category theory is a fundamental technical as well as conceptual framework for many branches of modern mathematics, and it enjoys increasing influence in theoretical physics and computer science. The role of category theory in the foundations of mathematics and proof theory goes back to seminal works of F. W. Lawvere and J. Lambek in the 1960s. Such early questions as "what are the mathematical structures of proofs" and "what is equality of proofs" continue to generate important foundational (as well as mathematical) questions. More recently, such subjects as linear logic, game semantics, geometry of interaction, and quantum computing have led to fundamental new structures in categorical proof theory, with considerable mathematical interest. We survey some recent developments.

Invited Lecture

The importance of compositionality

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The idea of compositionality is old but a precise formulation requires a few basic mathematical tools. Those tools are now readily available, but there is still some disagreement among linguists and philosophers of language about exactly what compositionality amounts to. Moreover, there is huge disagreement about its empirical and theoretical status, from the claim that it is a fundament for semantics, to the view that it is an empty requirement. (Computer scientists, on the other hand—the other major interest group in this context—take a more relaxed and pragmatic view of what compositionality is and what it is good for.) Almost everyone has a strong view about the notion, but the views vary wildly—an indication that something is amiss. After a couple of brief historical remarks, I will spell out the major relevant notions of compositionality in play. It is somewhat embarrassing that such basic groundwork is still necessary, but it seems to be, and without it one cannot hope to settle the issue of the importance of compositionality. I will take some steps towards such a settlement, touching upon aspects like: compositional vs. recursive semantics, mathematical facts about compositionality, compositionality and context (extra-linguistic and linguistic), compositionality and computation, compositionality and ambiguity, compositional translation, and compositionality and logical form. Most of what I have to say has been said before by others (and myself). If compositionality were an established scientific notion, repeating what is known would have been inappropriate on the present occasion. The fact that it actually is appropriate reflects, in my opinion, the present state of this notion in current linguistic and philosophical debate. My claim is that we now know enough to raise discussion about it to a level where metaphor and vague ideas give way to opinions based on theoretical and empirical facts.

Contributed Papers

An axiomatization of Paul Weingartner's 6-valued deontic logic and a result concerning its possible extensions

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To overcome difficulties, which appear when classical Modal and Deontic Logic are applied, Paul Weingartner developed new logics based on a 6-valued matrix system. Weingartner justifies his logics by applying them "successfully" to some of the—for the classical account—problematic cases. However, adequate proof theories are missing. We will provide such a proof theory for his Deontic Logic and reexamine his logic's (im?)plausibility. In addition, we will briey mention some of the consequences of our axiomatization and sketch how this method can be applied to Weingartner's Modal Logic RMQ and similar systems.

Localising logical rules in the sequent calculus

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An analysis of the sequent calculus rules by means of trivial instances of the cut rule obtains *local* logical rules. These local logical rules can be interpreted in two very distinct ways. When looking at them from a top-down point of view, they correspond to the (generalised) elimination and introduction rules of natural deduction, the calculus of presenting argument. When looking at them from bottom-up, they lead to a purely structural setting that might be considered as formal representation of pre-linguistic reasoning. This analysis recommends the sequent calculus as the centerpiece of formal reasoning.

Skeptical doubt, the common doubt and the contextualism of Keith De Rose

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In this paper I review briefly the contextualist proposal made by Keith Rose in response to skeptical doubt. Skeptical doubt face us to the impossibility of knowing anything about the world, De Rose rejected in his proposal that inability, at the expense of manage in contexts of knowlwedge with low epistemic standards (common knowledge). Notwithstanding the foregoing, in fact retain (and in the first instance, accept as true) the lock principle, contextualism is presented as a valid alternative against to skepticism.

Meinong and Husserl on existence

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This paper will analyze and compare the treatment of existential judgments—and therefore of the notion of existence—in Alexius Meinong and Edmund Husserl. Both philosophers inherited the problem of how to interpret existence from their common teacher Franz Brentano and tried—as will be argued—to provide a more refined account by deviating from Brentano's semantic model. Their work brings to bear on the contemporary debate over whether existence is a predicate and the related question of how to make sense of references to objects that don't exist.

Homotopies in classical and paraconsistent modal logics

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Topological semantics for modal logics has recently gained new momentum in many different branches of logic. In this paper, we will consider the topological semantics of both classical and paraconsistent modal logics. This work is a new step in the research program that focuses on paraconsistent systems from geometric and topological point of view. Here, we discuss the functional transformations in paraconsistent and classical modal cases: how to transform one classical or paraconsistent topological model to another, how to transform one transformation to another in a validity preserving way. Furthermore, we also suggest a measure to keep track of such change.

The consistency of Peano arithmetic: Why bother?

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Gödel's second incompleteness theorem should not worry us. Replacing classical logic by a suitable adaptive logic, PA (Peano Arithmetic) is turned into an adaptive theory APA. If PA is consistent, APA has exactly the same theorems as PA. If PA is inconsistent, the kinds of inconsistencies that are likely derivable by the lower limit logic have the form $\exists x\ x \neq x$ or $\exists x\ x = x'$. In view of this, APA will still be undecidable (in Gödel's sense) but all members of the standard block (the natural numbers) will behave as they were intended to behave by PA.

The bird of the hybrid type theory

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This paper brings together the work of four great names in the history of logic: Hans Reichenbach, Arthur Prior, Richard Montague, and Leon Henkin. Although the work of the first three of these authors have previously been combined, adding the ideas of Leon Henkin is the addition required to make the combination work at the logical level. But the present contribution does not focus on the underlying technicalities; rather it focuses on the underlying instruments and the way they work together.

Peano and Gödel

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The paper will take into account several remarks made in Gödel's manuscripts on Peano's introduction of the iota operator and its inverse, in order to evaluate the influence of the *Formulario* on Gödel's criticism of Russell's understanding of definite descriptions. In particular, the paper will discuss the understanding of an empty definite description as a null object (*Unding*), i.e. neither as a class, nor as a definite function, but rather as an individual concept.

Which constructive negation for falsificationism?

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To falsificationists such as Karl Popper, falsifying an empirical statement or a theory implies building a *conclusive disproof* of them, that is something that settles once and for all and negatively the issue of the truth-value of the statements. Falsificationist reasoning seems then to require a) a constructive notion of falsehood; b) a notion of non-constructive truth, to model the role of those theories that are provisionally accepted and not yet falsified. Here, we attempt at finding a framework where a) and b) are guaranteed and fulfil the needs of the falsificationist, while discussing Nelson's logic N3 and Connexive Logic, which both have constructive negations, and showing their inadequacy to our aims.

Modal dialogical logic, validity and universal satisfiability

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Modal Dialogical Logic provides a systematic account of the contextualized aspect in argumentation. We present an extension of modal dialogical semantics to give account for a further dimension on contextualization in argumentative debates, namely the possibility for argumentative parties to "shift" the context of the topic of argumentation. We show that this semantics coincides with the model-theoretical one for Universal Satisfiability ("truth at some world in every model", see Humberstone [1]). Our proof features a new way to present the systematic connection between the strategical level of dialogical games and (model-theoretical) validity.

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Neighborhood semantics for dynamic epistemic logics

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Two central topics in contemporary epistemic logic are *logical omniscience* and *epistemic dynamics* (how knowledge changes over time). However, almost no one has treated these two topics simultaneously. In the talk I will make a beginning with exactly this project. I will introduce and defend neighbourhood semantics as a particular solution to the logical omniscience problem. I will then show how to 'dynamify' this framework, thus obtaining a neighbourhood semantics for logics of public announcements (the best-known example of epistemic dynamics). I will discuss the technical and philosophical advantages of this system, and mention some further generalizations.

An elementary model theoretic perspective in mathematics education

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A main issue in mathematics education concerns the relationships between logic, language, proof and proving in mathematics. It is widely attested in literature that students face strong difficulties in using formalised mathematical language

in mathematics. A didactic study of these difficulties enlightens the fact that it is necessary in mathematics education to take into account not only syntax, but also semantics. In this communication, we aim to present some main results on implication, negation and quantification, relying on the logical semantic approach and to support by empirical results the relevance of the elementary model theoretic point of view for analysing proof and proving in mathematics in a didactic perspective.

Higher-order belief change in a branching-time setting

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While the standard work in logic for belief revision after [1] is cast in a syntactic-axiomatic and single-agent setting, recent developments in modal logic show how a semantic approach can give more insight in complicated belief revision scenarios that arise in a multi-agent setting. We consider here two different modal logics for belief change. We start with the branching-time temporal logic developed in [4] and extend this setting with ideas that arise from recent work in Dynamic Epistemic Logic (DEL) [2][3]. We motivate the extension of Bonanno's logic by considering multi-agent scenarios in which higher-order belief revision plays an important role.

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Cooperation in the prisoner's dilemma

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An analysis of the payoff matrix in a prisoner's dilemma (PD) shows that defection is the only rational choice for three reasons: it is the dominant strategy, it is the maximin strategy, and mutual defection is the only Nash equilibrium. Yet, the literature contains several attempts to explain how it could be rational to cooperate in a PD, especially when we iterate the game. We will show that even though it is right to assert that cooperation is a rational choice in these situations, this choice is not made within the payoff matrix of a PD.

Representation of Bolzano's content inferences with singular terms in the language of predicate logic

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In the paper the author describes his method of comparative historical analysis of the deductive conception of B. Bolzano (1781-1848). This work analyses the syllogistics with negative and singular terms. According to secularities of Bolzano the natural deduction system BS1 is built and all Aristotle's syllogistics rules and modus and additional rules for syllogistics formulas of singular terms are proved.

Three complications in modelling abduction in science

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In [3] the author presented the modal adaptive logic MLA^s which is very apt to model abductive reasoning processes of scientists investigating several independent hypotheses. However, some abductive reasoning processes have complications that this logic was not able to model. The three most important complications that could arise are insufficient implications, inconsistencies and priorities in the background knowledge. Still, several solutions to these complications have been developed in the adaptive logics program concerning other contexts and/or logics. In this talk, we will (1) adapt these ideas to the framework of the logic MLA^s; (2) explore to what extent it is possible to combine these solutions.

Representing a finite mind

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Logical models of a person's belief state or (more generally) mental state are necessarily highly idealized. In belief revision theory we have primarily used models where the individual has an infinite number of non-identical beliefs. The finitistic models that have been used (belief bases) require a somewhat arbitrary division of the individual's beliefs into basic and derived. Two new models are presented that are finite (in spite of a finite language) but do not require such a division, namely specified meet contraction and repertoire contraction. In specified meet contraction, the selection function is applied to the sentence to be contracted (and replaces it by another sentence), rather than the remainder set as in AGM theory. The basic assumption of repertoire contraction is that only some of the logically closed subsets of the original belief set are viable as contraction outcomes. Contraction takes the form of choosing directly among these viable outcomes, rather than among cognitively more far-fetched objects such as possible worlds or maximal consistent subsets of the original belief set. A close connection is shown to hold between repertoire contraction and specified meet contraction.

A statistical model of vagueness based on supervaluationism

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This study attempts to introduce a probabilistic aspect into the supervaluationist account of vague predicates. By thinking of precisification situations rather than precisifications, we can start to talk about "common" precsifications, which in turn allow us to use vague predicates bivalently. This model, called statistical model of vagueness, can handle the issue of higher order vagueness better than ordinary supervaluationism. I also introduce the idea of reductionist strategy to explain the incomplete nature of bivalent uses of vague predicates. These coceptual tools make supervaluationism more realistic.

Why blame Aristotle? Rational coherence and the principle of contradiction

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Some supporters of paraconsistent logic have attributed the widespread acceptance of the logical principle of (non-)contradiction to Aristotle. They consider this to be one more of Aristotle's many bad ideas. But we don't need to get

this principle from Aristotle, because the principle, properly understood, is obviously, evidently correct. I will appeal to principles from speech act theory and illocutionary logic to give the proper understanding of denial, negation, and the principle of contradiction. This provides the basis for arguing that it is irrational and incoherent to abandon the principle of contradiction.

The ways of modality: On the notion of higher-order modality

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In this paper, we examine the notion of higher-order modality by considering a few case examples where this modal phenomenon occurs. We claim that conventional possible worlds semantics is ill-suited for the expression of higher-order modality, and from there we show a way of generalising possible worlds semantics to repair the expressive deficiency. We finally introduce a new type of language interpreted in this semantics, and show how it naturally accommodates higher-order modal statements.

Information change and first-order dynamic logic

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Information change can be achieved by expansion, update and revision when dealing with new information and by eliminating impossibities and combining information when dealing with old information. This dynamic perspective on information provides a fruitful approach to problems in first-order modal logic.

Lost in translation: The logic of paradox

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We show that the Logic of Paradox (LP) [2] can be embedded in the modal logic $S5$ by way of a translation of the language of propositional logic into a fragment of the language of modal logic. On the basis of our translation, we argue that Slater [3] is right in claiming that LP's true contradictions are actually true subcontraries and that Lewis [1] hit the mark with his claim that LP can best be understood as a logic for ambiguity. Therefore, if we want a formal semantics and a philosophical interpretation of LP, we can completely rely on classical logic.

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Classical model existence and pure implicational logic

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In this talk we will present a new axiomatization of the pure implicational logic (i.e., the pure implicational calculus). This result is achieved by the technique in the paper [1] and the weak deduction theorem of BCI logic. We will also investigate the quantificational case of pure implication logic.

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Is the sensitive principle or the safety principle enough?

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What is knowledge? In recent years, Robert Nozick claimed that the sensitive principle could deal with the lucky problem of knowledge. Someone may think that the sensitive principle is enough for constituting knowledge. However this principle has set the standard too high to allow for our inductive knowledge. Ernest Sosa also tried his safety principle to solve the same problem. But we will find that this principle also has its own problems to face. We may safely form our beliefs, while at the same time we may not reflect our cognitive abilities. I attempt to explore the possibility of improving them and making them sufficient enough for satisfying the need of knowledge.

La logique des concepts paraconsistents

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Le but de ma communication est de montrer que les prédicats métathéoriques de consistance, inconsistance et paraconsistance peuvent être transférés, avec les modifications de rigueur, du niveau des théories à celui des concepts. Par conséquent, je parlerai de trois grandes catégories de concepts — concepts consistants, concepts inconsistants et concepts paraconsistants. Ce qui nous intéresse tout d'abord sont les concepts paraconsistants, mais, nous ne pouvons pas parler de la paraconsistance des concepts sans avoir une définition très claire du concept, en général, et de la consistance, respectivement, inconsistance des concepts, en particulier.

Explication as specific method of philosophical research

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This report purpose is, to considerate the subject of modern theoretical logic, essence and main stages of explication method and to represent this method as specific method of philosophical research.

On some natural four-valued generalizations of weak Kleene logic

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The aim of our paper is to determine several natural four-valued generalizations of the weak Kleene scheme, guided both by formal restrictions that the generalizations should satisfy and by the possible philosophical interpretations of the four values. We think that these logics can be applied to analyze languages in which two different sources of semantic deficiency are present.

Truth, dependence and supervaluation

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In [3], Leitgeb provides a theory of truth which is based on a theory of semantic dependence. We argue that the conceptual thrust of this approach

provides us with the best way of dealing with semantic paradoxes in manner that is acceptable to a classical logician. However, in investigating a problem raised at the end of [1], we discover that something is missing from Leitgeb's original definition. Moreover, we show that once the appropriate repairs have been made, the resultant definition is equivalent to a version of the supervaluation definition suggested in [2] and discussed in detail in [3].

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Adaptive deontic logics for various types of normative conicts

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In this paper, I shall first present a taxonomy of different kinds of normative conicts. Next, I shall discuss different strategies (and sufficiently rich languages) to deal with the different kinds of conicts and present general procedures to characterize the (monotonic) logics that are obtained by means of these strategies. Finally, I shall argue that these monotonic systems are too poor to deal with the different kinds of normative conicts, but that the adaptive versions based on them lead to satisfactory results.

On the variable-sharing property and the axiom mingle [1]

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A logic S has the "variable-sharing property" (vsp) if in any theorem of the form $A \to B$, A and B share at least a propositional variable. The axiom "mingle" (M) is the thesis $A \to (A \to A)$. According to Anderson and Belnap, the vsp is a necessary property of any relevance logic (cf. [1]). Now, Relevance Logic R plus M lacks the vsp. Therefore, Anderson and Belnap conclude that "relevance and mingle are incompatible when truth functions are added", [1, 98]. The aim of this paper is to show that Anderson and Belnap's conclusion must be qualified.

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Truth as a semantic switch

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I will combine Quine disquotational view about truth together with his idea of semantic ascent and argue for "a semantic switching theory of truth". In this theory, the truth predicate plays a role like a semantic switch. When we turn the switch on, the truth predicate will help us make a semantic ascent from using a sentence (on the first-level) to mentioning the sentence (on the second-level). When we turn the switch off, the truth predicate will help us make a semantic decent (or disquotation) from mentioning a sentence (on the second-level) back to using the sentence (on the first-level).

A partial modal semantics for the Adams logic of indicative conditionals

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Adams p-entailment provides a probabilistic extension of sentential logical consequence to simple indicative conditionals. Adams approach, taken together with David Lewis' *Triviality Results,* supports the view that indicative conditionals of the form $\varphi \Rightarrow \psi$ lack any truth-value (NTV thesis). In this paper, by providing a new partial modal semantics for sentential logic that encompasses Adams Logic, both the NTV thesis and the opposite view (according to which indicative conditionals always bear a truth-value) are questioned. Moreover, since every probability function P defined on the underlying algebraic structure satisfies the equation $P(\varphi \Rightarrow \psi) = P(\psi|\,\varphi)$ whenever $P(\varphi) > 0$, Lewis' *Triviality Results* are also challenged.

The paradoxical context of logical information: The core of the paradoxical context of information and inference

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The problem of the *logical information* has advanced the same slippery problems in the very case of philosophical logic: *treating logically, with logical means,*

real logic processes. A possible start point of a logical-philosophical approach to information and the seeking for the peculiar feature of logical information means to consider *information as information* only in the *very moment of its achievement as such* ; i.e. it supposes a receiver / addressee / end to turn it into a good account in some way. Therefore, this paper submits three basic statements : *(i) Cognitive and non-cognitive information exists only within the semiotically generative processes (semiosis), within signs/signals/symbols communication and transmitting processes ; (ii) As any information exists only by a codifying device or manner, it follows that any codification process is a continuous re-codification process and this fact modifies information (generating or annulling it, increasing or decreasing it) ; (iii) Information exists if and only if there is an information user.*

Modal analysis of strict implication

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In order to find a solution for the paradoxes of material implication, C. I. Lewis introduced a new constant in the vocabulary of propositional logic, namely, the constant named *strict implication.* This constant has the role to symbolize the relation of deduction between the premises and the conclusion of an inference. Starting from the Aristotle's analysis of inference, that the conclusion is necessarily deduced from premises, Lewis admits a modal interpretation for the strict implication. But, in this way, although the material implication paradoxes are eliminated, new paradoxes appear, namely, the paradoxes of strict implication. We can avoid this kind of paradoxes, admitting a different analysis of strict implication without a modal component. Strict implication can be reduced to material implication and the quantification of propositional variables.

Illocutionnary logic and social interaction: Speech acts and the conversational record

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The formal semantics of speech acts, even in the classical framework of illocutionary logic, requires considerations that go beyond individual speech activity and beyond the interpretation of individual sentences. We show how the formal semantics of speech acts should be extended to take into account the social effects and interactive aspects of illocutionary activity. To illustrate our point, we propose an account of assertions and descriptive discourse, conciliating the individual aspect of speaker's meaning and the epistemic effects of assertion making. This approach generalizes to all other types of illocutionary acts, adding specific content to the conversational record.

Strong antirealism, logical rules and structural rules

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According to semantic antirealism, intuitionistic logic satisfies the requirement that truth should be constrained by provability in principle. Some philosophers have argued that semantic antirealism must be committed to effective provability and thus lead to a stronger kind of logical revisionism exemplified by substructural logics. I shall look at two radical antirealist principles disqualifying structural rules: Token Preservation and Preservation of Local Feasibility. Against criticisms, I shall argue that conceptual support may be provided for both. There is a decisive difference between the rejection of classical logic via the curbing of the epistemic idealization embedded in structural rules and the rejection of classical logic via the criticism of invalid introduction and elimination rules. It will be explained why the second rejection is stronger.

The many dimensions of contextualism in epistemology

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Keith DeRose proposes a counterfactual account of knowledge and combines it with a contextualist semantics. In this paper, first, we give a formal model for DeRose's contextualist counterfactual account of knowledge, by taking it as a variation or augmentation of David Lewis's formal semantics for counterfactuals. Second, we extend our model by assigning two different functions to contexts: to determine the relevant epistemic standard and to specify the relevant similarity measure for ordering possible worlds. As a result, our model can deal with the objection that DeRose's contextualism fails to handle the genuine threat of skepticism.

On what grounds should we build deontic logic?

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The aim of the present paper is to provide an analysis of the syntax that should represent the behavior of deontic operators. We first present three arguments against the modal interpretation of deontic logic. These arguments will bring to light the two fundamental principles on which deontic logic should be built. We then propose a syntax that represents these principles and describe a semantic that will complete the system.

Constructive semantics and classical logic

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We scrutinize a constructive semantics proposed by Sandqvist [1], which justifies Peirce's rule, thus yielding a constructive justification of classical logic. This justification relies on bases composed of production rules only. Admissibility of Peirce's rule can be shown then, but not when bases comprise more than production rules. We argue that the restriction to production rules leads to a conflation of admissibility with derivability, and that this makes possible a constructive justification of classical logic. Arguing for natural deduction of minimal logic NM, a non-derivable but admissible rule like Peirce's would then have to be considered justified and NM to be incomplete.

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In the vicinity of Sette logic [1]

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Following [4], [3], [2], [1], we, first, define an infinite class of paralogics in the vicinity of Sette paraconsistent logic which was inducted by calculus P^1 [6], second, describe relations between these paralogics and properties of these paralogics, and, third, set up sequential axiomatizations for all paralogics studied here. Additionally, a method proposed by the second author is described that, for any paralogic in question, allows one to set up a Fitch-style natural deduction calculus which axiomatizes this paralogic.

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Non-Fregean logic and Ludwig Wittgenstein's early insight into application of logic

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The paper compares philosophical grounds of Roman Suszko's *non-Fregean* logic with Ludwig Wittgenstein's early insight into application of logic. The first section presents Suszko's inspiration for inventing sentential calculus with identity (SCI). Suszko's philosophical claims refer to Wittgenstein's *Tractatus.* The second section shows the role of the concept of application of logic in Wittgenstein's *Tractatus.* Application of logic cannot be anticipated by logic and thus identity sign expressing arbitrary interpretation of signs' representation is redundant in proper logical notation. The third section attempts to answer the following question: is Suszko's idea of logic contrary to Wittgenstein's idea of proper logical notation?

Proof-theoretic validity

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The idea of proof-theoretic validity originated in the work of Gerhard Gentzen, when he suggested that the meaning of each logical expression was encapsulated in its introduction-rules. Logical truths and consequences are deemed analytically valid by virtue of following from the meaning of the logical constants. But different logics are based on different rules, confer different meanings and so validate different consequences, some of which are arguably not valid at all. It follows that some analytic statements are false. The moral is that we must be careful what rules we adopt and what meanings we use our rules to determine.

The basic constructive logic for weak consistency in the ternary semantics with designated points [1]

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Given a logic S, a theory is *w-consistent* (consistent in a weak sense) iff it does not contain the negation of a theorem of S. In [1], the basic constructive logic adequate to w-consistency in the ternary relational semantics (*trs*) *without* a designated set of points as well as its extensions are defined. The aim of this paper is to carry out a similar investigation now in the trs *with* a set of designated points. That is, in the semantics for relevant logics defined by Routley and Meyer in the early seventies of the last century.

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Translation invariance as a criterion of likeness. An analysis of hybrid versimilitude theories

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The paper will present the general overview of hybrid theories of truthlikeness (verisimilitude), all of which can be considered as resulting from likeness-based and content-based approaches. Confronting the proposals of Hilpinen, Zwart and Niiniluoto with their most influential criticism, I will try to compare their views on likeness. This view will be dependent on many factors: the logical space considered, the approach followed (*e.g.* possible-world account *versus* consequence account) and the relation of the likeness itself to so called 'content'. After this short characterization I will proceed to the analysis of each theory from the perspective of satisfying the condition of translation invariance.

The BHK interpretation and extensions of NJ

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We consider the BHK interpretation and basic/non-basic extensions of natural deduction for intuitionistic logic NJ. For any basic extension there is a construction converting each proof of the premiss $\neg A \to (B \vee C)$ of Harrop's rule into a proof of its conclusion $(\neg A \to B) \vee (\neg A \to C)$. According to the BHK interpretation the Kreisel-Putnam formula $(\neg A \to (B \vee C)) \to ((\neg A \to B) \vee (\neg A \to C))$ is then assertable, whereas it is not derivable in any basic extension of NJ. For the non-basic extension to Heyting Arithmetic HA it can be shown that any closed instance of the Kreisel-Putnam formula is BHK-assertable, while not every such instance is derivable in HA. Therefore the BHK interpretation and NJ mismatch for basic as well as for non-basic extensions.

Logic as consequence in opposition

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An abstract operator of opposition is devised in this talk, following Tarski's abstract operator of consequence. It is claimed that, against a current view of logic as the theory of consequence, opposition is a basic logical concept that can be used to define consequence itself. This requires some substantial changes in the underlying framework, including: a non-Fregean semantics

of questions and answers, instead of the usual truth-conditional semantics; an extension of opposition as a n-ary relation between any structured propositions; a definition of oppositions in terms of basic negation. Objections to this claim will be reviewed.

The errors of Bertrand Russell

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Beyond his success as philosopher, logician or mathematician, especially by the theory of descriptions or by the axiomatic formal logic system from "Principia Mathematica", the philosophy of Bertrand Russell is marked by a series of errors, some of them elementary, but others of a major importance for the further development of the ideas in the 20^{th} century.

The present paper will present some of Bertrand Russell's errors and their impact on the philosophical problems in general.

Logic as an art and logic as a science: Is it only precedents or tradition?

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Nowadays the answer to the question "what is logic?" seems very simple and obvious—"logic is a science", and after that usually one says what is this science about. The expressions "logic is an art" or "the art of logic" are only metaphors or some kind of "*façon de parler*" used in serious scientific discourse. One of the my aims here is to trace a line of development of dichotomy "logic as an art—logic as a science" and to demonstrate that both these features of logic have fundamental historical roots and play very important conceptual role in any theorizing about logic.

Quine's other way out

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1 have pointed out in several places that the Fregean tradition mixed up predicates with the forms of sentences, and that it was this that led Frege into Russell's Paradox, through substituting the sentence form 'x is not a member of x' for 'F' in the naïve abstraction schema: $(\exists y)(x)(x \text{ is a member of } y \equiv Fx)$. But I have recently realised that there is another way of obtaining this conclusion using a standard feature of formal logic. For the substitution of the predicate 'is not a member of x' for 'F' would violate a formal restriction. The context is not free for 'x', i.e. the variable 'x' in that predicate would become bound by the quantifier '(x)'.

Semantic foundations for the logic of assent

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In the paper epistemic semantics for determining variants of the logic of assent are developed. These semantics presuppose neither the completeness nor the consistency of epistemic worlds. There will be developed a semantic system, which is an alternative to the confinement of classical semantics by the system of tautological entailments and by the first degree analytical implication (or Zinovev's strong logical entailment). This semantic system deviates from tautological entailments by confirming the disjunctive syllogism and from analytical implication by allowing the disjunction introduction in a restricted form, similar to the restriction of disjunctive syllogism in tautological entailments.

An adaptive approach to detachment in conditional logics of normality

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Conditional logics of normality (see e.g. [2, 3, 7, 4, 5]) offer an attractive formal approach to default reasoning, i.e. reasoning on the basis of what normally is the case.

In my talk I present an adaptive logic framework [1] that is able to enhance monotonic conditional logics of normality with the ability to perform default inferencing. The adaptive logics are able to apply detachment to a conditional $A \rightsquigarrow B$ and A defeasibly. The inference is retracted if there is a more specific conditional $C \rightsquigarrow \neg B$ and its antecedent C is the case. The various adaptive strategies are shown to correspond to different rationales, e.g., a different handling of so-called floating conclusions (see e.g. [6]).

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A quantitative logic of normality

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Logics of normality suggested so far take normality as typicality. Something is normally the case if it is true in the most typical circumstances. On the other hand one might think of normality as majority. In this sense something is normally the case if it is true in most circumstances. The first view is qualitative and the other view quantitative. The aim of the talk is to introduce a formal logic of quantitative normality and to point out the advantages and disadvantages of quantitative normality in comparison to qualitative approaches.

Some calculus of change with $S4$-necessity

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We add to the language of sentential logic an operator C ("It changes that...") and present an axiom system based on classical logic to catch the intuitive meaning of the term "change". A typical axiom is e.g. "CA implies Cnot-A", a basic rule is "From A you may infer not-CA" (theorems don't change). Furthermore we add $\Box$ for $S4$-necessity. We prove the completeness of this calculus in respect to a semantics where we introduce "stages" (in the development of some world, of convictions of an agent or in some argumentation) on which a sentence may be true or false.

Undecidability of some mereotopological structures

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In this paper, I will consider three possible domains: (1) the power set of R^2, (2) the set of regular open subsets of R^2 and (3) the set of semi-algebraic regular open subsets of R^2, and I will show that the theory of the mereotopological structure whose domain is among (1)~(3) is undecidable. The method in general is to try to interpret the intended model of first-order arithmetic into the mereotopological structure in question. However, the interpretations here will not be traditional ones, for the formal mereotopological language is not expressive enough to define the set of natural numbers.

The swamping problem

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When we discuss the value problem about knowledge, we need to resolve the primary value problem, the secondary value problem, and the tertiary value problem. Among those problems, Linda Zagzebski raised a special problem about the value of reliability, the swamping problem. She compared the case of coffee with our belief. Just like the value of a reliable machine being swamped by its product of a cup of good coffee, the value of a reliable process of producing a belief will be swamped eventually by the true belief. We will formulate a swamping argument which will deal with three intuitively acceptable claims but mutually incompatible with each others. The three claims include epistemic T-monism, the general thesis about value, and a very strong intuitive presupposition about knowledge.

The Ramsey test and Chellas-Segerberg semantics

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We investigate a possible worlds interpretation of the Ramsey test. For that purpose we describe a possible worlds semantics for conditionals, namely Chellas-Segerberg (CS) semantics, [2]; [5]; see also [7], which has largely been neglected in the literature. We aim to show that CS-semantics can—unlike the conditional logic semantics of Stalnaker [6] and Lewis [4]—be plausibly interpreted in terms of a minimal Ramsey test. We, furthermore, discuss in which way CS-semantics might serve as a minimal semantics for AGM belief revision [1] and dynamic doxastic logic [3].

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Busting a myth about Leśniewski and definitions

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A theory of definitions which places the eliminability and conservativeness requirements on definitions is usually called the standard theory. We examine a persistent myth which credits this theory to S. Leśniewski, a Polish logician. After a brief survey of its origins, we show that the myth is highly dubious. First, no place in Leśniewski's published or unpublished work is known where the standard conditions are discussed. Second, Leśniewski's own logical theories allow for creative definitions. Third, Leśniewski's celebrated 'rules of definition' lay merely syntactical restrictions on the form of definitions: they do not provide definitions with such metatheoretical requirements as eliminability or conservativeness. On the positive side, we explain how among the Polish logicians, in the 1920s and 30s, a study of these meta-theoretical conditions is more readily found in the works of J. Łukasiewicz and K. Ajdukiewicz.

Adaptive belief contraction

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In this paper, I explain Parikh's criterion for propositional relevance in the context of belief contraction [2]. In order to obey this relevance criterion, Kourousias and Makinson [1] propose a translation of the initial belief base into a canonical form. I argue why this leads to technical and philosophical problems. Finally, I present an adaptive logic that does not depend on such a translation and hence avoids these problems, yet still obeys the relevance criterion.

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Towards non-monotonic mathematics: Adaptive logic theories as a pragmatic foundation for mathematics

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I discuss the philosophical advantages of non-monotonic mathematical theories with an adaptive underlying logic. Such theories can characterize (most of)

classical mathematics but do not have the limitations expressed in Gödel's incompleteness theorems. The price to be paid is the non-semi-recursiveness of the discussed theories. This entails that the theories are not suitable as absolute foundations for mathematics. The dynamic proofs for adaptive logics warrant that the theories can nevertheless perfectly function as pragmatic foundations for mathematics. I argue that a pragmatic foundation is all we can hope for.

For a Popperian theory of conditionals

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The main theories of conditional logic share a same defect: from a conjunction, they allow one to infer a conditional. This thesis, named CS, is intuitively wrong. The explanation is that we generally interpret a conditional as putting a connection between its antecedent and its consequent. Illustrated by the Wason selection task, we can also note that this type of conditional allows contraposition. This entails first that we can directly falsify hypothetical reasoning. Secondly, since it needs two steps, the truth of a conditional is much more complex to establish, showing similarities with Popper's theory of demarcation, which differentiates verification and falsification.

Which core logic?

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This article aims to define the scientific standards that could be used to resolve the dispute on the philosophical question of knowing which is Core logic. It's a much more accurate question than the classical philosophical dispute for knowing which is the right logic: if there is a Core logic, it should be this one, i.e. the logic behind the systems that are logical extensions of Core logic. In this inquiry, we wonder about the virtues of intuitionistic relevant logic (IR), which is Core logic in Tennant's opinion.

Against classical dialetheism

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Classical dialetheism classifies all sentences into three categories: simply true, simply false, and both-true-and-false. It is this doctrine that I will argue against in the proposed paper. Based on a number of carefully selected truth-ineliminable sentences, I will contend that a classical dialetheist will have an insurmountable difficulty when deciding how to assign truth-values to these

truth-ineliminable sentences in such a way that the assignment will neither trivialize his or her dialetheist theory nor conflict with our intuitions about the truth-values of them.

Decidable many-valued logic for the application in empirical sciences

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The contribution discusses a many-valued propositional calculus RMQ and its application to domains of theoretical and empirical sciences. The aim of RMQ is to avoid difficulties and paradoxes due to superfluous redundancies, complexities and too strong assumptions permitted by classical two-valued propositional logic CPC. RMQ contains all CPC but distinguishes those valid principles which lead to difficulties in the application as only materially valid, from those which do not, as strictly valid. RMQ is decidable and has the finite model property. It contains a modal logic and can be extended with deontic operators.

Lorenzen dialogues and sequent calculus: Equivalence, correspondence, and cut

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We introduce sequent calculus inspired versions of Felscher's systems of Lorenzen-dialogue rules for the implicational fragment of intuitionistic propositional logic and use these versions (i) to prove the equivalence theorem for Lorenzen dialogues and the sequent calculus, (ii) to discuss the correspondence (i.e., image relations) between winning proponent strategies and sequent calculus proofs, as well as (iii) to define cut for the sequentstyle versions of Lorenzen dialogues and to establish cut-elimination for them.

Material implication v. mutually inverse implication

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The definition of material implication is both valuable and defective. Its merit lies in that it correctly reflects the establishment of sufficient but not necessary condition. It has a well-known defect: material implicational paradoxes. The author finds out that it has a less obvious but more serious defect: it cannot be used to make hypothetical inference. In order to inherit its merit, overcome its defects, the author proposes mutually inverse implication, which is composed of the mutually inverse processes of inductive composition and decomposition.

Contributed Symposia

Symposium

Hyperintension, intension, extension

Organizers: Marie Duži, Bjørn Jespersen, Pavel Materna

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The aim of this symposium is to explore the notion of hyperintensionality. This notion is anchored to foundational research in philosophical logic. One example is formal semantics and its research into topics like linguistic meaning, synonymy, reference, and expressive power. Another example is attitude logic and its research into topics like epistemic and doxastic closure, illogical attitudes, omniscience, and the flow of information within one agent or among several agents. The intention behind the symposium is to highlight the different reasons for opting for a hyperintensional framework in one's semantic, logical or philosophical pursuits and not least the different forms hyperintensional frameworks may assume.

In particular, we wish to investigate how, relative to a given framework, hyperintensions relate logically to intensions and extensions. This investigation presupposes in turn an investigation into what to make of the notions of intension and extension. The most straightforward approach to going hyperintensional would consist in adding a superstructure of hyperintensions onto an existing edifice of intensions and/or extensions. But it is not a matter of course that either intensions or extensions are to be preserved.

The overall purpose informing the symposium is to lend further impetus to the accelerated research into hyperintensions characteristic of various quarters of analytic philosophy. While the need for hyperintensionality is widely appreciated in logically oriented quarters, this is less so with the more informal portions of analytic philosophy, including philosophy of language and epistemology. It is our ambition that a strengthened focus on the need for a generally hyperintension-friendly approach will install the capacity to accommodate hyperintensions as a touchstone for philosophical theories of meaning and knowledge/attitudes/information. We see this symposium as an important move in that direction.

In the following we provide philosophical motivation for exploring the notion of hyperintensionality and some conceptual background.

The enterprise of philosophical logic has been through an extensional and an intensional phase. The former was marked by the efforts of Quine, Davidson and others to interpret all scientific, mathematical and logical language, as well as those fragments of natural language that were deemed reducible to such regimented languages, within a framework of extensional logic, which was the

first-order predicate calculus. This logic is well-defined and has well-known properties, but is much too restrictive for the purposes of most fragments of natural language and arguably also for various fragments of logical and other artificial languages. The intensional phase was ushered in by the arrival of possible-world semantics, which established itself as a respectable paradigm of formal semantics in the early 1960s. Much technical and philosophical mileage has since been had out of this intensional logic. Among its virtues were that it was continuous with the prevalent model-theoretic semantics, was able to account for contingency, the *de dicto/re* distinction, sets versus properties, truth-values versus propositions, etc., and provided a rigorous answer to Quine's challenge of how exactly to individuate intensions—namely up to necessary equivalence, equating necessary co-extensionality with co-intensionality. Needless to say, though, the possible-world conception of intensions left Quine and other advocates of a frugal logical ontology unimpressed, since possible worlds were invoked as functional arguments. Possible worlds were deemed too abstract and obscure.

However, since Carnap introduced his notion of intensional isomorphism, in 1947, it had been widely acknowledged that some logical objects needed to be finer individuated than up to logical equivalence. Cresswell, in 1975, defined negatively as 'hyperintensional' any individuation finer than logical equivalence: if A, B are hyperintensions, it is possible that A, B are necessarily co-extensional and yet distinct. Whereas possible-world intensions arguably fit most or all modal contexts, they are demonstrably too crude for explicit attitudes (i.e. those attitudes that are not deductively closed) and synonymy. The problem, in essence, is that what makes possible-world semantics into an extensional logic of intensions is its individuation of intensions (rather than the validation of the principles of extensional logic). Hence, formal theories of meaning and attitudes/information have seen the light of day. For instance, some hyperintensional theories are algebraic, like Bealer's and Zalta's, while others are procedural, like Tichý's, and others again remain within the world idiom, such as Hintikka's or Priest's, in order to model (para-) inconsistent beliefs, or turn to n-tuples for some notion of structure, like Cresswell's or Kaplan's. Explorations into hyperintensional logic have often run in tandem with the research paradigm of so-called structured meanings. However, the exact nature of the relationship between hyperintensions and structured meanings remains an open research question, and is one we would be happy to see addressed in our symposium.

Whereas possible-world semantics maintains an extensional principle of individuation of intensions, a possible-world-intensional approach such as Montague's is notorious for failing to validate various principles of extensional logic. Therefore, perhaps *the* fundamental question to raise when it comes to hyperintensions is whether it is feasible, or desirable, to aim for an extensional logic of hyperintensions, or whether the ambition to validate the principles of extensional logic also for hyperintensions is either unattainable or misguided. Thus, what are the prospects of preserving properties like the compositionality of meaning, the transparency of reference and sense relations, existential quantification into hyperintensional contexts, and extensionality as a criterion of individuation and substitution? And what is a *hyperintensional* context, anyway? Must the adoption of such contexts perhaps come with a semantics that alters the semantic properties of terms and expressions? Are there cardinality issues to look out for when adding hyperintensions to one's logical ontology?

Furthermore, we find ourselves confronted yet again with Quine's old challenge to intensional logic: What is the precise individuation of intensions? Should we perhaps adopt a range of hyperintensional criteria of individuation, tailored to particular contexts, rather than privileging one particular individuation? Research in this direction may revive an interest in, inter alia, Church's so-called Alternatives, which he defined relative to the logic of functions he invented (the lambda calculi). A function-based approach is potentially a fruitful paradigm of hyperintensional individuation, but ultimately just one among several options. Finally, a deeper perspective with potentially far-reaching consequences is that the quest for hyperintensions may exceed the bounds of model-theoretic semantics and set theory. Of course, it is always a technically feasible option to introduce hyperintensions, of various sorts (like hyperpropositions, hyperproperties, etc.), as (intra-theoretical) primitives, in which case a range of such primitives can be simply added to a model-theoretic structure. But as soon as one wishes to define and describe hyperintensions intra-theoretically, it is no longer obvious just how to go hyperintensional merely by means of structure-less entities like mappings or sets. The era of hyperintensionality may conceivably see a revival of structured, or complex or compound, logical entities (whereas the received view has tended to be that structure is syntactic structure only). Thus, the relationship between hyperintensions and structured logical objects adverted to above may turn out to run deep. But the hyperintensional quarters are as yet far from having arrived at anything like a consensus or a dominant view on this topic.

Contributions

- **Algorithmic intensionality and underspecification of verb-phrase ellipsis**

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- **Towards an extensional calculus of hyperintensions**

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- **Non-ideal epistemic spaces**

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- **Proposition, procedure, predication**

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- LOGICAL HYPERINTENSIONALITY: FROM CONSEQUENCES TO ACTIONS

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Symposium

THE MEANING OF AXIOMS: FROM MATHEMATICS TO LOGIC

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The standard conception of axiom is characterized by the conceptual and ontological priority assigned to the notion of mathematical structure. Starting from a certain «body of facts [*Tatsachenmaterial*]»([12] translated in [10]), composed by propositions, theorems and conjectures belonging to different mathematical systems, it is possible to single out some *invariants* that allow to identify the common features of these systems. In this process of *abstraction* a general and univocal abstract form is pointed out and the axioms fix, at the linguistic level, «what might be called a relational structure»[2, 497]. These structures will then play the rôle of the interpretational-structures (i.e. *models*) for the other sentences of the theory.

This process of abstraction is also connected to a *synthetic* moment: the axioms are required to capture all the relevant information belonging to a certain domain of discourse, in the sense that they should compactify and synthesize everything we know in some field of knowledge. [1] On the other hand, the *analytical* moment of the mathematical enterprise is represented by proofs: the information present in the axioms should be extracted and deployed just by the use of pure logical derivations, [22], [3], [11]: it is by deriving theorems from the axioms that the mathematicians study a certain class of structures [13].

Axioms have then a double rôle: they are, on the one hand, the points of entrance of the semantics into the syntax (i.e. they single out a class of models) and, on the other, the starting points of derivations. The fact that axioms connect semantics and syntax takes its formal characterization in the *soundness and completeness theorem* (for Hilbert-systems): $Ax_T \vDash \varphi \Leftrightarrow Ax_T \vdash \varphi$ (where Ax_T is the set of axioms of a certain theory T).

1. This aspect is strictly connected to the ideal of syntactical completeness.

Such a conception became the source of inspiration on which other fields of studies organized the construction of their theories. For example, (propositional) classical logic had been considered as the axiomatic theory complete for the class of Boolean-algebras, while (propositional) intuitionistic logic as the one whose structures correspond to Heyting-algebras. Also non-classical logics have been described following this idea; a neat example are modal logics, where to every (modal) axiom corresponds a frame-condition and *vice versa*[2] (cf. the so-called *correspondence theory*; [1]).

However, focusing on the correspondence between syntax and semantics established by completeness theorems leads to an impoverishment of the concept of proof; more precisely, the notion of proof is overcome by the purely algebraic relation of 'deducibility'. What counts is not the way in which a sentence is derived by the axioms, but just that it is derived or not. This conception creates an image of axioms as *static* entities: they play just the rôle of an explicit context of deduction. More prosaically, they are the (upper) borders of proofs and they don't seem to have any inferential and active rôle in proofs. This is clearly a too idealized image. In fact, even if the axioms don't enter into the body of proofs, their presence perturbates it. In particular, two problems emerge. On the one hand there is the loss of the "computational" and constructive properties such as normalization, cut-elimination and the disjunction and witness properties. On the other, axioms hide structural properties, such as contraction and weakening, that are obstacles to the reconstruction of proofs and their combinatorial analysis. The consequence is that axiomatic proofs don't provide sufficient informations useful for establishing interesting metamathematical results. For example, axiomatic proofs don't permit, in general, to extract a *computational content* from mathematical theorems (cf. Kreisel's unwinding program) nor to find decision procedures for certain classes of sentences.

A solution to these problems is possible if a *dynamical* character is assigned to the axioms. This implies a radical change of the standard notion of axiom.

A first way is to transform axioms into *postulates* or *principles of derivation* (*Erzeugungsprinzipien*; cf. [25], cited in [23, 199]). From a philosophical point of view, axioms are no more closed sentences used for describing structures but they represent specific types of mathematical reasoning, i.e. hypothetical types of actions that can be performed if certain conditions are satisfied. This is the solution proposed by S. Negri and J. von Plato. Axioms are no more considered as the starting point of proofs, but become part of them: they are transformed into inferential rules that can interact with the logical ones. In this manner the computational properties are maintained and the structural rules are shown to be admissible. This latter aspect is of particular interest because it permits to assign to proof-search methods a crucial rôle. First, it is possible to prove independence and decidability results ([21], [24]). Secondly, completeness theorems can be proved in a quasi-syntactic way, by constructing a counter-model from the non-terminating branches of a derivation (i.e. Schütte's method; see [20] for the case of modal logics). Thirdly, it is possible to operate a proof-analysis on axiom-like sentences so to detect which structures are singled out by those sentences. The standard relationship between axioms and structures is

2. Under the proviso that the frame condition is expressed in second-order logic. More precisely, the frame condition must be expressed by a Π^1_1-formula where the propositional matrix corresponds to the *standard translation* of the modal axiom and this translation takes modal formulas to first order formulas (see [1]).

therefore inverted. The primacy is no more assigned to structures; instead the contrary holds: it is by starting from proofs that structures can be identified.

A second solution is the so-called *deduction modulo*, [4]. Its basic idea is to transform axioms into (decidable) *rewrite rules*. In this way, the notions of axiom and of axiomatic theory radically change: an axiom becomes a rule for calculating, while an axiomatic theory is no more a coherent set of sentences, but it becomes an *algorithm*. Moreover, it is possible to maintain an epistemological distinction between the mathematical part of a proof and the logic-inferential one. By working with the usual logical (inferential) rules modulo the mathematical rewrite rules the constructive properties of proofs are saved; in particular, normalization is obtained via the construction of a model whose truth values are formed by the sets of proof-terms (i.e. *reducibility candidates*; [5]). This means that the semantical objects are built starting from syntactical proof-objects: structures are not something prior to the axioms but are constructed from proofs.

A third way is to extend the Curry-Howard paradigm to axiomatic theories. Roughly speaking, instead of changing the set of inferential rules or enlarging the set of types corresponding to a lambda-term, the idea consists in enriching the notion of proof-term. [3] First, the term-language is expanded with the so-called *stacks*, which represent counter-proofs. Thus, the usual lambda-terms are replaced by binary entities, the *realizers* (or processes), formed by a proof-term and a stack. Secondly, each axiom is associated to a particular set of realizers, i.e. a set of programs that behave in the same manner. This method is strictly connected to the so-called *Krivine's classical realizability*, which allows to give a computational content to axioms, without necessarily changing the usual classical logical setting into an intuitionistic one [15, 16]. This framework extends the set of syntactical objects in order to give them a semantical flavor as in Schütte's syntactical proof of completeness, where the non-terminating branches of a derivation allows the construction of a counter-model. More precisely, the notion of counter-model is replaced by that of counter-proof, which is in turn an extension of the notion of proof. The completeness theorem then becomes an *internal completeness theorem* because of this homogeneity between proofs and models: for every sentence (i.e. type) A and every proof (i.e. realizer) t, either t realizes A ($\vdash t : A$) or there exists a counter-proof p such that $p \Vdash A^{\perp\!\!\!\perp}$ and the normalization of t applied to p does not terminate (i.e. $tp \not\Vdash \perp\!\!\!\perp$).

Girard's *ludics* [7] takes a similar approach, with the difference that objects are no longer syntactical, as in realizability, but of mathematical nature. Ludics is a general framework for studying the notion of *interaction* between computational processes. In this setting, logic is no more considered as something primitive, but it emerges from specific ways of interaction between process. Even axioms are no more the point of departure of proofs and loose their primacy: in ludics there is an operation (the *daimon*) that allows to decide that an arbitrary sentence can close a derivation tree and therefore become an axiom, cf. [17, 5–6]. Nonetheless, ludics does not just reduce itself to a framework for reconstructing logic; it has applications in different fields. An interesting one is linguistics. In particular, a ludics-based approach to linguistics and formal grammars permits to model some features that axiom-based theories are

3. A famous example is the recursion operator of Gödel's system T; cf. [9] and [18].

not able to do in a satisfactory way (e.g. the notion of *dialogue*, *speech act* or *anaphora*; cf. [6].

To sum up, these three solutions seems to impose a radical change on the traditional notion of axiom when constructive or dynamical properties of proofs are taken as crucial aims. A fundamental point is to understand if this change is really indispensable or if it is possible to conceive alternative proposals that allow the coexistence of the traditional notion of axiom with constructive and dynamical approaches to mathematics.

Following the line of this summary, another aim of the symposium is to investigate the evolution of the notion of axiom in relation to other notions belonging to logical practice, as those of semantics and of proof. The leading idea is that, even if the notion of axiom seems to be unproblematic or already settled, nonetheless it still remains a critical notion in the contemporary philosophy of mathematics, at the crossroad of different fields of studies (as logic, mathematics, computer science and linguistics). Moreover, as we sketched, the reflection on the meaning of axioms seems to permit a reevaluation of a central question for the philosophy of logic: the relationship between syntax and semantics.

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Contributions

- Extending theories: Why and how ontology matters

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- **The impact of automated theorem proving on proof theory**

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- **Krivine's theory of classical realizability: A genuine alternative to the Brouwer-Heyting-Kolmogorov interpretation of proofs**

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- **Fitch's paradox in the light of structural proof theory**

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- **On the meaning of logical axioms**

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- **The social meaning of axioms**

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Symposium

New directions in dialogical logics

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Paul Lorenzen founded the tradition of dialogics in order to provide foundations to intuitionnistic logic, using dialogues, forty years ago, in [4].

From this root germinated various dialogical projects or programs related to dialogues, as different as the approach to argumentation theory by Erik Krabbe [1], who seeks to structure the logic of actual argumentations, building new links between logic and rhetoric, and the diametrically different program of dialogical logic [2], which uses formal dialogues to modelize, combine and study logics. These two conceptions delimit a broad field of applications, in constant growth.

In this symposium, we will explore the interface between argumentative practices and dialogic in some specific topics. The first point is to delve into the notion of meaning involved in the dialogical framework and to confront it with the antirealist claims—particularly in relation to some recent challenges by M. Marion [6] :

> There is a risk that any such "fairly uniform line of argument" might be jettisoned in a pluralist framework, where room would be made for other possible revisions of classical logic than the specific one advocated by intuitionists. This, however, would cause some problems for anyone wanting to put forth a successor program to Dummett's anti-realist challenge. In other words, what would happen of the arguments put forth by anti-realists in support of their (intuitionistic) revision of classical logic?

A good question is where exactly is the place of the antirealist approach to meaning in the dialogical framework and how does it relate to the dialogical approach to intuitionistic logic.

The second problem about meaning and argumentation is to test if and how the characteristic features of the dialogical framework offer a fruitful base to study actual argumentative practice such as argumentations involving vagueness. The guideline is that the dialogical framework can help us to explicit some implicit mechanisms; and gives original point of view on speech acts, like "making a claim", "concede", "reverse the burden of proof"... The second topic is about argumentation and ontological commitments. In particular the idea is to explore the scope of some recent work on the logic of fictionality that implements the dialogical meaning of quantifiers, where, to use Matthieu Fontaine's formulation in [1], to be is to be chosen. With our third topic, we will explore the relations between argumentation and Law. We will focus on the issue of conditional right [3], especially in the relation to the new project by Sébastien

1. For example, we can cite [3].
2. A good overview can be found in [2]
3. All the contracts of the kind "If you do something, I owe you something".

Magnier and Shahid Rahman (Lille): the study of conditional right in the context of Magnier's dialogical reconstruction of public announcement logic in [5] the dialogical approach to linear logic.

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Contributions

- Weak impermeabilism for fictionality in a dialogical framework

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- Completeness on dialogues and dynamic epistemic logic

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- Game semantics and the manifestation thesis

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- Conditional right and public announcement, which kind of dynamic?

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- **Advice on the logic of argument**

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Symposium

Proof theory, meaning and paradoxes

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The idea of characterizing the meaning of linguistic items and, in particular, of logical constants in terms of the role they play in inferential practices has lead to semantic pictures alternative to the truth-theoretic one developed by the Frege-Tarski tradition.

In spite of the efforts initially devoted by authors such as Michael Dummett [5, 4, 7] and Dag Prawitz [9, 10] to develop knock-out arguments against the rival view, the claims of a supposed superiority of inferentialist approaches over the truth-theoretic account of meaning are, as yet, far from being settled.

The aim of the proposed symposium is that of suggesting a different way of framing the contention. Namely, given a theoretical problem with which both approaches are expected to deal, does one of the two have more to say than the other?

Among many possibilities, we choose to engage the theme of semantic paradoxes. The choice is motivated by the belief that a reflection on the nature of paradoxes may prompt the inferentialist perspective to look for new research directions and that it may offer a fruitful ground on which inferentialism may be compared with the more traditional approach to semantics.

Paradoxes have accompanied the development of modern logic—and of the semantic theories built thereon—ever since its birth. In the inferentialist case, one could say that a paradox tied to such semantic picture arose even before the formulation of the semantics itself. So was the case when Arthur Prior [12] showed that to equate the meaning of a linguistic item with its inferential role could easily yield to the problems prototypically represented by the nowadays famous connective 'tonk'. The response was that several authors devoted efforts in order to give the inferentialist idea more solid basis.

Although half a century of work produced a family of related constraints (such as normalization, conservativeness, uniqueness, see among others, Belnap [2], Dummett [6] and Read [13] that the set of inference rules of a logical system should satisfy in order to avoid bad-behaving features, there is still no general

agreement on which constraints, if any, can be imposed on a given set of rules. The situation has indeed become worse due to the incredible development of proof-theoretic means of characterizing logical systems. However, although several types of formalism have been fruitfully developed, it is not always easy to formulate exactly corresponding criteria of meaningfulness in the different settings, in spite of equivalence results concerning the deducibility relations across the several systems. The result is that the attempt at characterizing what is meaningful risks of becoming a formalism-dependent issue.

Furthermore, the class of logics which have proof-theoretic significance has sensibly enlarged. Either more implicitly or in a more explicit manner, in every type of formalism it is possible to trace some parameters governing structural properties of the deducibility relation. The degree of freedom in tuning such parameters yielded the characterization of a wide variety of sub-structural logics. In the light of this, it is natural to ask whether the philosophical paradigm at the core of the proof-theoretic version of inferentialism, born before the 'substructural revolution', should be revised in order to account for such developments.

The contribution of Ole Hjortland to the symposium aims at clarifying the relationship between the sequent and natural deduction settings, by providing a general proof-theoretic semantic framework from which an original characterization of tonk-like operators can be given, thus offering a general analysis of what it is for a linguistic item to be, proof-theoretically speaking, paradoxical.

Several strategies have been proposed in order to cope with 'traditional' semantic paradoxes such as the Liar Paradox and its relatives in the truththeoretic tradition. Most of them focused on restrictions that a semantic theory should satisfy in order to avoid paradoxical consequences, typical examples of which are the restrictions on the possibility of fully displaying self-reference.

More recently, an alternative approach to paradoxes has been developed, aiming at an analysis of the principles needed to actually infer the paradoxical consequences from the semantic theory [14], [3],[11], [8], [1]. The pursue of this strategy has awaken the inferentialist interest in these more traditional semantic paradoxes. For, it turned out that in order to get paradoxes at all, crucial is the appeal to structural principles governing deducibility.

In his contribution, Luca Tranchini, considers in particular Neil Tennant's proposal [17, 18] of viewing the proof-theoretic counterparts of paradoxes such as the Liar or Curry's paradox as being characterized by the fact that, in a natural deduction formulation, the arguments leading to paradoxes fail to normalize.

Interestingly, Tennant's proposal looks like a bridge between the traditional conception of semantic paradoxes and the more typical proof-theoretic one inquired by Hjortland's contribution, namely the one raised by bad-behaving connectives. For, to request that valid arguments must normalize is indeed the most basic strategy to rule out connectives such as 'tonk' as unacceptable. Hence, from the proof-theoretic standoint advocated by Tennant, it could be argued that the paradoxical status of a connective such as 'tonk' is structurally analogous to the one of, say, a Liar-like sentence.

In his talk, Julien Murzi, will discuss recent views that focus on the role played by contraction in formalizing paradoxes. In particular, he will focus on one of the corollaries of Hartry Field's [8] truth-theoretic approach to paradoxes, according to which the validity of arguments cannot always be interpreted as the transmission of truth from the assumptions of an argument to its conclusion. Although this claim has been originally presented as induced by Curry's paradox,

Field has recently argued in its favor in a more general fashion, by presenting the irreducibility of validity to truth-transmission as a direct consequence of Gödel's second incompleteness theorem.

According to Schroeder-Heister [15, 16], one of the dogmas of standard semantics is the idea that the notion of logical consequence, which underlies the one of the validity of an argument, should be analyzed in terms of transmission, or preservation, of a more basic notion (truth, provability). Although from a different perspective, he also suggests, like Field, that the transmission view of consequence should be rejected.

In his contribution, Tranchini will try to apply this idea to Dummett and Prawitz's natural deduction based proof-theoretic approach. In particular, he argues that, by relaxing one of its fundamental assumptions—namely, that valid arguments must always normalize—an account of at least certain types of semantic paradoxes could be given.

On the other hand, Murzi's line of argument aims at casting doubts on the viability of the revisionary approach to the semantic paradoxes. Murzi will argue that validity paradoxes suggest that, in order to preserve consistency, or non-triviality, it is not sufficient to weaken the logic of connectives such as negation and implication. Rather, one needs to weaken the structural rules of the logic, i.e. the rules that characterize the structure of the consequence relation. In particular, Murzi will suggests that a natural way of revisionary logicians to cope with the validity paradoxes, and with the semantic paradoxes more generally, is to weaken the structural rule of contraction. In his contribution, Murzi will raise some worries about contraction-free logics. His criticism will focus on the the difficulties of interpreting as rational the inferential practices subject to contraction-free logics.

Without aiming at exhausting the theme, we hope that the symposium will at least suggest a few possible lines or research, along which a seemingly highly-promising field could be further developed. The talks will approach the theme from distinct perspectives: as a result, we hope that the complexity of the problems at stake could be best appreciated and that unexpected convergences may possibly be traced.

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Contributions

- **Proof-theoretic semantics in the substructural era**

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- **Validity and the unprovability of consistency**

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- **Proof-theoretic semantics, paradoxes and Dummett's fundamental assumption**

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A3. Logic and Computation

Invited Lectures

Homotopy type theory

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Homotopy type theory refers to a new interpretation of Martin-Löf's system of intensional, constructive type theory into abstract homotopy theory. Propositional equality is interpreted as homotopy and type isomorphism as homotopy equivalence. Logical constructions in type theory then correspond to homotopy-invariant constructions on spaces, while theorems and even proofs in the logical system inherit a homotopical meaning.

In parallel, Vladimir Voevodsky (IAS) has recently proposed a comprehensive, computational foundation for mathematics based on this homotopical interpretation of type theory. The Univalent Foundations Program posits a new "univalence axiom" relating propositional equality on the universe with homotopy equivalence of small types. The program is currently being implemented with the help of the automated proof assistant Coq.

This talk will survey some of these recent developments.

Coinduction and program extraction in computable analysis

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We describe a new approach to computable analysis based on coinductive definitions. Applying program extraction to proofs in this setting leads to representations of real numbers and continuous real functions by non-wellfounded trees, and to implementations of new algorithms in exact real arithmetic. We will also discuss ongoing work on an extension of this approach to a new coinductive model for computable functionals in all finite types.

How tightly close descriptive and computational complexity are: a personal view

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This talk will give a personal view of the close relationships of descriptive complexity, that is logical definability in finite structures, and computational complexity.

It is well known that, since its origin 75 years ago (1936), computation theory is intimately related to logic. This is particularly true for computational complexity, a theory (born in the sixtieth and seventieth) that tries to classify problems according to the machine resources—time and space, mainly—that their computations require.

Besides, it is natural to classify problems according to logics (first-order, second-order logics, etc.) that are used to express/define them: this is descriptive complexity theory. As Jörg Flum asserts, "what is surprising is how close the relationship between computational and descriptive complexity is. In fact, the most important classes of computational complexity have descriptive characterizations..." Typically, the famous class NP—with its celebrated NP-complete problems—equals the class of problems definable in existential second-order logic (ESO): this is Fagin's Theorem, 1974, abbreviated as NP = ESO.

In this talk, I shall argue that not only the main classical complexity classes (polynomial time, polynomial space, logarithmic space, etc.) are characterized by appropriate logics but also that, more accurately, precise time or space resources of algorithms - linear time, quadratic time, etc. - can often be syntactically characterized "exactly". Typically, for nondeterministic time $p(n) = O(n^d)$, the number of first-order variables is the degree d of the polynomial time bound p. This refines Fagin's Theorem (stated above) as $NTIME(O(n^d)) = ESO(dvariables)$.

Such equivalences constitute a strong argument for stating, as Immerman does: "time and space are not model-dependent engineering concepts, they are more fundamental." Such results also have the following interests and features:

- They show that the involved logical/computational complexity classes are robust (in the sense of Gurevich) and multiform concepts;
- The logics and logical resources involved also have several equivalent versions proved by normalization technics (such as Skolemization, etc.);
- The machines and computation resources involved have similar robustness properties. Typically, for *sequential* (resp. *parallel*) algorithms, several time complexity classes on *random access machines* (resp. *cellular automata*) are exactly characterized in terms of second-order logic with quantified *functions* (resp. Quantified *relations*).

The talk will illustrate and justify our personal view (that is: "precise logical resources = precise computational resources") by a number of results of the literature (including ones of the author) and conjectures and open problems.

Uniformization in automata theory

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A uniformization of a binary relation is a function whose graph is a subset of the relation. The uniformization problem asks, given a class C of relations and a class F of functions, whether each relation in C can be uniformized in F. A natural scenario where uniformization plays an important role arises in computer science where the relation describes an input/output specification. A uniformization of this relation then corresponds to an actual system that maps inputs to outputs such that the specification is satisfied. This talk addresses uniformization questions for relations and functions that are definable by finite automata or equivalently in monadic second-order logic over the domain of infinite words and trees, which are used to model the behavior of non-terminating systems. We present classical results and recent developments concerning the existence and computability of uniformizations in various settings.

Contributed Papers

Cut-elimination, substitution and normalisation

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We present a proof (of the non-trivial parts of which there is a detailed version, checked with the Isabelle proof assistant) that, for a $G3$-style calculus covering all of intuitionistic zero-order logic, with an associated term calculus, and with a particular SN and confluent system of cutreduction rules, every reduction step has as its natural deduction translation either an identity or a sequence of zero or more reduction steps (detour reductions, permutation reductions or simplifications). This substantially simplifies earlier work by (e.g.) Zucker [3] and Pottinger [2] on a question raised in 1971 by Kreisel [1].

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Logical properties of finite arithmetics

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We investigate families of finite, initial segments of standard models for various arithmetics. We give an axiomatization of the theory of sentences true in almost all finite models with addition. We also characterize its complete extensions and relate its infinite models to models of Presburger arithmetic. We estimate also the complexity of complete extensions of the arithmetic with addition and multiplication.

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Modal types and their procedural semantics for contextual computing

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Under the proofs-as-programs interpretation, issues of correctness and interaction can be commonly treated for both programming languages and knowledge representation: correctness relies on addressing structural procedures in terms of proof-checking on program derivations; interaction is based on contextual information processing and consists in providing metadata control on output. To unfold these problems, we present the role of modalities in type theories and show a procedural semantics where these notions are treated in terms of input accessibility via code mobility rules.

Gödel's incompleteness phenomenon from computational viewpoint

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For a better understanding of the phenomenon of incompleteness, first discovered by K. Gödel, we review the basic concepts of completeness and incompleteness from both syntactic and semantic standpoints. Once the distinction between completeness, incompleteness, incompletability, undecidability and essential undecidability of an axiomatizable theory has been clarified, we then present proofs for Gödel's and Rosser's incompleteness theorems with minimal use of logic but basing on the computability theory concepts. Relativizing the arguments to a definable oracle, we obtain Tarski's theorem on the undefinability of truth in arithmetic and computation theory. We get these new proofs of old results from translating some statements and techniques of computability theory into the study of logical theories.

Computing the infinite

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Computability Theory studies the notion of computability in principle [7]. We introduce a new notion of 'computable function' inspired by Nonstandard Analysis [2] and show that the new definition is equivalent to the classical one. Using techniques from Reverse Mathematics [6], we show that Nonstandard Analysis yields a 'more computable' framework for calculus than the notorious Weierstraß $\varepsilon - \delta$ method [3, 4, 5]. Furthermore, we demonstrate a concrete connection between Nonstandard Analysis and Bishop's constructive analysis [1].

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Computability theory in relation-algebraic form

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Using a small selection of primitive relations and set-theoretical operations, we give pleasingly simple and uniform inductive characterizations of four familiar classes of relations in a denumerable domain, including the recursively enumerable and the arithmetical relations. Perhaps most interesting is the treatment of recursively enumerable relations; to the author's knowledge, it is the first time this class of relations has been characterized in a direct, inductive way. The characterization is attractive not only in its formal simplicity *per se* but also in the simplifications it brings to proofs of standard results in computability theory.

Contributed Symposia

Symposium

Proof systems at the test of computer science: Foundational and applicational encounters

Organizers: Francesca Poggiolesi, Giuseppe Primiero

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For the 14th edition of the Logic, Methodology and Philosophy of Science Congress, to be held in Nancy, from 19th to 26th of July 2011, the special topic has been chosen to be "Logic and Science Facing the New Technologies". We consider this to be an ideal opportunity for addressing one of the most recent and challenging subjects in the area of logic: the link between proof systems and computer science.

The origins of this connection go back to the study of proof systems in constructive logics and their notion of mathematical proof. In this purely theoretical setting, the socalled CurryHoward isomorphism, establishing the identity between proofs and programs, represents the starting point for an extremely fruitful research area. Indeed, the number of topics that have been connected to this root is far more large and impressive than just its constructive flavor. Just to mention a few, consider issues such as the evolution of procedural semantics, the large number of typing systems, proof nets, extensions of the sequent calculus, the role of verification both for proof systems and hardware. The list can be easily extended. The most striking aspect of this evolution is certainly that proof systems have been shown to be central not only as logical tools, but also as the privileged way to study properties of programs in the area of computer science. As the latter represents the conceptual basis of many new technologies, it seems important to address the common background with formal logic and especially the prospects for future research.

The possible influences between the two areas are unbounded, ranging from foundational to computational aspects. Most recently, and especially from the community of computer scientists, a novel interest has arose for the mathematical foundations of the semantics of programming languages and in general for the study of formal systems, calculi, proof and type systems that help designing models of computation for the most new technologies, such as distributed networks, grid computing, security systems. The development of these computational models is a task that deeply invests philosophers devoted to logic, even under apparently different motivations. This is especially true in view of the connections of the same theoretical problems with other applicational aspects, such as those induced by modal and epistemic logics. Trying to address these topics is a crucial aim for both involved communities, the one originating from philosophical logic and that of computer scientists, for their evolution and reciprocal comprehension.

With this workshop we aim at improving such collaboration, and the 14$^{\text{th}}$ LMPS offers a great opportunity for such an encounter, both in for its current theme and for the general inspiring environment it offers. Following related events such as the "Proof Systems for Program Logics" Workshop at FLoC 2010 and the "Logic in Security" Workshop at ESSLLI 2010, and in line with the intense research on computability issues supported by the like of the Computability in Europe Association and the International Association on Computing and Philosophy, this Workshop might represent a successful anticipation to the large number of events that are planned in the coming years, starting from the Conference on History and Philosophy of Computing (710 November 2011, Ghent University) and preparing the great Turing Centennial (2012).

The workshop "Proof systems at the test of computer science: Foundational and applicational encounters" has three major goals.

1. The first goal is the collaboration and the exchange between highlevel young researchers and more experienced academics. This aspect is clearly reflected in the lineup of our invited speakers: distinguished professors such as F. Pfenning and H. Wansing can offer an exceptional opportunity of dialogue and a certainty of scientific value; younger researchers such as S. Kremer, and L. Strassburger (and also ourselves) will profit of this occasion to suggest new possible research trends;
2. The second goal is to offer, as far as this is possible, a complete panorama of the research developed in proof theory. More precisely, we have a double aim: on the one hand, we want to show some of the latest results in new developping logics; on the other hand, we want to focus on a deep prooftheoretical analysis of more standard logics. Also in this case, our program reflects this task, with talks delivered by the invited speakers basically falling into two categories. On the one hand, A. Ciabattoni, F. Pfenning, G. Restall and H. Wansing are going to deal with quite recent nonclassical logics. On the other hand, S. Kremer and L. Strassburger are going to investigate more standard logics, such as intuitionistic and classical logics, with new recent proof tools.
3. The third and perhaps most important goal of this workshop is to increase the dialogue between computer scientists and philosophers.To obtain such mix, we have carefully decided to mingle philosophers (G. Restall, H. Wansing) with computers scientists (A. Ciabattoni, S. Kremer, F. Pfenning, L. Strassburger) and we hope that the interaction between them and the participants (that we expect to be from both areas) can prove to be productive, fruitful and among the first steps for a solid bridge between philosophy and computer science. The desired mix is especially endorsed by the topics that will be delivered at the workshop: from the relation between the foundation of intuitionistic logic and interactive processes, to that between epistemic logic and security; from decidability problems to prooftheoretical interpretation of modalities; from the use of techniques from lambdacalculi, to those of category and graph theory for representing derivations and proofs. All these crucial themes under one or the other theoretical or applicational problems will surely be recognized as extremely interesting to all participants.

Finally, let us briefly explain some practical arrangements for a succesfull realization of the workshop. The workshop will be a long one of 150 minutes.

Each speaker will have 20 minutes for presentation, together with 5 minutes for brief discussion. We plan to have two sessions of three speakers each, with a pause in between. We realize the short amount of time dedicated to questions and answer between talks, but we see the opportunity LMPS offers to go beyond the dedicated slots and we believe that discussions and ideas will run long after the conclusion of the workshop. It is indeed our intention to seriously consider the possibility of bringing our efforts and the discussions of the workshop to a next level: should the event prove successful, we shall look into the possibility of collecting the talks in the form of papers and to have them presented as an edited special issue of a relevant journal or academic publisher.

As a way of summary, we desire to emphasize the importance of this workshop not only for its strong impact on the crossfertilization between different disciplines, but also for its purpose of helping the transfer of scientific knowledge and expertise between seniors and juniors, and finally for the broad range of subjects that it aims to broach. Besides all this contentual and formal reasons, we cannot avoid to stress the relevance of this workshop in the framework given by LMPS and its chosen theme for this edition: this workshop coud really be seen as a short, but deep, close examination of the very issue of 'logic and science facing the new technologies'.

Contributions

- **SAT in monadic Gödel logics: (un)decidability results and applications**

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- **An intuitionistic foundation for interactive computation**

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- **Epistemic logic and computer security**

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- **Towards a theory of proofs of classical logic**

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- **Encoding derivations in constructive logics**

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B. General Philosophy of Science

B1. Methodology and Scientific Reasoning

Invited Symposium

Symposium

Evolutionary models in epistemology and philosophy of science

Chair: Elliott Sober
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Abstracts

Probabilities, conditionals, laws

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We will present a new theory according to which it is possible to determine from a given probability measure, from a partition of propositions, and from a threshold number a logically closed set of conditionals that functions as a coarse-grained qualitative description of the quantitative information that is represented by the given measure. Depending on the kind of probability to which it is applied, applications of the theory lead to different outcomes: conditional belief if applied to subjective probabilities, counterfactuals if applied to single-case chances, approximate truths or truths under vagueness if applied to semantic probabilities, and finally normality laws ("normic laws") if applied to statistical probabilities. We use the latter application in order to throw some light on Gerhard Schurz' recent theory of normic laws in evolutionary systems. So rather than applying an evolutionary model in epistemology and philosophy of science, we will translate a theory that was motivated epistemologically—determining belief or acceptance from subjective probability—into one that has applications to evolutionary models, amongst others.

Evolution of signaling

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This paper discusses how conventional meaning can arise spontaneously in signaling games under simple adaptive dynamics.

Changing your spots

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This paper explores the relationship between two general types of dynamical processes, sometimes called "spontaneous ordering" processes, that can contribute to the formation of relatively cohesive communities. Segregation processes can explain how a population whose members can easily migrate tend to form relatively homogeneous subpopulations even when none of the individual members intends to move to a segregated community (Schelling 1978). Adaptive learning processes can explain how the members of a society adopt some new pattern of behavior or meme (Dawkins 1976). Segregation models and adaptive learning models have become important parts of social science in recent decades, but these two areas have developed somewhat independently. I believe this is because most of the segregation model literature tacitly assumes that individuals migrate according to personal characteristics or types they cannot change, whereas most of the adaptive learning model literature tacitly assumes that individuals cannot choose their interaction partners. In this paper I consider the consequences of explicitly enabling the members of populations to change both their types and their interaction partners. This approach explicitly integrates some of the best insights of the segregation and the adaptive learning programs. I will present models, including agent based computer models, of community formation based upon this integrative approach.

Invited Lectures

Philosophy of mathematics: Making a fresh start

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In the last century it has been generally assumed that the question of the nature of mathematics should be treated neither as a part of a general theory of knowledge nor in relation to our cognitive architectures. In this talk an alternative

approach is outlined, which takes into account the role of mathematics in human life and considers mathematics as a way of categorizing the world through which we make it compréhensible to us. From this viewpoint, the only mathematics which can exist is the mathematics our cognitive architectures permit us to make. This involves distinguishing between natural mathematics, which is a result of biological evolution, and artificial mathematics, which is a result of cultural evolution. What is more, it involves substantial changes in the traditional views concerning mathematical objects, demonstrations and definitions, which will be outlined in the talk.

Cause and chance

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I develop a framework for thinking about objective chance that combines elements of the propensity interpretation of probability, first introduced by Karl Popper, and the subjectivist account of chance developed by Mellor, Skyrms and Lewis. The framework will provide a much clearer metaphysical and formal foundation for a propensity theory of chance than exists at present. The account makes use of causal Bayes nets to represent the causal relations on which propensities depend. I will motivate the account with a suite of problems for both propensity and subjectivist accounts of chance. Some of these problems are familiar, and already have promising- looking solutions. Others are new, or at least under-appreciated. The problems do not, at first blush, appear to be related to one another; I will argue, however, that the problems are all inter-related, and offer an account that deals with all of them in a unified way.

The differences between data from simulations and experiments

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My talk will address one of the original philosophical issues about computer simulations: in what ways, if at all, do computer simulations differ from, scientific theories on the one hand and experiments on the other? Early in the debate, claims were made that simulations had some kind of intermediate status between theory and experiment, while standing as sui generis methods. More recently, broad claims have been made that simulations can be used in place of material experiments under certain circumstances. I shall explore three questions related to the core question from the perspective of data. The first is: Do arguments that have been advanced suggesting that the causal dynamics of simulation implementations is mirrored in the formal structure of the dynamic simulation model support the view that experimental data can be replaced by simulation data? The second is: Are the data produced by computer simulations

different in kind and in content from experimental and observational data, and also from data generated from traditional scientific or mathematical theories? If we have reasons to agree that they are different, that is one dimension along which simulations occupy a distinct scientific niche. The third question is: Is the degree of epistemic risk associated with inferences from simulations different from the degree of epistemic risk associated with experiments? In each case I shall show that more refined claims need to be made and that the epistemological issues do not lend themselves to an easy solution. We can bring to bear on the first of these questions considerations from the literature on computational theories of mind, in which simulations of cognitive processes are often considered to themselves count as examples of cognition. There are important differences between most scientific simulations and mental simulations, especially with regard to causal content, that make a general replacement thesis untenable for most scientific simulations. The second question can be approached by considering what is the motivation for insisting on empirical content in models. I argue that older arguments in favor of the view that empirical and theoretical content are inseparable are too unrefined and that it is easier in the case of simulations then in observation to invert processes that transform what is often called 'raw data' so as to separate the theoretical background from the data. Finally, I explore the role of background theories in justifying the use of output data from simulations and identify conditions under which such data can legitimately replace data from experiments, including the role of semi-empirical methods. The conclusions support the position that there is no genuine empirical novelty produced by simulations, unlike the case of some experiments, and that the important philosophical task is to identify the conditions under which simulation data can serve as an adequate replacement for empirical data.

Does the Brain 'Initiate' Freely Willed Processes? A Critique of Libet-Type Experiments and Their Interpretation

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In the extensive, recent debates on free will, the pioneering experiments by Benjamin Libet continue to play a significant role. The claim that these experiments demonstrate the illusoriness of freely willed actions is both strongly endorsed and hotly disputed. In this paper, we provide an analysis and evaluation of Libet's experiments from a philosophy of science perspective, which differs from the usual approaches in philosophy of mind or moral philosophy. Our analysis focuses on Libet's central notion of the 'initiation' of freely willed processes by the brain. First, we use the INUS theory and the manipulability theory of causation to investigate whether the experiments show any causal relationship between brain activity, on the one hand, and free decisions or (freely willed) motor activity, on the other. In addition, we examine three other interpretations of the notion of initiation (in terms of a necessary condition, a correlation and a regular succession). We argue that none of these four interpretations can be supported by the design and results of Libet's experiments. Significantly

enough, on the basis of these experiments we cannot even conclude that each free decision, or each (freely willed) motor action, is always preceded by a readiness potential. Furthermore, more recent Libet-type experiments cannot solve these problems either. Our general conclusion is that neither Libet's nor Libet-type experiments can justify the claim that the brain initiates freely willed processes.

A priori principles of reason

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Basically, A is a reason for B, if A speaks in favor of B, or makes B more plausible, or is positively relevant to B. And basically, a doxastic feature is *a priori,* if all rational doxastic states have that feature. The talk will unfold these notions. And it will argue, partially in a deductive way, for a series of a priori principles about the structure of reasons, starting with a basic empiricist principle (capturing the gist of the positivists' verifiability principle) and ending up with a weak principle of causality (which thus turns out to be *a priori*, after all).

Contributed Papers

Extended cognition meets epistemology

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In a recent paper, Duncan Pritchard argues that the thesis of extended cognition has important implications for epistemology. In the end, Pritchard maintains a compatibility between what he calls the "ability intuition" in epistemology and the thesis of extended mind. In this paper, I will question Pritchard's "ability" intuition for epistemology and will argue that extended cognition has nothing important to say about epistemology. The thesis that cognition (the mind) extends is a metaphysical thesis that has no consequence for epistemology, as far as I can tell.

A structuralist theory of belief revision

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The present paper aims to integrate the Sneed formalism known as the structuralist theory of science into belief revision theory. Among other things, this integration allows for a substantial simplification of the ranking information that is necessary to define revisions and contractions in a unique manner. In classical belief revision theory, some form of ranking is needed that orders any item of the belief set. Standard concepts to introduce this ranking are, e.g., the relation of epistemic entrenchment and Spohn's ordinal conditional functions. In the hybrid system of the present paper, by contrast, it is only theory-elements, i.e., pieces of background theories, that need to have a ranking. Our thesis is that epistemic ranking is an effect of theorising and hence requires, for it to be investigated, an analysis of how theorising governs our beliefs.

How can a purely cognitive philosophy of science deal with social biases? Embodied, situated and distributed cognition to the rescue!

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Should contemporary philosophy of science limits its object of study to epistemic/cognitive issues or should it be concerned with social conditions of scientific practice? Our answer to that question will depend on our answer to this other one: is scientific objectivity an exclusively cognitive issue or have social

aspects some role in it? I would like to propose that, under the embodied, situated, socially distributed approach to cognition, it is possible to approach some social aspects of scientific practice, e.g. gender and racial biases, without abandoning the idea that philosophy of science's object of study should be different from social and historical studies of science.

Cartography revisited: A key to understanding scientific knowledge

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Two apparently incongruent assertions are a source of controversy among philosophers of science: (1) Scientific arguments are either true or false, and (2) they assume presuppositions which are neither true nor false.

The cartography analogy can clarify and reconcile the two assertions. It draws a distinction between the rightness of maps and their methods of projections: maps can be right or wrong, while the methods of projection are rather useful or useless. The analogy intends to characterize the relationships between true or false scientific arguments and their presuppositions which are neither true nor false.

What makes an object 'epistemic'? Criteria of relevance for scientific collections and exhibitions

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Many universities afford scientific collections, which document former states and progresses of their particular research institutions. The storage and exhibition of material objects, which have formerly been instrumental in generating, modifying or representing a knowledge base ("epistemic objects") faces three conceptual problems: (1) The proper choice of objects; (2) their accurate conservation; (3) the adequate representation of the corresponding research institution. We will mainly focus on the first point in this paper and discuss exemplary criteria for the definition and choice of "epistemic objects" according to the requirements of scientific collections.

What objective probability could be

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Objective probability is commonly used in daily and scientific discourse, but usually taken as a given. A precise account of objective probability is provided by reduction to frequentism, but is only applicable to statistical physical

events. Objective probability as a primitive unnecessarily expands our metaphysics, without practical gain and verifying other than by frequency, and the view of a branching world allows only for subjective probabilities which are, in addition, unfalsifiable and arbitrary. Since subjective probability provides an adequate account of non-statistical and non-physical applications, it is proposed to understand objective probability only in above reductive and restricted sense.

A similarity based model of scientific concept formation

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In this paper I deal with the similarity based view of concept formation, developed by Thomas Kuhn in his mature work, [2], [3], [4]. Contrary to Andersen, Barker and Chen, [1], I try to show that dynamic frame model is not adequate to deal with Kuhn's view of concepts. Consequently, I will try to sketch another model. This is based on the concept of distance between objects, showing how similar two objects are. Based on this concept, I build a condition for two objects to belong to the same concepts. This formula can explain the appearance of anomalies.

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Why do we need Phenomena? What we can learn from the Engineering Sciences

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By means of a case-study, I will argue why authors such as Van Fraassen and Glymour miss out on crucial aspect of scientific reasoning in experimental practices when suggesting that phenomena are superfluous. Subsequently, I will

explain why Bogen and Woodward's (1980) ideas about the character of phenomena as independently existing things, properties or processes is in need of revision as the engineering sciences teach us that infinitely many phenomena can be discerned in principle. Finally, I will follow up on Hacking and Massimi by suggesting that *phenomena described by* $A \rightarrow B$ must be regarded as ontological entities 'carved-out' by us.

Theories of axonal transport in the cell: Empirical evidence against scientific realism

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The strongest argument for Scientific Realism is the No-Miracle-Argument, which states that the only plausible explanation for the success of a scientific theory is its (approximate) truth, whereas successfulness means that the theory correctly predicts novel results. Referring to two case studies from the history of cell biology, I want to raise two objections against this argument: First, what if a theory successfully predicts a novel result, yet the result later turns out to be an artifact? Second, what if a theory successfully predicts a novel result, yet the theory later turns out to be completely false?

A new logic for new technology

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I propose a new extension of logic to real, complex processes as appropriate for science, technology and their philosophies. This non-propositional Logic in Reality (LIR), grounded in quantum physics, is applicable at biological, cognitive and social levels of reality. I propose the paper for Section B1; LIR does not address issues in mathematics; proof theory; computer science or technology *per se*. It constitutes a conceptual tool for reasoning about interactive, contradictory aspects of real phenomena, *e.g.,* the ambivalence of technological progress. The principles and methodology of LIR thus constitute a new basis for an ethics of the evolving informational-technological society.

Theory success: Some evaluative clues

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The purpose of this work is twofold: to clarify the different ways in which theory success may occur, and to outline a criterion for the comparative evaluation of theory success. Empirical adequacy, predictive power, specification of causal

mechanisms, and explanatory unification will be regarded as the four main, not mutually exclusive kinds of theory success. From that basis, the traditional quantitative criterion for theory success—mainly revolving around the number of a theory's true empirical consequences—, will be refined by adding some key qualitative requirements specially focused on issues regarding a theory's scope and possible *adhocness*.

Mechanistic and neo-mechanistic accounts of causation: How Salmon already got (much of) it right

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In the last decade or so the mechanistic approach to causation has been undergoing a revival, with a number of mechanistic theories being put forward. Authors such as Machamer, [9], [10], Darden, [5], [6], [7], Craver, [3], [4], Glennan, [8], Bogen, [2], Bechtel and Abrahamsen, [1], and others, have been suggesting different definitions of "mechanism" and stressing the role this notion plays in the sciences. I argue that Wesley Salmon's contribution, [11], [12], [13], [14] to the development of the mechanistic approach has been underestimated by the current debate, and show how his works already present many hints towards what are now regarded as the crucial steps forward in dealing with mechanistic causation.

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Laws of nature and induction [1]

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The problem of induction is a problem of justifying the inference from the observed to the unobserved. Apparently, the regularity view of laws of nature, that laws are only regularities, does not answer to this problem—inductive scepticism is inevitable. Alternatively, it seems that David Armstrong's necessitarian view of laws of nature, [1], that laws are relations between universals, gives a rational answer to this problem. On the contrary, Helen Beebee,[2], argues that if the regularity view implies inductive scepticism, the necessitarian view also implies inductive scepticism. I will defend Armstrong's solution to the problem of induction against Beebee's arguments.

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The Epistemic Structural Realist Program. Some interference

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Epistemic Structural Realism (ESR), is the view that we are justified in believing in the equations of our best theories (Worrall, 1989). Such structural features seem immune from radical theoretical changes. Realists can on this grounds reject historically based scepticism. Knowledge is then limited to structure. Here I present ESR with a concern. Past scientific success traps ESR's advocates in a dilemma: either ESR defence of realism is not particularly structuralist; or a defence based merely on structure is not sufficient to support realism. The case of the prediction of the Normal Zeeman Effect is the background for the analysis.

Pluralistic ontologies for scientific realism

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Scientific realists generally commit to the existence of a mind-independent world, literal interpretations of scientific claims, and knowledge of unobservable entities. Of these commitments, the notion of mind independence is the least well developed. I elaborate an account of mind independence for the realist, recognizing that so far as scientific taxonomy is concerned, there is likely no one correct way to carve nature at its joints, but proposing an objective basis for taxonomy nonetheless. This suggests three forms pluralism, concerning: the scientific "packaging" of properties into entities; the precise metaphysical natures of these entities; the context relativity of their behaviour.

Neither between nor within: Selfhood and otherness in epistemology

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The aim of this study is to propose an epistemological integration of the philosophical traditions of Contructivism and Systemics, and of the physical traditions of Quantum Mechanics and Modern Thermodynamics. All this theoretical viewpoints subsume a revolutionary definition of truth and reality, of time and space. I hypothesize they issue three main challenges we must face with: (I) a radical construct of the observed reality as the autopoietic space of an observer; (II) a systemic integration of any observer and relationship through the phenomenon of the entanglement; (III) an assumption of irreversibility in the construction of any process and state.

A theoretical analogy: How is Darwin's theory of natural selection analogous to Malthus' theory of population

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This paper suggests a concept of "theoretical analogy" to account the relation between Robert Malthus' theory of population and Charles Darwin's theory of evolution by natural selection. A theoretical analogy means an analogous relation between two theories and the theories are understood as a population of models. It can be further analyzed into the six aspects: conceptually structural similarity, organized structural similarity, material analogy or analogous inference, application of abstraction, heterogeneous extrapolation, and prototypical role. I will also show how Darwin discovered the theory of natural selection from Malthus' theory by applying the theoretical analogy.

Dispositions, conditionals, and ordinary conditions

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The notion of disposition is basic and important in metaphysics and the philosophy of science, but how to understand it properly remains a daunting challenge for contemporary philosophers. Choi, [1], [2], [3], [4], [5], has recently made a vigorous and systematic attempt to defend a conditional analysis of dispositions which he claims to be adequate. The aim of this paper is to show that, contrary to Choi's claim, his analysis is deficient.

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Higher- and lower-level phenomena: A nonhierarchical approach to fundamental properties

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Nowadays, a striking feature of the scientific representation of reality is the use of the metaphor of levels. Talk of levels is no longer confined to philosophers. Biologists, psychologists, anthropologists, historians, etc. routinely appeal to higher- and lower-level phenomena in discussions on a variety of topics. Reality, it is widely presumed, is hierarchical, and given that everything is physical, all entities, properties, relations, and facts which are studied by the sciences may be reduced to a fundamental physical level. Scientist may not all agree with the spirit of Rutherford's oft-quoted remark that "there is physics; and there is stamp-collecting", but they all grant physical science the authority to tell us what there is, [1].

It will be argued here that, on the contrary, this line of thought has deeply confused us in matters relating to fundamental properties. Following Ladyman & Ross, [2] I put forward an argument designed to reinforce a non-hierarchical approach to the problem, and to illustrate that the thesis positing a fundamental level might be undermined by an application of a 'Ramseyan Humility' strategy, [3].

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Accumulation of theory parts and meaning variance

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In advanced scientific disciplines theories are underpinned by conceptual networks that are much too tightly entangled for synchronic partition to be even available. Science critics thus charge that progressive interpretations of scientific theories are hopelessly naïve, even for the most empirically successful theories. The proposed investigation addresses the question of what "theory-parts" of epistemic significance, if anything, accumulate through theory-change. A notion of "local meaning", arguably suitable for inter-theoretic dialogue and of interest to realist perspectives, is lifted from scientific practice.

What do we learn from case studies?

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There are two opposed positions about case study: we may either insist on the alethical and theoretical weaknesses, or we reduce deictic thought to an illustrative, educational even purely rhetoric role. To get away from this false alternative, I shall try to show what we learn from case studies: the essential and specific cognitive value of deictic thought is due to its contextualized narrative density. It's a fertile scientific mode of investigation both from the result's point of view (with regard to the very problem) and from the process' point of view (virtuous exercise of cognitive and emotional capacities).

On the ontology of linguistic frameworks: Toward a comprehensive version of empiricism

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While most empiricists regarded the *designation* of the abstract entities as the first step toward Platonism, in 1956 paper Carnap tried to comprise the existence of a language *referring* to abstract entities of science with empiricism. In executing his plan however, he finally slipped toward nominalism and embraced it more wholeheartedly than any other empiricist dead or alive.

But still I believe that Carnap's original program could be repaired. Moreover I believe that it is best to borrow the cement for the necessary reparation of the main structure from no one else but the notorious antagonist himself—antagonists in the eyes of empiricists, i.e., Meinong.

Therefore my efforts in this paper are aimed at achieving two goals.

Firstly, building on Carnap's strategic scheme, I am after presenting a comprehensive empiricist philosophy based upon the necessary ontological basis, by making a synthesis between empiricist views of Carnap and Meinong.

Secondly in the way to achieve the first goal and as a decisive step toward it, my effort would be directed toward presenting an empiricist interpretation of Meinong's philosophy. In this vein I will highlight the differences of my interpretation from the Platonist defenders and other commentators of Meinong.

A Bayesian model of no alternative arguments

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In the absence of empirical confirmation, scientists often resort to non-empirical strategies of theory assessment in order to enhance trust in their theories' empirical viability. A main strategy of that kind is based on the observation that

no-one has found an alternative to a proposed theory so far. We construct a Bayesian model to show that the observation of a lack of alternatives indeed constitutes confirmation of the one available theory under certain conditions. The talk is based on joint work with Stephan Hartmann (Tilburg).

Idealization, scientific modeling and simulations: A new analysis of idealization as a common framework for the study of models and simulations

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This is a new account of the idealization-concretization method in natural sciences, based on a counterfactual analysis of idealized statements (scientific laws) in which the notion of "degree of contingency" becomes very important (see [3]). Our account reveals to be similar to other recent and interesting proposals of analysis of scientific laws (see, in particular, [2], [1]). The novelty of our approach lies in the fact that it is an analysis of the concept of idealization as well as an explanation of its function in scientific methodology, model construction and simulation.

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Scientist vs. nature— priors, strategies and discovery

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Traditionally, the Bayesian paradigm in philosophy of science is only seen as providing an account of how new evidence confirms or disconfirms different hypotheses. It is prescriptive of what a Scientist ought to believe given incoming evidence. While there is substantial debate concerning the Scientist's starting point in this process—the choice of appropriate prior—the literature has been silent on how the Scientist ought to proceed in choosing the next source of evidence, e.g. a new experiment. We addresses this question by modeling discovery as a game the Scientist plays against Nature, in which the Scientist attempts to extract Nature's secrets.

Expanding our grasp: Can causal knowledge save realism from Stanford's new induction?

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Anjan Chakravartty has recently sketched a realist reply to Kyle Stanford's *new induction.* I discuss two problems that might affect Chakravartty's strategy. First, it might be too similar to previous (unsuccessful) attempts to block antirealist inductions, relying, as it does, on causal reasoning. Second, the modest realism it generates might not really go beyond the commitments that Stanford himself would accept. Against these two objections, I argue that Chakravartty's strategy does indeed allow us to be realists about domains of inquiry which Stanford considers to be beyond our grasp.

The stabilizing role of material structure in scientific practice

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Knowledge of the environment is essential for the survival of organisms. But such knowledge would be almost useless if it were ephemeral. The aim of this article is to analyze the various strategies for stabilizing human knowledge, with a special focus on its material anchors and their interactions with other stabilization means. Our hypothesis is that the role of material anchors in stabilizing conceptual blends is analogous to that of technology in grounding scientific knowledge. In both cases the material base constitutes a tool for consolidating knowledge, being the key point the connection with other elements involved in cognitive processes.

How to defend scientific realism against the PMI

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I defend scientific realism (that empirically successful theories are approximately true) against the pessimistic meta-induction which states that the history of science is full of counterexamples to scientific realism, i.e., theories that were once empirically successful but later refuted. I develop a counterstrategy against the pessimistic meta-induction using a notion of graded success of scientific theories. With its help, I argue for the claim that our current best theories enjoy far higher degrees of success than any of the refuted theories found in the history of science. I do so by examining "indicators of success" such as the amount of scientific work done by scientists, computing power, and the quantity, diversity, and precision of data.

Identity of scientific concepts and theoretical dependence

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The aim of this study is to analyze what conditions a law of a theory T must satisfy in order to contribute to the conceptual identity (or meaning) of a T-dependent term. With that aim I will focus on the difficulties of the proposals of Carnap, [1], [2], Lewis, [3], and Papineau,[4] about theory-dependent terms. I will advocate the idea that only some few laws of a theory T contribute to the meaning of a T-dependent term. Finally, I will consider which criteria a law of a theory T has to satisfy in order to contribute to the meaning of a T-dependent term.

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A problem for semantic definitions of analyticity

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According to the usual distinction between syntax, semantics and pragmatics, no semantic definition of analyticity uses pragmatic concepts like 'observation term' or 'theoretical term'. In this contribution we are going to show that a very weak semantic conception of analyticity, which seems to be included in many other conceptions of analyticity, is inadequate. For this purpose we give a method for transforming theories with a fully synthetic empirical basis into logical equivalent theories with an analytic empirical basis. We draw the following conclusion: If any definition of analyticity is adequate at all, then it is a pragmatic one.

On emergence and causation

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The underlying aim of this presentation is to defend the relation of emergence by means of a comparison to the relation of causation, in order to uncover their common rational ground. The concept of emergence is analyzed and its systemic and formal aspects are surveyed, followed by an introduction of a different type of rationality, here dubbed 'soft' rationality. The introduction of this type of rationality and its reasoning practices will reveal a new understanding regarding the shared rational foundation of the two relations.

Narrative explanations

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Narratives or stories are found in many disciplines, including history, the social sciences, evolutionary biology and psychology, and of course the writing of literary fictions. Stories provide many types of explanations of the events that occur in them. I am going to concentrate on one important type of explanation that stories often provide: explanations of why the conclusion of the story occurred, or in other words, narrative explanations of the story's conclusion. I shall distinguish between narrative why-explanations and standard why-explanations and then argue that narrative why-explanations are often irreplaceable by standard why-explanations of equal explanatory strength.

Reasoning without language or logic

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This talk proposes an explanation on how reasoning may work on an intuitive level (as it may be the case for superior animals) before the use of language and logic. We exhibit some processes that allow for the acquisition of mental representations of our environment. Our main contribution is: unlike proofs where hypothesis and goals have the same nature, actions and intentions are not comparable but have mirror properties. This might explain how our mind can select and refine "pertinent" notions so that solving a problem is as straightforward as possible.

Determining causal relevancies at event-level

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Causal graph theory is intended to determine the underlying causal structure of a system on the basis of an empirical probability distribution. The inferred causal structure is, however, always at a very abstract level (variable-level). Without further information it seems that this abstract structure reveals not very much of the causal relevancies of specific instantiations of variables for each other (event-level). In my presentation I will illuminate the connection between causal variable-level and event-level graphs by presenting a method of how causal relevancies at event-level can be uniquely determined on the basis of causal relevancies at variable-level.

How international comparisons transform social reality

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This paper is based on methodological difficulties faced in experiences with EU-funded research: many classifications do not fit the objects and actual issues are not measured or observed. First, we propose to experiment new methodologies to make large international comparisons without overlooking specific contexts and effective tools to analyze large amounts of data, taking into account translation problems and mixing various data: qualitative, quantitative, first-hand and second-hand, statistical, from case studies, etc. Second, as scientific governance is based on such comparisons outcomes, we explore how rethinking methodology for comparative science studies could lead to rethinking the governance of science itself.

The logic of surprise: Puzzle, quantum games and information

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This work is directed towards a systematic study of surprise, wonder, admiration and puzzles within the context of logic, epistemology and philosophy. This study also involves important philosophical concepts such as information, game, problem, reasoning, knowledge and ignorance. The logical and philosophical analysis about information theory will be firstly based on the work of Jaakko Hintikka, Yehoshua Bar-Hillel and Rudolf Carnap [5], [2] and [1], on the semantics of information, and on the text of Walter A. Carnielli, Marcelo E. Coniglio and João Marcos [3] on the logics of formal inconsistency. In this context, the goal is

to identify the logical spectrum underlying an informative scenario. After this step, we propose a semantic information theory with non-classical constituents. We will investigate the idea that part of the puzzle theory is an instance of the game theory on non-classical scenarios and the relevance of such study for philosophy, in particular to epistemology. The approach between game theory, logic and non-classical information theory will be developed from the approach of Daniele Mundici [6] on Ulam's games. With the intention to illustrate the perspective that deals with the use of elements of the game theory in non-classical context we also investigate quantum game theory exemplified by the work of E. W. Piotrowski and J. Sladkowski [7], which provides an introduction to quantum games, as well as the work of Kay-Yut Chen and Tad Hogg [4] that shows results of simulated quantum games obtained in laboratory.

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The principal principle, and theories of chance: An account of primitive conditional chance

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David Lewis's Principal Principle is perhaps one of the most important contributions to a philosophical understanding of the notion of object chance. However, the original principle is awed, forcing a modification which turns out to be quite significant. This New Principle, however, is not without its own problems. Here, I respond to a particular challenge to this New Principle, developing along the way a notion of primitive conditional chance.

Reasons for relativism: Feyerabend on early Greek thought

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This paper argues that the radicalization of Feyerabend's philosophy, namely his defense of more and more relativistic views on science are due to his research on early Greek thought. Feyerabend's research in the so-called transition from myth to reason, which was taken to be the origin of Western scientific thought, resulted in his conviction, that Western science is merely an incommensurable tradition among others.

Metaphors, the solar system and scientific research

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Sometimes, metaphors can be a source of ideas and thence of knowledge. In the past, imagination and scientific intuition, when combined with a broad cultural background, have proven extremely productive. Our understanding of the celestial mechanics perhaps represents a good historical "paradigm" of how enriching this combination can be.

Here I will show a few examples of this creative approach at work, in current research on planets and asteroids. These tools are also changing the way in which scientific discovery is brought to students and the general public.

The ultimate argument against convergent realism and structural realism: The impasse objection

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For the sake of argument, two assumptions are conceded to convergent realism. First, a theory space with a metric can be defined containing the relevant sequence of theories. Second, the convergence of this sequence can be diagnosed on the basis of a finite number of elements. The impasse objection states that the limit theory may be substantially different from the true theory. This objection also hits structural realists who base their realism on the stability of structure in the sequence.

Theory vs. interpretation: From a methodological point of view

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Debates on the interpretation of quantum mechanics have shown that the interpretation of a scientific theory cannot be unambiguously deduced from the theory. The nature of interpretations obviously differs from that of theories. There have been, however, only a few attempts to point out the difference. In the present paper, I address this question: I demarcate theories from interpretations, and develop a methodological framework for the latter in realistic terms. Thereafter, I demonstrate how the methodology works by applying it to interpretations of quantum theory. I shall also show that the methodological considerations shed some light on the interpretation debate.

Collective science: The loss of scientific understanding?

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This talk is devoted to trying to clarify under which conditions a scientific group can be said to have scientific understanding of an item of knowledge. In the first part of the talk, I argue that the possession scientific understanding is a specific problem for collaborative science, even if it has so far been largely ignored by social epistemologists. In the second part, I present some features usually ascribed to scientific understanding and discuss in what sense a group can be said to have or develop some scientific understanding. In the final part of the talk, I highlight the crucial role of modularity in the development of group understanding.

Is the relativized a priori incompatible with scientific realism?

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I defend the compatibility between Friedman's relativized a priori and scientific realism, [1]. Such compatibility is usually questioned due to the Kantian reading of the former, which clashes with the premise of mind-independence associated with the latter. I argue that this reading of the relativized a priori is misguided and that the notion of constitutivity associated with it does not imply mind-dependence. After analysing the difference between the Kantian synthetic a priori and Friedman's relativized a priori, I argue that the only aspect preserved in the latter from the former is the aspect of constitutivity. I argue that constitutivity does not imply mind-dependence.

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Theory-talk, meta-theory-talk and metaphysical-talk: Intricacies and pertinence of three levels of discourse in the scientific realism- debate

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What is the stuff by which successful theories are hooked on-to-the world- or were there any constituents in past theories by virtue of which they were successful? This is the central concern, based on which most of the debate over scientific realism is carried out today. Selective skeptics and scientific realists, in their attempt to evade the charge leveled by Laudan (pessimistic induction (PI)) respond that past theories were successful precisely because some 'reality-hitting' constituents were in them, whose descriptions were 'truth-bearing'. I identify three different levels of discourse in the scientific realism-debate with regard to PI. I argue that identifying the above discourses brings into light a peculiar epistemological weakness in all these positions.

The commercialization of research— A threat to the objectivity of science?

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In this paper I argue that certain features of commercialization of research can pose a threat to objectivity of science. Not only may conflicts of interests threaten the integrity of scientists, but increasing private funding may also lead to detrimental institutional changes. I will address the importance of the publicity of research in securing the objectivity of science, and then show how some practices that have become more common with increasing private funding of academic research can limit this publicity.

From foundation to function: Rethinking the role of data in science

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In Empiricism and Experience, Anil Gupta proposes an empiricist epistemology in which the rational contribution of experience to knowledge falls in the logical category of a function. I argue that the application of this framework to scientific

theorizing yields fruitful results. In particular, conceiving of the data obtained in scientific experiments as playing a functional role from one set of commitments to another rather than a foundation upon which a theory is constructed can explain how scientists are entitled to claims that appear to be circular in their justification.

Idealization and inference: How false models explain

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In this paper I describe the way in which false scientific models explain. I argue that the explanatory power of models does not depend upon representational accuracy or adequacy, but rather upon the ability of the user to draw inferences from the manipulation of the false components within the models. False models can explain if the manipulation of their false components leads to true inferences about the nature of their target systems. The example that I use as illustration is drawn from my own previous work in astrophysics.

A generalization of the Condorcet jury theorem

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According to the theorem the French philosopher Condorcet developed in the hopes of improving the French tribunal system, under some plausible assumptions, the probability that the majority of judges makes a correct decision gets arbitrarily close to unity when the size of the tribunal increases. This theorem acquired new currency in the twentieth century attempts to develop quantitative models of group decision-making. In this paper, I derive a generalized version of the classical Condorcet Jury Theorem by relaxing the condition for the uniform credibilities of the jurors, and discuss some interesting implications in philosophy of science.

Reason, emotion, and the context distinction

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Recent empirical and philosophical research challenges the view that reason and emotion necessarily conflict with one another. Philosophers of science have, however, been slow in responding to this research. I suggest that they continue to exclude emotion from their models of scientific reasoning because they typically see emotion as belonging to the context of discovery rather than justification.

I suggest, however, that recent work in epistemology challenges the validity of the context distinction, taking reliabilism as my example. Emotion may be seen as playing a reliable role in the formation, which for the reliabilist also means the justification, of scientific beliefs.

One law, 23 derivations: On the plurality of explanations of Planck's law

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The extant theories of explanation are usually seen as competitors. On the basis of a detailed case study concerning Planck's law I want to argue that it can be more appropriate to view different theories of explanation as highlighting different explanatory aspects about one and the same phenomenon. Moreover, I want to claim that even explanations with idealized and in this sense false assumptions can be perfectly good explanations. In effect, this means that explanations of different types and with false assumptions and with mutually conflicting such assumptions can coexist peacefully as explanatory facets concerning one and the same phenomenon or law.

Refined truth approximation by refined belief base revision

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In a forthcoming paper, I have generalized the bridge, due to Cevolani, Festa and Crupi [1], between the conjunctive approach of verisimilitude and AGM belief base revision from finite propositional languages to the general case of approaching any divide of a (finite or infinite) universe, allowing all relevant interpretations. The present paper extends this general form of basic truth approximation by 'basic' belief base revision to refined (i.e. similarity based) truth approximation by a refined form of belief base revision, inspired by Grove's spheres approach and Rabinowizc's similarity foundation of it, hence similar to, but not equivalent to, so-called partial meet revision.

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From philosophy of science to theories of knowledge systems

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We hypothesize that (i) the domain of philosophy of science (PS) includes structures, and processes associated with 'pure' scientific knowledge and (ii) this knowledge exists in a form of particular, but intertwined systems. The *metaknowledge* produced by PS has constituted *epistemic ontology.* It is possible to distinguish descriptive, reconstructivist and constructivist kinds of such ontology. We demonstrate that reconstructivist ontology has made essential steps toward theoretization (more exactly *metatheoretization*) of PS. There are great similarities between reconstructivist metaknowledge systems and theoretical knowledge systems.

How to talk with a skeptic?

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In response to the finding of the impossible, the skeptics could only be silenced. It is impossible that the tribal beliefs can reach the moon, and that should silence those who question the epistemic superiority of science. In this paper, we take this idea to deepen and consider instead the prospect of a possible dialogue between skeptics and opponents. We propose a scheme for describing positions skeptical that neutralizes the wrong issues and paves the way for genuine debate. We apply this scheme to Michel Callon thesis in economics of science.

A turn in computational modeling. The case of quantum chemistry

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In computer methods, mathematization and technology exhibit a close relationship. To illustrate this relationship, a prominent method of quantum chemistry will be discussed, namely density functional theory. The claim is that the remarkable success of this theory in recent years is based on a turn in computational modeling. This turn is based on an explorative and iterative mode of modeling as well as on the infrastructure of networked and easily accessible computers.

Two impossibility results about revision of conditional probability

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Taking *conditional probability functions as a primitive notion,* we prove two triviality results about the possibility of defining the probability $Pr(A > B, \Gamma)$ of a counterfactual as the probability of the consequent B after some revision of the probability function that puts the probability of the antecedent A to 1. Our first result is that any probability revision process that satisfies the identity $Pr(A > B, \Gamma) = Pr^A(B, \Gamma)$ for all Pr, A, B and Γ. The second result is that any probability revision process that satisfies the identity $Pr(A > B, \Gamma) = Pr^A(B, \Gamma^*_A)$ for all Pr, A, B and Γ (where is Γ^*_A a revision of the background Γ in the light of A) is also trivial.

Pluralism and objectivity: On Longino's and Kitcher's approaches

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For about ten years, Helen Longino and Philip Kitcher had a debate on facts and values in science, trying to take a "third way" to overcome the classic fact-value-dichotomy in the philosophy of science. However, Longino's pluralistic approach implies a theoretical inconsistency as it claims a preferably high number of qualified contributions to any scientific discussion that aims for objectivity, but does not regard the question who or what sets an objective standard for the decision who is qualified and who is not. Kitcher, by introducing "deliberators", only bypasses the problem as these deliberators, again, would have to be appointed by someone. Both standpoints premise objectivity for a process that is to generate objectivity. Thus, Kitcher's and Longino's approaches entail political case-by-case decisions. However, this is not as unsatisfying as it might seem at first glance.

Why normal distributions are normal

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It is usually supposed that the Central Limit Theorem explains why various quantities we find in nature are approximately normally distributed—people's heights, examination grades, snowflake sizes, etc. This sort of explanation is found in many textbooks across the sciences, particularly in biology, economics, and sociology. Contrary to this received wisdom, I argue that in many cases, we are not justified in claiming that the Central Limit Theorem explains why a particular quantity is normally distributed, and that in some cases, we are actually wrong. I then offer alternative explanations for why normal distributions are normal.

Externalism, internalism and the conception of the socio-cognitive potentialism

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Potentialism research in sociocultural context conditionality scientific knowledge—an approach which (unlike externalism and internalism) focuses on interactions between its own scientific and socio-cultural factors and the actualization of the potential for interaction. It is an attempt to find the golden mean to solve this problem, based on comparable—existence of opposing potentialities of scientific knowledge and external socio-cultural environment in which it develops. The social environment affects learning indirectly, through the so-called social-cognitive sphere of culture, like science, being an organic part of social and cultural integrity, interacts with it using the same chain, selling only the inherent creative potency comparable to relevant external challenges

Efficient experimentation

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I discuss a new theorem that asserts that scientific institutions (e.g. NSF) can fund *efficient* sequences of observational studies (where efficiency is a function of the variables measured in and sample sizes of the studies) if and only if they heed the following policy: fund any studies that are sufficient to distinguish among the *simplest* models compatible with data, and fund no others. The theorem is important as philosophers of science typically discuss the role of theoretical virtues in *inference* from data. In contrast, I show that simplicity ought to play a central role in *deciding what data to collect.*

Difference-making and ontological explanation

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As far as commonsense naturalistic principles go, the Eleatic principle—roughly, existent things make a causal difference—has proved to be one of the more threatening tools in the eliminitivist's arsenal. Amongst naturalistic philosophers, it is widely assumed that an adherence to this principle is most useful, perhaps even required, to protect us from an inflated ontology, helping us maintain a devotion to streamlined science. In this paper, I show how the enemy of streamlined science, the non-reductive physicalist, can nevertheless maintain an adherence to the principle while at the same time dodge the eliminitivist threat, despite recent suggestions (Wrenn 2010, Beckermann 1997) to the contrary.

The contemporary notion of chance and Salmon's interactive fork model. An attempt to describe chance by means of some causal criterion

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According to the contemporary notion of chance, intersections between different events that belong to independent causal chains are the origin of accidental events, called "absolute coincidences". This paper is devoted to showing the strong relation between the contemporary notion of chance and Salmon's interactive fork model, enforcing the idea not only is there a connection between chance and the Principle of Causality, but that chance can be described in terms of some causal criterion.

Science-based metaphysics: On some recent anti-metaphysical claims

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This paper looks at the recent debate concerning science and metaphysics and whether and in what way, if at all, metaphysics should be accepted alongside science. In particular, it examines some recent objections made, among others, by Van Fraassen against metaphysics as an intelligible autonomous enterprise worth pursuing. Science-based metaphysics is defined in some detail, essentially as an a priori study of a possibility space that both requires science to be 'fleshed out', as it were, and is necessary for the interpretation, and thus proper understanding, of science itself.

Pseudo-scientific explanation and scientific explanation

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This presentation provides new criteria that are useful to distinguish genuine science from pseudo-science. Two demarcation criteria between science and pseudo-science are proposed: experimental and theoretical. In this presentation, I specifically examine the theoretical criterion. These criteria are an application of a new view of scientific explanation. The point is that scientific explanation must unify phenomena into a common frame work. Simultaneously, we must clarify why the phenomena in question differ from other phenomena that have already been embedded into the existing framework.

The role of philosophy of science in the understanding of "post-academic" science

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The objective of this paper is to analyse the problem of "post-academic" science—which intertwines university, science and industry—on the work of the physicist and epistemologist John Michael Ziman F. R. S. (1925-2005) who defended that evolutionary epistemology could work as a methodological tool in the understanding of the changes that have been going on in scientific practice since 1970's. We shall also analyze the use of evolutionary analogy done by Ziman and scrutinize its strength and limits in the understanding of contemporary scientific practice. Ziman's ideas can eventually help academics to overcome the problems imposed by "commodification" of academic research, since he points out to a possible solution to the problem of the relationship between science and society.

On an inconsistency in Constructive Empiricism

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We argue that Constructive Empiricism—B.C. van Fraassen's much debated and explored view of science—is inconsistent. The logical clash is essentially due to two things: (A) Van Fraassen's anti-reductionism: descriptions of human behaviour as intentional actions, as manifestations of human agency, cannot be reduced to, or faithfully translated in, physicalist vocabulary; they cannot be naturalised; and (B) his naturalisation thesis of observability.

The aim of science—knowledge or wisdom

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Scientific knowledge is believed to be an especially valuable product created by humans. According to Nicholas Maxwell all scientific inquiry in the classical sense can be called knowledge-inquiry. Maxwell claims that this is not enough. We need to surpass knowledge and get to wisdom in order to address and perhaps even solve problems of human existence. Still, exchanging knowledge-inquiry for wisdom-inquiry would mean abandoning idealized physics as the most fundamental model of science and putting the stress on the humanities instead. But this will not be science as we have known it any more. Science as such remains in the limits of knowledge pursuit.

Interrogative model of inquiry as a logic of experiment

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In the 1980s, Jaakko Hintikka and his collaborators ([1], [2], [3], [4]) developed a logico-philosophical model of scientific inquiry, the *Interrogative Model of Inquiry,* in which an inquiry is understood as a questioning and answering process—inquiry as inquiry. The model is used in analyzing the logic of experiment. The notion of strategy plays a central role in the interrogative model. The strategic aspects are related to model theoretic forcing. This allows us to get a fresh look at the problem of induction.

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Scientific progress as increase of expressibility, accuracy and coherence

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Does science make progress? If it does, then what are criteria for scientific progress? In philosophy of science, there has been much discussion on these problems. In this presentation, I take a non-realist position and describe the progress of science using three key notions, namely, *expressibility, accuracy,* and *coherence.* These three notions represent different dimensions of scientific progress. In my presentation, the creation of special scientific languages also plays an important role. When science progresses, we will have more specialized languages than before and we will be able to describe the world more accurately than before.

Rethinking belief revision by truthlikeness

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Belief revision (BR) and truthlikeness (TL) emerged independently as two research programmes in formal methodology in the 1970s. In earlier papers I have tried to show that TL gives reasons for rethinking BR in two respects ([1], [2], [3], [4]). First, TL uses distance measures which allow the extension of BR models from propositional logic to full first-order logic. Secondly, it turns out that AGM expansions and revisions of false belief systems by new true input information may fail to increase truthlikeness. The alternative model of updating by imaging seems promising but leads to other problems.

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Conditioning and unfaithfulness

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Causal discovery algorithms have been criticised for relying on the assumption that the true Bayesian network is faithful. Here we explore how the faithfulness of a single path can be altered by conditioning upon or marginalising over a variable connected to it. We identify the possible patterns and phenomena, and hence which paths can exhibit just conditional unfaithfulness or just marginal unfaithfulness. Our findings are relevant to causal discovery. For example, the popular conditional independence learner PC cannot discover conditionally unfaithful arcs, but the metric learner CaMML can. Also, using soft interventions to orient an arc may require appropriate conditioning.

The scientific realism debate from the epistemological viewpoint —Why not consult the theories of knowledge ?—

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One of the main problems in the scientific realism debate is the validity of a form of inference called "inference to the best explanation (IBE)." In the debate over IBE, however, it is known that both realists and anti-realists use certain assumptions which are not admitted by the other side. And some authors suggest that this fundamental disagreement cannot be bridged. Admitting this possibility as far as the truth of realism and anti-realism are concerned, I would suggest that there is another way to show the (il)legitimacy of these positions without begging the question.

Scientific representation: Uses and interpretation of models

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Philosophical production in relation to scientific representation is very rich in recent years. The crucial question now is: Although levels of idealization, constructivism, pragmatism and interpretation inherent to scientific practice, how are theories connected with the world? What is a representation, how it represents, what are the essential elements that permit us to talk about an adequate representation, and what are the conditions of possibility of the scientific representation, or their ways, are questions to discuss in this paper proposal. I'll defend a triadic model, one in which the role of the agents is crucial to speak about a representation.

Accuracy, chance, and the Principal Principle

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I adapt Jim Joyce's 'non-pragmatic vindication' of Probabilism to give a novel argument for various versions of David Lewis' Principal Principle. Joyce enumerates properties that must be had by any measure of the distance from a set of partial beliefs to the set of truth values; he shows that, on any such measure, and for any set of partial beliefs that violates the probability axioms, there is a set that satisfies those axioms that is closer to every possible set of truth values. I replace truth values by objective chances. I show that for any set of partial beliefs that violates the probability axioms or a version of the Principal Principle, there is a set that satisfies them that is closer to every possible set of objective chances.

The limits of probabilism

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We argue that Bayesian probabilism works as methodological foundation only for *phenomenological* theories, where conventions and empirical hypotheses can be clearly separated, while it fails for *abstract* theories like physics, where such a separation is not feasible. We argue for three premises: First, scientific theories contain conventions besides empirical hypotheses. Second, it constitutes a category mistake to speak of the probability of conventions. Third, in abstract theories conventions and empirical hypotheses cannot be clearly separated. These three premises allow to conclude that it constitutes a category mistake to speak of the probability of abstract theories and of abstract hypotheses.

Scientific representation, denotation, and explanatory power

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I argue that existing denotative accounts of representation are plagued with some weaknesses that prevent them from accurately capturing important elements of scientific modeling and attempt to develop an account of scientific representation that ties the representational function of scientific models to their explanatory power. The argument relies on an understanding the notion of representation in relation to the notions of idealization and scientific model and leads to the conclusion that denotative accounts must make use of the notions of 'mechanism' and 'explanatory power' if they are to overcome the weaknesses raised and do justice to how scientific models represent.

Visual representation in the light of methodological demands—A critical review of symbol theoretic attempts to operationalize scientific visualization

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Visual representation has become an issue of increasing interest in the philosophy of science, as it was found to be essential in the process of knowledge acquisition according to the requirements and standards of various research programs. However, the expectations on the epistemic relevance of nonverbal representations in contexts of exploration and argumentation collide with their marginal role in epistemology. In order to bring the empirical and epistemological perspective together, we would like to discuss current strategies to justify the epistemic relevance of visual representation from symbol theoretic points of view.

Seven fundamental versions of philosophy of science

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Seven typical versions of philosophy of science—analytic with and without given logical-linguistic context, social constructivist with and without given social context, hermeneutic/phenomenological with or without given life-world, and anarchistic without any preferred context—can be identified. Considering their view on the question *what science is* fundamental differences between these versions can be characterized. Additional dimension of classification is based on the further characterization of the above mentioned contexts: according to some views these contexts (the logical, the social and the life-world systems) are pre-*given* as unchangeable entities, but in other views they *can be changed* during the scientific activity.

Metaphysical aspects of postpositivism

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Philosophy of postpositivism has incorporated few metaphysical problems—for the most part implicit problems. Philosophy in the classical positivism was a "critique of language". It includes at least some metaphysical hint. K.Popper tried to distinguish science from other means of human knowledge such as myth or metaphysics. He considered that philosophical theories are metaphysical ones. But every rational theory—scientific or philosophical—is rational in so far as it tries to solve certain problems. P.Feyerabend defended pluralism and equality of different means of human spiritual activity. So a world without scientific monopoly would be more perfect than the world we live in today.

Re-positioning realism

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I suggest the 'evolutionary progressiveness' of a theory, rather than its approximate truth only, as novel and promising explanation for the success of science. My basic claim is that we cannot be realists about anything except the progress affected by myriad science-reality interactions that are constantly moving on an evolutionary continuum. I outline a new version of realism in science, referred to as 'interactive realism', and argue that it disarms Kyle Stanford's recent anti-realist 'unconceived alternatives' threats by showing that traditional 'explanationism' and his 'epistemic instrumentalism' are just two positions among many on a constantly evolving continuum of options between instrumentalism and realism.

Scientific realism and inferentially veridical representations

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Many scientific realists think that appropriate kind of empirical success justifies belief in theoretical success: theories making novel predictions (say) are arguably likely to be approximately right about the unobservable world. It is incumbent on such realists to give an account of how scientific theorising can 'latch onto reality'—be approximately or partially true, or partially veridical—in a way that is explanatory of its empirical success. In addition to allowing for an explanation of empirical success, such an account must be compatible with various kinds of inconsistencies in science (in order for the realist to avoid some famous anti-realist challenges). This paper erects a conceptual framework that gives a unified response to various kinds of inconsistencies in science—diachronic, internal, multiple models—allowing the realist to give an account of scientific theorising latching onto reality in an appropriate realist sense. @

Methodology and scientific reasoning Both billiard ball and butterfly?

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The principle of strong causation marks an important difference between traditional methodology and complexity theory. While received conceptions require principles like "similar causes, similar effects" or "cause and effect are corresponding in quality and quantity", complexity theory assumes chains of multiple causal dependencies, appearing and fading away in a hard-to-predict manner.

We concentrate on the following questions:

1. How can the efficacy of emergent properties be translated into a methodological principle?
2. Does downward causation fit in a reductionist approach?
3. What are the prospects of integrated multilevel explanation for the philosophy of science?

We discuss formal models of causal structures suitable to address these questions.

Causal inference, mechanisms and the Semmelweis case

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Ignaz Semmelweis's discovery of the cause of puerperal fever has become a standard case study in history and philosophy of science. It has been used to appraise and compare methodological proposals such as the hypothetico-deductive method and inference to the best explanation. Here I offer an alternative reconstruction of Semmelweis's work in terms of causal inference and mechanisms. I argue that this reconstruction makes better sense of the published accounts of the discovery and is thus historically and methodologically more adequate. Moreover, my reconstruction provides a framework for understanding objections that were raised against Semmelweis's claims.

Bayesian confirmation of creationism? On the problem of genuine confirmation

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According to the Bayesian concept of confirmation, rationalized versions of creationism come out as empirically confirmed. The characteristic feature of these and other pseudo-explanations is that with help of them all kinds of experiences are explainable in an *ex-post* fashion. Intuitively they are not confirmable at all. Alternative concepts of confirmation which attempt to capture this intuition are the novel prediction (NP) and the use novelty (UN) criterion of confirmation. Against both criteria serious objections against have been raised. In this talk I develop a new criterion of *genuine* confirmation which solves the problems of *ex-post* pseudo-explanations in a purely probabilistic way.

From malfunction to mechanism

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Errors, deficits, and malfunctions, or rather, the systematic analysis of patterns of malfunction and function, can help in explanation and understanding of the things in which they are observed and their normal functioning. Based on the errors of a system, information about its structure, its function, its underlying mechanisms can be gained—often information of considerable amount and usefulness. In some cases and in some phases of the research process, more can be learned by focusing on errors than from the analysis of normal behavior alone.

Disambiguating the notion of pursuit worthiness

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The aim of this paper is to explicate some of the major ambiguities underlying the concept of pursuit worthiness. To this end, I will make the following four distinctions: 1) the distinction between the epistemic and the practical notion of pursuit worthiness, 2) the distinction between being worthy of pursuit and the actual theory pursuit, 3) the distinction between a threshold criterion of being worthy of pursuit and the comparison of theories in terms of their pursuit worthiness, 4) the distinction between pursuit worthiness of theories and pursuit worthiness of phenomena.

Screening-off (aka the Markov property) and causal incompleteness—a no-go theorem

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We consider two screening-off principles—one for an intermediate link in a causal chain, the other for a common cause of two or more effects. They are logically independent of each other, and each is independent of Reichenbach's principle of the common cause. Each says that causal completeness entails screening-off and simple examples show why causal incompleteness means that the screening-off relation may fail to obtain. A stronger result about causal incompleteness is then derived: in a rather general setting, if the composite cause $C1\&C2\&\ldots\ \&Cn$ screens-off one event from another, then each of the n component causes $C1, C2, \ldots, Cn$ must fail to screen-off. The idea that a cause may be ordinally invariant in its impact on different effects is defined; it plays an important role in establishing this no-go theorem. Along the way, we describe how composite and component causes can all screen-off when ordinal invariance fails.

Proofs as spatio-temporal processes

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Goguen proposed the concept of *proof-event* to signify a novel approach to proof, designed to cover apodictic, dialectical, constructive, non-constructive proof, as well as proof steps and computer proofs. *Proof-events* are spatio-temporal social processes that at all times require two agents for their understanding and final validation: a *prover* and an *interpreter*. They can allow any semiotic system as a means of formalization and communication, and they incorporate the history

of a proof. Proof events generate proofs presented in different styles which characterize different cultures, schools or scholars that may differ in views of meta-theoretical character. Style can be defined as a meta-code that determines the individual mode of integration (selection, combination, blending) of concepts into a narrative structure (proof). Style then depends on the underlying mode of signification *(semiosis),* the selected code and the underlying *conceptual space.*

Towards a non-adaptationist approach to mathematics

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The aim of this contribution is to show the compatibility between an embodied and bottom-up approach to mathematics, as developed by Carlo Cellucci, and a non-adaptationist view of evolution, adopting an antirealist perspective in philosophy of science.

Deduction, induction and abduction according to Charles S. Peirce: Necessity, probability, discovery

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For C. S. Peirce, there are three types of reasoning. Deduction is the mode of necessary logical reasoning, for the truth of the premises grounds the truth of the conclusions. Induction doesn't draw necessary conclusions, for the truth of the premises doesn't necessarily warrant the truth of the conclusions, but only probably state it, being only a method for testing the conclusions we draw by deduction. Abduction is the type of reasoning that doesn't have logical necessity and has the least probability of establishing a true relation between premises and conclusions, but is the only one with heuristic power.

Prioritized adaptive logics and the epistemology of thought experiments in physics

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Brown, [2], [3], [4], [5], and Bishop, [1] argue that thought experiments in science cannot be arguments and cannot be adequately represented as such. They rest their case on examples of though experiments which either proceed through a contradiction to reach a positive resolution or are used by different people with

opposite results. This, supposedly, makes it impossible to represent them as arguments for logical reasons. I argue against such claims and explain how such phenomena can be modeled within the logical framework of adaptive proofs for prioritized logics.

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Simplicity as a guide to falsity?

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Discussions of simplicity typically assume that once we find the right measure of simplicity and once we find how the right way to weigh simplicity against other virtues the problem of theory choice will be solved. In this talk I argue against this assumption. To be precise I argue that even if we knew that simplicity is somehow tied to truth and we could find the right measure of simplicity and the right way to weigh simplicity against other virtues, we would still face an additional problem, namely there seems to be no guarantee that the simplest theory out of an available class of competitors is the one closest to the truth.

Veritistic social epistemology. A reliable proposal?

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Veritistic Social Epistemology proposed by Alvin Goldman pretends to assess scientific practices by their veritistic dimension. From this perspective, the notion of truth is in the end what determines what knowledge is. Therefore, it claims that beyond methaphysics connotation any epistemology should be

provided by a truth theory. The aim of this paper is analyze this proposal from a critical point of view. We are going to pay attention at how veritism is enclosed in Social Epistemology and if it really answers to the problems of Contemporary Philosophy of Science.

The futility of prescribing what scientists should do: Supplementing van Fraassen's empirical stance with scientific practices

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This paper intends to strengthen van Fraassen's empiricist position (or stance) by pointing out the necessity of taking into account the real circumstances of scientific practices. Its main body develops from the debates between van Fraassen's constructive empiricism and ontic structural realism represented by J. Ladyman and D. Ross. By cumulating all previous improvements achieved in philosophy of science, the debates are valuable by shedding lights on avoiding former mistakes. Their result reminds us of the significance of maintaining an empiricist position without falling into a realist one by committing to opinion. Yet, the remind appears to be prescriptive in the sense that it urges scientists to practice science in accordance with a normative position. We argue in this paper that this position can be strengthened by taking scientific practices into account.

Confirmation, verisimilitude, and acceptance

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Franz Huber's [1] attempt to unify inductivist and hypothetico-deductivist intuitions on confirmation by means of a single measure are examined and compared with previous work on the theory of verisimilitude or truthlikeness. The idea of connecting 'the logic of confirmation' with 'the logic of acceptability' is also critically discussed, and it is argued that 'acceptability' takes necessarily into account some pragmatic criteria, and that at least two normative senses of 'acceptability' must be distinguished: 'acceptable' in the sense of 'being allowed to accept', and 'acceptable' in the sense of 'being obliged to accept'. Lastly, some connections of confirmation theory with naturalism, intertheoretic reduction, and explanation vs. understanding are explored.

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Conservatism in scientific research: A new problem

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Many modern critics of science have suggested that the current institutional system of funding in the U.S. creates strong incentives for scientists to be overly cautious and discourages high-risk, high-reward projects. We suggest this problem goes beyond U.S. science funding, and that it is instead a much more general problem which will arise in any learning situation where there is a conflict between safe and risky alternatives. Through the use of mathematical models and computer simulation we show that individual scientists will often face a "free-rider" problem which will result in overly conservative project choices. We conclude by relating our findings with the philosophical literature on cognitive diversity in science.

Contributed Symposia

Symposium

The interpretation and scope of models of complex systems

Organizer: Christopher Pincock
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Complex systems involve many parts and different kinds of interactions between these parts. They pose a difficult challenge to traditional scientific methods which are based on general theoretical principles and experimentation. To deal with complex systems scientists have developed an alternative approach which deploys models as a distinct object of study in addition to the complex systems themselves. The benefits of this model-based science are considerable. To start, the focus on an abstract or concrete model allows a scientist to narrow her focus to a small number of relevant features. The selective nature of the model then leads to specific predictions and insights which can be tested through a comparison with the complex system itself. Even when a model fails this sort of testing, the simple structure of the model often permits an understanding of the source of the failure and may even suggest a way to revise the model and obtain greater success. These benefits have contributed to the widespread adoption of the model-based approach across the sciences.

In this symposium we will investigate some of the conceptual problems which model-based science gives rise to. Two issues and their relationship have been used to organize our session. First, for any given model, there are questions about the proper *interpretation* of the model. Given that the model is distinct from the complex system in question, which parts of the model represent genuine features of the system and which parts are merely artifacts of the modeling process? The tests which scientists use to evaluate their models do not always help in answering this question. More generally, it is far from clear how models are linked to complex systems so that a model is about those systems. For testing to get off the ground, a scientist must first determine which systems are relevant to the acceptability of the model. But the model does not always indicate clearly what it is about. A second major issue for modeling is the *scope* of a successful model. Even after the proper interpretation of a model is determined, there can still be a question of how widely applicable that model is. Does the model apply only under restricted conditions, or is it valid across many different complex systems, perhaps even for complex systems made up of different sorts of things? Questions of interpretation and scope can interact in significant ways. For example, there seems to be a link between settling for a more general or abstract interpretation of a model and a corresponding increase in its scope. However, even here it is far from clear what adjustments in interpretation are appropriate, or how desirable it is to have an abstract model which is wider in scope than its more concrete competitors.

This symposium is motivated by our dissatisfaction with what philosophers have offered to date to address these problems. We believe that the problems of interpretation and scope arise across the different sciences which deal with complex systems and so a solution must be based on an interdisciplinary approach. That is why we have brought together philosophers whose backgrounds include general philosophy of science, philosophy of biology and philosophy of physics. In addition, our symposium involves contributions from two economists working on the front lines of the struggle to understand complex systems of considerable practical significance. The case studies which these economists will offer focus on the notion of rationality in economic decision making (Kimbrough) and the niche concept as applied to entrepreneurs (Westgren). Both studies illustrate the difficulties in developing, applying and evaluating models. These difficulties also inform the contributions of the three philosophers of science to the symposium. On the interpretation side, there is the general issue of how a highly idealized model can provide correct information about a complex system. Pincock discusses this issue through a case study of an influential model of gregarious behavior. Weisberg tackles the difficult question of how an abstract model can even be about a complex system. Ariew's contribution explains how the proper interpretation of models of natural selection is central to their wide scope of application. He argues that a change in this interpretation in the 1920s was crucial to this success. All five contributors, then, show how model-based science can be successful, and what this success tells us about scientific knowledge.

Prior discussions of the interpretation and scope of models can be found in the philosophy of physics, economics and biology, but we believe each part of the literature suffers from significant limitations. Cartwright initiated much of the discussion of models in the philosophy of physics with her *How the Laws of Physics Lie* [1]. She argued for a clear contrast between the abstract, general principles of our physical theories and the concrete, specific nature of our physical models. This opposition engendered an extended debate on how theories relate to models and to what extent models are autonomous from theories [7]. While helpful in focusing philosophers of physics on issues of modeling, we believe that the terms of this debate are flawed. Some models are characterized in abstract terms and some models are widely applicable. So, even in physics, it seems clear that a simple opposition between theory and model is not helpful in making sense of how model-based science can succeed. Similar flaws are evident in the most influential debates about modeling in economics. On one side, there is the influential argument by Friedman that economic models are evaluated only in terms of their predictive accuracy [2]. This form of instrumentalism fails to fit with the more substantial interpretations which many economists give their models. On the other side of this debate, there are any number of attempts to present a more realistic approach to economic modeling [5]. But these approaches tend to try to fit all successful economic models into a single category. For example, Sugden argues that good economic models "describe counterfactual worlds which the modeler has constructed" [8]. But it is far from clear how this framework can find a place for the many different sorts of economic models encountered in economics and their diverse applications.

The most promising discussions of model-based science have come from biologists and philosophers of biology. Beginning with Levins' groundbreaking "The Strategy of Model Building in Population Biology" [4], philosophers of biology

have recognized the special role of the study of models in advancing our scientific understanding of complex systems [3], [9]. One strand of the literature considers the tradeoffs between different representational goals so that, for example, a general model may involve sacrificing precision [6]. We aim to further develop this literature by showing how questions of interpretation and scope can clarify the aspects of model-based science originally discussed by Levins. Among other things, it is important to see how the challenges and successes of model-based science play out differently in other areas of biology beyond population biology, and also how interactions between fields as distinct as economics and biology can lead to successful models.

By bringing together specialists in different sorts of models our symposium will overcome the flaws of many earlier discussions of model-based science. The case studies and general issues pursued will clarify how scientists determine the proper interpretation and scope of the models they develop. This is an urgent issue for philosophy of science as it is only by making sense of model-based science that we can hope to determine what our current, best science tells us about the world.

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Contributions

- Causation, idealization, and natural selection

André Ariew

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- **On modeling imperfect rationality in agent-based models**

Steven O. Kimbrough
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- **Mathematical models of biological patterns: The legacy of Hamilton's selfish-herd model of gregarious behavior**

Christopher Pincock
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- **Metaphor and models of entrepreneurial behavior**

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Symposium

Integrity and diversity of traditions and trends in today's philosophy of science

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Today's philosophy of science stems from a number of different intellectual traditions and presents a variety of very different trends. Philosophers of science widely disagree not only about specific claims concerning the subject-matter of their study but also about the aim, scope, epistemic status and even the very subject-matter of their discipline. Since questioning of such general issues belongs to philosophy we do not assume that philosophy of science may or should reach a final consensus about such matters. What can possibly bind the philosophy of science into a single whole is, in our view, not a consensus about its first principles but a rational dialogue. What we see as a problem that needs an urgent remedy is not the diversity of the contemporary philosophy of science itself but the poor communication between philosophers of science representing different intellectual traditions and working in different parts of the globe, often in different languages.

We would like to use the 14th CLMPS as an opportunity to engage different schools in philosophy of science into an active intellectual interaction. Since this meeting takes place in France we find it appropriate to make a particular accent on French school of philosophy of science and evaluate its legacy within a broader international context. We hope that our Symposium will serve a better integrity of existing schools and trends in today's philosophy of science and open new possibilities for the collaboration of people having different cultural and philosophical backgrounds. Although any reflection about intellectual traditions necessarily involves a historical aspect our ultimate aim is to develop forms and modalities for a future world-wide dialogue.

The proposed Symposium comprises five papers of rather diverse character. Jean-Jacques Szczeciniarz describes the place of French school of philosophy of science within today's international context, traces its historical origins and suggests some guidelines for its further development. Hourya Sinaceur Benis provides a more specific account of works of Jean Cavaillès and his influence onto the Anglo-Saxon philosophy of mathematics. Jonathan Regier discusses a hot dialectical controversy between the historical and the systematic approaches in the philosophy of science as one of main dividing lines between the Analytic and the Continental (both broadly conceived) traditions in this field. Andrei Rodin considers another controversy, which equally has to do with the Analytic/Continental division, namely the controversy between translation and formalization. Elena Mamchur warns in her paper against a straightforward multicultural and sociological approach in the philosophy of science, which makes this discipline irrelevant to issues of scientific knowledge and scientific truth. In spite of their different character all the five papers are given from a perspective that takes into account the existing diversity of philosophical traditions and aim at a better integrity of these traditions.

Contributions

- **Jean Cavaillès and the philosophy of concept**

  Hourya Bénis-Sinaceur

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- **Should the role of epistemology in the philosophy of science be reconsidered?**

  Elena Mamchur

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- **Representing a relationship: The historical and the philosophical in the history and philosophy of science**

  Jonathan Regier

  University Paris-Diderot (France)

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- **Translation versus formalization**

  Andrei Rodin

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- **French school and the diversity of traditions of philosophy of science**

Jean-Jacques Szczeciniarz
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Symposium

Calibration in scientific practice

Organizer: Léna Soler
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Calibration is an important but often neglected topic in the philosophy of science.

It is important since it essentially conditions the constitution and the reliability of instrumental methods which play a major role in science. The outputs of measurement apparatuses commonly used in everyday laboratory practices would be no more than marks deprived of any significance unless the apparatuses in question have been correctly calibrated. New devices or techniques must successfully pass calibration tests (or tests akin to calibrations) in order to be recognized as sound candidates for further development (standardization and broader diffusion in the scientific community). These calibration tests often exhibit a recursive structure that ultimately depends on the work of metrologists, i.e. measurement experts who build the primary standards that realize basic units.

If calibration is an often neglected topic, it is because it is treated as a preliminary and unproblematic procedure that precedes the "main show", namely the investigation of the object under study *by means of* what has been previously calibrated in this purpose. Philosophers have tended to focus on primary investigations as the truly interesting, difficult and problematic aspects of laboratory science. Contrary to this approach, the study of calibration practices shows that even in everyday uses of well-mastered instruments, calibration procedures are often delicate, complex, and problematic activities. This *a fortiori* holds for the calibration involved in creative metrological practices and in practices dedicated to the elaboration of new instrumental prototypes.

When calibration is mentioned in the literature devoted to science, the word appears to be used in a more or less broad sense: it can naturally apply to measurement devices, but equally to other ingredients of scientific practice such

as the measured sample, the algorithm used to analyze data or even scientists themselves in their capacity as instrument-readers. At a more general level, the core idea of calibration that brings together the different uses seems to be the comparison of a questioned object or procedure (say X) with an already fixed, accepted, taken-as-more-robust reference R, in the intention to conclude about the properties or reliability of X on the basis of R. Now the kind of X which might be the targeted object of calibration, and the kind of process through which the reliability of X is assessed against an R taken as a benchmark, are likely to vary, and may be associated with different philosophical issues, according to the kind of scientific practice under scrutiny.

The symposium will consider three different cases of calibration. First, L. Soler *et al.* will discuss the calibration of already well-known and well-mastered instrumental devices in everyday laboratory practices. The aim of this first paper is to propose a conceptual framework for analyzing calibration processes that sheds light on the internal logic of scientists' calibration practices. Based on examples of such practices, this conceptual framework will emphasize that scientists' calibration practices are complex and epistemologically interesting even in the case of 'ordinary' scientific instruments. Second, E. Tal will discuss calibration from the point of view of metrological practice. The aim is to elucidate the inferential structure of calibration by defending, in place of an operationalist view of calibration, a model-based account. This paper will notably show that a model-based account of calibration can explain how metrologists estimate and improve the accuracy of standards, and why metrologists have recently proposed to redefine all base units in the International System in terms of fundamental physical constants, that is, without any reference to material artifacts and empirical procedures. Third, J. Livengood will discuss the calibration of a new instrumental technique. By focusing on an historical case-study concerning the development and use of chromatographic adsorption analysis by the Russian botanist Mikhail Tswett, this third talk will discuss the difficulties scientists may face in their attempts to establish the reliability of new devices when they are used on new targets of investigation.

Contributions

- **Calibration in everyday scientific practice: A conceptual framework**

Léna Soler[1], Catherine Allamel-Raffin[2], Catherine Dufour[3], Jean-Luc Gangloff[2], Emiliano Trizio[1], Frédéric Wieber[1]

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- **Calibration, modeling and the International System of Units**

Eran Tal

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- **Tswett's control experiments and Mill's method of difference**

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B2. Ethical Issues in the Philosophy of Science

Invited Lectures

Scientific integrity in a politicized world

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That politics has an influence on science is unavoidable. Political winds shape the amount and emphasis for research funding, on which science is dependent. Political contexts determine the ethical boundaries for research. Political debates draw upon scientific research (often selectively). It light of these forces on science, it becomes imperative to understand clearly what we should mean by scientific integrity, so that it can be both identified when present (or absent) and defended when threatened. By delving into the roles for values in science (both acceptable and unacceptable), this talk will present a clear and defensible view of scientific integrity, develop its implications for the assessment of expertise, and show how defending scientific integrity is not sufficient to remove all of the influences of politics on science—it removes only the most pernicious. To address the full range of politicization concerns, we need to consider both the social community of science and the reasons why we pursue science.

On the co-unfolding of scientific knowledge and viable values

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Ethical/social values have proper roles to play at several moments of scientific research, including (1) when items are chosen for investigation and (2) when the risks of technoscientific innovations are appraised. Concerning (1), since values of technological and economic progress are currently highly salient, priority tends to be given to research that may lead to technoscientific innovations that will contribute to economic growth and competitiveness. Concerning (2), values influence what is considered a potential harm; and empirically grounded risk analysis often leads to endorsing hypotheses that risks are insignificant, i.e., to making the judgment—after considering the consequences of acting informed by such a hypothesis should it be false, and their ethical salience—that the evidence supporting it is sufficiently strong (despite remaining uncertainties) to legitimate acting in ways informed by it.

Common views about the dichotomy of fact (scientific knowledge) and value usually overshadow deliberations on these matters. But they ignore that holding

values has presuppositions that are open to empirical investigation. Holding the values that currently tend to shape research priorities, e.g., presupposes claims such as: 'technoscientific innovation provides benefits that contribute towards the well being of human beings generally', 'there are technoscientific solutions to most human problems, including those occasioned by technoscientific innovations themselves' and 'there are no serious alternative proposals available today to the pursuit of economic growth based on technoscientific innovation'. Such presuppositions also underlie the value judgment that undermining alternative practices, which are not based on technoscience, is not a potential harm that needs to be investigated; and they inform the presumption of legitimacy usually accorded to implementing novel technoscientific innovations.

Presuppositions like these are widely endorsed, usually without their evidential support being carefully appraised. They may be investigated empirically, however, but only if a range of methodologies is deployed that is more comprehensive than those used in research that leads to technoscientific innovations and in the standard risk analyses that accompany them—methodologies that are able to take into account the social/economic/ecological/historical context of innovations (and of the problems they may be intended to resolve). The outcomes of such investigation may put the presuppositions into question and thus the value judgments that they underlie. Not only do values have impact at certain moments of research, but also scientific research can have impact on what are viable values.

If the appropriate range of methodologies is not deployed, however, and scientific research is limited to methodologies of the kind that are involved in the research that generates efficacious innovations, the values in play at (1) and (2) will not be subjected to the critique that can come from scientific investigation; and they will play their role without being opened to reasoned (and democratic) deliberation, and perhaps go unnoticed. The co-unfolding of scientific knowledge and viable values depends on recognizing the essential role of methodological pluralism. (It also depends on upholding that the criteria for the appraisal of scientific knowledge claims do not depend upon holding any particular ethical/social values.) It points to dialectical (not reductionist) relations between scientific knowledge and reasonably held values that perhaps can cut through some of the impasses confronted in controversies about technoscientific innovations—and, in accordance with the Congress's aim, 'help deepen our understanding of the most promising orientations in science and even help promote future advances in human civilization'.

Contributed Papers

On rationality, irrationality and counterrationality in dynamics of knowledge

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The problem of knowledge dynamics is considered in the context of *the practical turn in logic* (in the Gabbay/Woods sense). Evolutionary and revolutionary changes within so-called *hierarchical knowledge systems* are investigated in three main situations—perfectly normal situation, the situation of constrain and that of exasperation. These situations were introduced by Leszek Nowak in the framework of non-Marxian historical materialism. Our approach results in models (matrices) for such fundamental processes as innovation, development and advance.

Scientific methods and strategies of research: A plurality of paths to the objectives of science

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I would like to present a comparative analysis of two models of scientific inquiry, concerning their dynamics to reach the goals of this activity. The reticulated model of scientific rationality, proposed by Laudan, offers an interpretation of science as shaped exclusively by cognitive goals. Lacey, on the other hand, presents a strategy based model of science, in which the investigation of relevant phenomena in human's lives is included alongside the cognitive interests of inquiry. In this analysis, both models admit the adoption of several methods to reach science's cognitive goals, articulating methods or strategies and aims.

Epistemethics: Lessons from an ethnographic study of global health research ethics concerning the articulation between research and practice

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Epistemethics is a proposed philosophical framework to question the structural tensions leading to ethical dilemmas in scientific practices. Focusing on research vs practice distinction in health research, this paper presents some empirical

data gathered in 2010 during a field study on three global health researches occurring in Latin America. These data illustrate the need to consider the necessary articulation of ethics and epistemology regarding the social relevance of scientific research, context-sensitive methodology, research engagement with social equity and participatory approaches.

Scientific credibility in the public exemplifying climatology: Why it is important, how it is challenged

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Credibility is important for the sciences. Especially mass media are laypeople's number one source to assign credibility to scientific information. However, they have no obligatory quality standards but are under the pressure of market forces which can tend to drive out truth. Therefore, scientists have to take on responsibility when they present their work to the public which means that they have to make uncertainties transparent. This causes another problem since it is expected in general that scientists are neutral revealers of "the truth". If scientists explain uncertainties and inform about value-laden decisions, it is observable that they fast lack trustworthiness in the public. I will ex- emplify these entanglements by climatology and conclude that deliberative instances as the IPCC are the best way to avoid and overcome these problems.

Science, episteme and mathematical ethics (A law of contraposition of episteme in algebra of formal ethics)

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Evolution of science is obvious, but evolutionary epistemology is problematic. If the word "episteme" is taken in the ancient Greek meaning, then "evolution of episteme" represents a logic contradiction. The relationship between the episteme and the science is considered as a *formal-ethical* issue: science and episteme are precisely defined as *moral-evaluation functions determined by two variables* in two-valued *algebra of formal ethics.*

Where the opposition to value-free science should be revised

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I want to show the argument against value-free science, particularly by Heather Douglas, could not be applied to every risk-related science, but should be revised in some way so as to be valid in some important cases of risk science. I'll focus

on its basic assumption that the two values, epistemic and non-epistemic, could go on a par with each other in hypothesis evaluation, and criticize this based on one of Japan's hottest issues concerning the drawing off the mad of a dam, in which there is unavoidable imbalance between the two kinds of values.

Philosophy of science and ethical issues —from a Warsaw perspective

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In my exposition I want to reflect—from a Polish perspective—on the question as to what is the prospect of the natural incorporation of ethical topics into logical, historic-sociological, and methodological currents in the contemporary philosophy of science. I will refer to K. Ajdukiewicz's, [1], [2], distinction between "metascience" and methodology, S. Amsterdamski's [3], conception of historically variable "ideals of science" and A. Grobler's [5] critique of L. Laudan's "reticulated account" of scientific rationality, [6]. In the final part I will briefly review the articles referring to ethical issues that have appeared in the columns of the quarterly *Filozofia Nauki (Philosophy of Science)* in Warsaw since 1993.

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B3. Historical Aspects in the Philosophy of Science

Invited Lectures

Verificationism and scepticism

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Verificationism is obviously different from scepticism, for, while the global doubt of the sceptic implicates any form of knowledge, verificationists typically trust some forms of knowledge while denying (purported) others. Nonetheless, the verificationist often joins forces with the sceptic in opposing claims to knowledge that both of them deny. For example, the verificationist may join the sceptic in denying absolute time and absolute temporal relations on account of their being unverifiable. In this paper, however, I examine three examples of verificationist arguments that can be used to counter (certain kinds of) sceptical arguments: Einstein's equivalence principle, Putnam's model-theoretical argument and Wittgenstein's rule-following paradox. I argue that while verificationism is often used to deny truth and meaning (to the unverifiable), it can also be used to confer meaning and defend truth in the face of the threat of scepticism.

On the question dynamics of research: Modes of finding and losing research topics in science

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Two major branches of research today are basic or epistemic research, on the one hand, and applied or application-driven research, on the other. I address how research topics are chosen in these tow branches. Roughly speaking, epistemic research proceeds in a knowledge-driven mode, in which research items are picked on the basis of what has been accomplished before and what is considered to be successfully treatable, whereas applied research proceeds in a demand-driven mode, in which questions are addressed that are deemed important from an extra-scientific point of view. A more thorough examination reveals, however, that many applied research projects are likewise pursued in close interaction with the available knowledge. Subsequently, I consider modes of topic loss in science and technology. My claim is that the loss of research items is governed by mechanisms that bear a resemblance to the modes of problem choice.

Contributed Papers

Compounding ratios, theories of ratio and geometry in theoretical music in the 16th century

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This paper aims at understanding mathematics related to a change in the conception of music during the Renaissance. Throughout its history from Antiquity to Renaissance, western music developed from a cosmological-mathematical-speculative model, in which attention was focused on a rational activity of speculation and the purpose of the musical sound was to imitate a supramusical order and regularity, to a geometrical-empirical model, in which the emphasis lay on the quality of the sound itself and music was examined through its laws and effects on people. We consider in particularly compounding ratios, theories of ratio and geometry in such a change.

Neurath on pictures, language and international communication

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This presentation examines Otto Neurath's educational work concerning visualisation of social statistics at the Museum of Society and Economy and the Isotype Institute. Neurath's use of pictures to express information about social and economic facts is informed by a deep-seated privileging of the universality of visual perception over conventional verbal language with its complexities and shortcomings. His focus on the visual expresses an anti-metaphysical attitude in an empiricist framework. Politically, visual communication promotes internationalism in a stratified society filled with educational inequalities. I suggest viewing Isotype as an international auxiliary language, considering its syntactical structure and political motivations.

Between Kantianism and empiricism: Otto Hölder's philosophy of geometry

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I discuss the philosophy of geometry developed by the German mathematician Otto Hölder (1859-1937) in his epistemological writings of 1899 and 1924 and put it in connection with his theory of measurement. Once he proved that

the so-called Archimedean axiom can be derived from Dedekind's continuity, Hölder suggests that his prove can be extended to projective geometry. The variability of metrics speaks for the opinion that the origin of geometrical axioms is empirical rather than *a priori*. Nevertheless, continuity may be a priori in Kant's sense. Hölder thus synthesises competing views of his time such as geometrical empiricism and neo-Kantianism.

Carnap's vision or: How we can learn from the past and enlighten the future of the philosophy of science

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Enlightening insights into the future of the philosophy of science can be achieved by taking seriously Carnap's mature view of the relation between philosophy and science, where he attempted to integrate and coordinate within a single philosophical enterprise the diverse approaches to scientific philosophy present within the Vienna Circle and represented by Schlick, Neurtah and himself. Although Carnap never actually pursued his synthetic proposal, his vision can teach us how to reconcile the apparently incompatible current versions of those approaches—based respectively on conceptual analysis, historical case-studies and formal modeling—and steer a new course for the philosophy of science.

Mathematics and the purity of methods: Some historical considerations

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In this talk I will bring attention to the role concerns for purity had played in shaping the acceptability of mathematical arguments and trains of inferences, by contrasting two examples: Hilbert's *Foundations of geometry* and Descartes' *Géométrie*. If the first one treated methodological purity as a subjective value, the latter considered purity as a principle for problem solving. Descartes discarded purity interpreted as the claim of solving problems by using the most germane techniques, but he also seems to adhere to another kind of purity, dictated by the search for an objective ordering of mathematical notions and objects.

Symmetry and the enigma of space and time. Reflections on the origin of gauge theory

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By considering the significant aspects of H. Weyl's theory of groups applied to quantum theory, this paper emphasizes the role played by the question of the nature of space and time at the origin of gauge theory. The paper first proceeds by identifying the transcendental approach underlying Weyl's view of mathematics and physics. Secondly, I will discuss Weyl's conception of symmetry. The paper concludes by showing how this conception is related to a certain notion of objectivity also discussed in current debate on gauge symmetry.

Quality and practice in mathematics from Hilbert to Grothendieck

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In order to characterize the new way of thinking and practising initiated by Hilbert, Grothendieck or Bourbaki, we refer to a "mathematical world". As a consequence we reconsider the "purety of methods" and link it to naturalness, which means consonance with this world. In particular "topical purity" gives place to "landscape purity".

We also consider new epistemic virtues, which aborrow from theological ones: faith, hope and charity. We demonstrate how this virtues influence the practises in research. We take a few examples from mathematical Analysis of the XX$^{\text{th}}$ century or from other themes, examples that we analyze methodically.

Theory underdetermination: The history of science perspective

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Theory undedetermination by data (TUD) entails existence of empirically equivalent, but postulating incompatible ontology theories. There is a definite link between concrete forms of such strong TUD and attitudes toward realism defense in the philosophy of science. Historical analysis reveals that some forms of TUD can be eliminated by future experiments and does not pose an unsolved problem for realist. But the other forms of TUD appear more than simple puzzle for realist. We can find such TUD in modern quantum-relativistic paradigm, that counters to realist's claim that modern physics is closer to the reality then classical physics.

Two traditions of conventionalism

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In the paper I offer an alternative to Lakatos's typology of conventionalism. Instead of conservative-revolutionary opposition I put forward the dualist-holistic distinction. The latter I consider more fruitful for the history of ideas, for it enables us to trace the conventionalist's motives and better understand their function in a number of contemporary approaches.

Kant, Fichte and algebraic operations: Philosophy of algebra according to Jules Vuillemin

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In Kant, geometrical concepts are constructed by an ostensive way, while algebraic concepts by a symbolical way. We can consider that the ostensive construction makes "objects" explicit, while the symbolical construction makes "operations" explicit. However, the function of "operations" is not clear in Kant. J. Vuillemin [1], [2], [3] considers how the process of the explicitation of "operations" has neglected concrete "objects" in the history of modern algebra. According to him, there are some similarities between Lagrange's theory of algebraic equations and Galois' theory, on one hand, and Fichte's *Wissenschaftslehre,* on the other. Reflexive method, practical or operative reason, and movement of concepts towards the liberation from the sensible are important for them.

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Gödel and Leibniz on concepts and relations

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This talk considers Gödel's ideas on perception of concepts in comparison with Leibniz. Our consideration is based on his posthumous notes on Leibniz: Max Phil X, which is recently almost transcribed. Gödel's main interest, in his

deep research on Leibniz in 1943-1944, was to create a new logic of concepts by searching the categories for concepts. Here, we find newly that Gödel was facing the problem of the epistemic and ontologic status of relations. To see this, we focus on his analogical argument and his study on the grammar of relational propositions, and compare them with Leibniz's original ideas.

Changes in the perception of time in Victorian scientific theories: Lyell, Darwin and Maxwell

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The perception of time changed dramatically during the 19th century in both science and the social sphere. This work explores these changes within the scientific context, and maps their interconnections with the major scientific developments that were taking shape during the same period of time. This study is focused on Charles Lyell's Uniformitarian Geology; Charles Darwin's Theory of Evolution; and James Clerk Maxwell's work on the Kinetic Theory of Gases. The main argument is that it is impossible to understand the revolution that was occurring in the concept of time without revealing its relationship with three central ideas that transformed Victorian science: emergence, directionality and probability.

The unimportance of Quine's *Two Dogmas of Empiricism*

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In *Two Dogmas* (1951) Quine rejected what he took to be two pillars of neo-positivism: analytic/synthetic distinction and reductionism. However, such criticism was put forward much earlier by thinkers closely connected with the criticized movement. At the very beginning of the thirties Tarski convinced Carnap that the distinction must be relativised; Carnap, influenced by Neurath, dropped reductionism and verificationism. Soon Neurath's physicalism, catalyzed by Poznański-Wundheiler ideas, turned into encyclopedism—a holistic and naturalized approach defended later in Quine's article. The paper is to that show the postulate of empiricism without the dogmas was materialized at least 15 years earlier.

A source of Feyerabend's decision-based epistemology: Hugo Dingler's voluntarism

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The aim of our contribution is to highlight a neglected source of Paul Feyerabend's philosophy, namely the work of the—by now—forgotten philosopher of science Hugo Dingler. Dwelling into unpublished archival sources, we show that Feyerabend studied extensively Dingler's work in his youth, as far as to become a "determined Dinglerian" for some time. This background, we argue, is important in order to assess the prominent role which Feyerabend assigns to decisions in settling conventional elements in scientific knowledge, i.e. the voluntarist bent of Feyerabend's Decision Based-Epistemology, as we propose to call it.

Constructing natural historical facts: Baconian methodology in Newton's first paper on light and colors

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The extent to which elements of Bacon's natural and experimental history can be identified in Newton's early papers has been subject to debate and controversies. In my paper I will explore some of such particular Baconian elements that can be found in Newton's first published paper on light and colors. I will show that the construction of Newton's paper follows some of the Baconian rules for writing a natural history, while his development of experiments and the construction of facts have features of the Baconian *experientia literata.* My research involves a historical reconstruction and philosophical interpretation of the methodology and theoretical structure embedded in Bacon Latin natural histories.

Evolution as metaphor for scientific progress

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Scientific progress is often described as *evolutionary.* How apt is this use of the idea of evolution as metaphor? I argue for the appropriate use of this metaphor albeit as one of a multiplicity of metaphors required to depict progress. To make this argument, I look at the role of metaphors in science and develop a framework for analysing metaphors. I then develop my own view on the possibilities of this metaphor by imaginatively linking contemporary understandings of evolution with prominent models of scientific change. In the process I evaluate interpretations of this metaphor already given in different philosophies of science.

With Bošković against Kant: Ivan Krstitelj Horvath on space and time

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In 1797, Ivan Krstitelj Horvath published his book against Kant's *Kritik der reinen Vernunft* under the title *Declaratio infirmitatis fundamentorum operis Kantiani 'Critik der reinen Vernunft'.* It was originally conceived as a supplement to his university textbook *Institutiones metaphysicae* (1767). Horvath targeted his criticism at the foundations of Kant's first *Kritik,* among which he expectedly included Kant's doctrine of space and time. He was convinced that Kant's argument »Der Raum wird als eine unendliche gegebene Größe vorgestellt« (A 25, B 39) may be successfully refuted by using Bošković's arguments expounded in *De spatio ac tempore* (1755).

Cartesian forces in a soulless physics

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Descartes' metaphysical dualism has important consequences for his physics. He intended to establish a certain and quantitative knowledge about the physical world, and his dualism drove away all kind of spirits or forces from it. Nevertheless, "forces" do not seem completely absent in his natural philosophy. Some contemporary scholars think that Descartes, in some passages of *The World and the Principles of Philosophy,* expresses himself as if the forces described were "real" properties of the bodies. Therefore, in this paper I will argue in favor of Cartesian dualism's coherence, making use of a little-known notion of force proposed by Descartes.

Historical epistemology Notes on Archimedes, Torricelli and Sadi Carnot

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Based on my recent studies I shall introduce a particular use of logics in historical-epistemological analysis and some cases-studies would be presented. *On the Equilibrium of Plane* focusing on the law of the lever, centre of gravity is examined. On *Opera Geometrica* (1644), centre of gravity of bodies and the famous principle in mechanics are discussed. On *Réflexions sur la puissance motrice du feu* (1824), the discursive part of Sadi Carnot's is dealt with, where more than 60 Doubly Negated Sentences are emerged not equivalent to the corresponding affirmative sentences and involving, generally speaking, non-classical logic.

Kantian aspects of Poincaré's epistemological thoughts on XIXth century physics

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Some of the Kantian principles and distinctions, considered in the Inaugural Dissertation of 1770 and developed in the First and Third Critics, illuminate Poincaré's methodological cogitations on the nature of XIXth century physics. His epistemological thoughts concerning the evolution of Physics that culminates in Maxwell's final approach to Electromagnetism, published before and around the year 1900, include an analysis of the principles of convenience (need of order, simplicity and harmony), the constitutive function of mathematics, the general methods of mechanical modeling of phenomena, the hierarchic organization of the system of natural laws and principles, and their changeable status.

A study of analogical reasoning based on William Harvey's problems and analogies

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In this paper I will study William Harvey's application of analogies in the *Exercitatio anatomica de motu cordis et sanguinis in animalibus.* I will show that Harvey applied analogies in many different ways and that some contributed to the discovery of the characteristic 'action' of the heart and even to the discovery of the blood circulation. The discovery process will be approached as a problem solving process as described in Batens' contextual model. The focus on constraints allows to see Harvey both as a modern in some respects and as strongly influenced by his Aristotelian teacher Fabricius of Aquapendente who propagated a 'natural philosophy interpretation' of anatomy.

History of science and language criticism : A cross-referenced reading of Ernst Mach and Fritz Mauthner

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The subject of this paper will be the correlation between history of sciences and language criticism, based on a confrontation of Ernst Mach's and Fritz Mauthner's philosophical works. We will see that this confrontation is relevant,

1. The author is Research Fellow of the Fund for Scientific Research - Flanders (FWO-Vlaanderen).

not as much in it specification of the history of philosophy as in its analysis of the interactions between a radical historization of science (Mach) and of language (Mauthner). This will give an input to the discussion of the relationship between the categories of "positivism" and "historism", which is linked to a re-structuration of the field of knowledge during the 19th century, and to the emergence of objectivity as a "scientific virtue", [2], which establishes itself in the interpretative disciplines—thus measuring the impact and the consequences of this historist fashion for the philosophy and historiography of science. Generally the cross-reading of Mach and Mauthner focuses on self-proclaimed "language criticism" as a critique of metaphysics, without questioning the relationship between Mach's "science"/"language" analogy, [1] and a historism which ascribes the critical and legislative power of philosophy to history.

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Semantics in type theory

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This talk surveys several transformations and applications of logical type theory between 1925 and 1940. Specifically, we focus on two related developments: (1) several contributions concerning the formalization of the type-theoretic universe and the flexibilization of types will be discussed. (2) Following this, we survey attempts by Carnap and Tarski to express the semantic metatheory of axiomatic theories within a single type theory. Specifically, different conventions introduced in their work to allow domain variation for models of theories expressed in a fully interpreted typetheoretic language will be compared.

Poincaré's two types of conventionalism

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Poincaré presents two kinds of conventionalism but they have not been clearly distinguished. Poincaré argues that certain elements of empirical science can be "erected" (érigées) into principles, that is, they can be taken to be definitely true and never questioned. However, geometric conventionalism has a separate two part justification which is quite different from his justification of the conventionality of principles, consisting of an argument first against the a priori determination of metric, and second against empirical determination of metric. I distinguish two kinds of conventions in Poincaré and show that the arguments for them are not the same.

Ibn al-Haytham's 'al-Shukuk' or the art of controversy: How the eleventh century arabic scientist's arguments changed astronomy forever

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This paper examines how Ibn al-Haytham changed the course of astronomy that was traditionally attributed to extrascientific factors. His *al-Shukuk* or *Doubts about Ptolemy,* among the newly discovered materials, is a systematic refutation of the Greek's approach to science, it is Ibn al-Haytham himself who presents the controversy by making Ptolemy the proponent while he plays the role of an opponent. I argue that Ibn al-Haytham's successful challenge is due to his application of this new scientific method to the discussion of scientific theories that brings to the forefront some of the weaknesses of Ptolemy's justification of his system.

Some semantic considerations for the conceptual transition from Euclidean to non-Euclidean geometry

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In my presentation first I will go to reconstructing the essentialist view of the conceptual-semantic transition from the Euclidean geometry to the historically formulated versions of the non-Euclidean geometry. This will be contrasted to one of the anti-essentialist views which can be derived from Quine's rejection of the analytic/synthetic distinction. Then both the essentialist and the anti-essentialist approach will be confronted with the historical evidence. This historical evidence is partly due to the recent re-reading of the *Appendix* of János Bolyai, and partly due to some novel but recently unpublished results concerning the early conceptual system of János Bolyai.

On A. A. Markov's attitude towards Brouwer's intuitionism

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The paper examines Andrei A. Markov's critical attitude towards L. E. J. Brouwer's intuitionism, as is expressed in his notes to the Russian translation of Heyting's *Intuitionism,* published in Moscow in 1965. It is argued that Markov's algorithmic approach was shaped under the impact of the mathematical style and values prevailing in the Petersburg mathematical school, which is characterized by the proclaimed primacy of applications and the search for rigor and effective solutions.

Beyond Poincaré and Einstein: A. A. Robb's theory of space and time

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Alfred A. Robb's position on the geometry of space and time is often considered closer to Albert Einstein's theory of special relativity than to Henri Poincaré's conventionalist doctrine, which ruled out any empirical determination of the geometry of phenomenal space. Robb, however, did not admit Einstein's distant simultaneity, rigid rods, or ideal clocks, in line with his mathematical training in Cambridge and post-graduate studies of the Zeeman effect in Göttingen under Woldemar Voigt's supervision. Based in part on Robb's surviving correspondence and newly-discovered archival documents, my paper traces the origins and evolution of Robb's work with respect to contemporary debates on the foundations of physics.

Methodological remarks on knowledge-production and text-production: Newton's optical controversy and methodological shifts

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The norms of reconstructing positions have not received sufficient attention in the historiography of controversies. In the optical controversy of the 1670s the paper shows how a reconstruction sensitive to the argumentative context can highlight the relevance of the critique a theory receives, and in Newton's case show how the response that the criticisms triggered paved a way to his novel methodology with wide-ranging repercussions. Such an approach has the potential to highlight the functional relevance and possibly the epistemic function of the discursive use of methodology in controversies.

Inconsistency of ancient skepticism

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Ancient skepticism faces the persistent charge that it is an inconsistent view. Recently, there are three important ways to defend the consistency of Sextus Empiricus' skepticism, [3]: (1) allowing a skeptic to have weak beliefs about things, [2], (2) allowing philosophy to be practiced without beliefs, [4] and (3) perceiving inconsistency as the effect of the development, [1]. The first option has a weak base in the texts. The second and third option cannot explain the logical power of skeptical arguments. They reduce skeptical philosophy to some kind of passive life in animal style. My thesis is that Sextan skepticism is inconsistent.

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Contributed Symposia

Symposium

A plurality of currents in today's historical epistemologies

Organizers: Karine Chemla, Koen Vermeir

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Historical epistemology is now again a burgeoning field of study, bringing history and philosophy of science together in new ways, potentially beyond any form of boundary (disciplines, time periods, geographical areas). On the one hand, historians of science understand historical epistemology as both a philosophical underpinning of their work and a heuristic tool. Some of them aim at uncovering the historically situated conditions of a practice of knowledge, of epistemic virtues or of scientists' styles of inquiry. Others study fundamental scientific concepts, which organize knowledge in different historical periods, along with the contingent conditions for their permanence or transformation. On the other hand, philosophers of science interested in historical epistemology develop new theories of concept formation and the naturalisation of epistemology and they think through the philosophical consequences of the social dimensions and historicity of knowledge.

This symposium derives from the conviction that such questions are essential ones if we are to attend to the specificities of concepts and results produced by the practitioners of science that we study as well as (and in relation to) the specificities of the practices they shaped. More precisely, the symposium aims at exploring the hypothesis that we can find in these recent developments the resources for defining a new form of contextualism, one that gets closer to the collectives of practitioners and attends to their scientific practice taken in a broad sense. The idea is to develop analytical tools to dissect scientific practice that are fine enough, on the one hand, to identify the main components in any of them and, on the other hand, to shed light on how these elements as well as their relations to each other vary from context to context. Among these elements, we need to take into account epistemic *dispositifs* and epistemological choices shared by collectives of actors. In addition, the new form of contextualism that we have in view is one that sets itself the task to account for the fact that despite their localization in given contexts, knowledge statements and practices do circulate. In this sense, this program is part of the European Research Council project SAW (Mathematical Sciences in the Ancient World).

To fulfill this task, it appeared to us as a necessity to reflect from this perspective on the history of historical epistemology and on its present-day state. This symposium is devoted to the task of analyzing some of the new directions taken by historical epistemology at the present day from the perspective outlined above. It is affiliated with the symposium "CONFRONTING FRENCH

ROOTS AND CURRENT HISTORICAL EPISTEMOLOGIES", organized by David Rabouin, which considers what historical figures whose name is at the present day attached to the program of historical epistemology can contribute to this perspective. Moreover, these two symposia are part of a long-term program on the topic that will be developed by SPHERE.

The first line of inquiry that will be explored in the symposium is to consider the interplay between elements of practice and the collectives that are sharing them.

K. Vermeir will revisit some of the "style" concepts that have been introduced by publications dealing with historical epistemology. Distancing himself from uses of "style" concepts that can lend themselves to essentialist conclusions—a key problem for all contextualist approaches—, K. Vermeir suggests introducing an approach to style with a pattern of family resemblance. In the historical situation analyzed, the style is precisely what allows the historian to perceive a collective of discussion, when historical evidence of exchange among actors is lacking. By being "shared" according to an open-ended pattern, the style is what reveals a context. The identification of this context is essential for the task of interpretation, which is the main issue for which a new form of contextualism in history and philosophy of science is badly needed.

N. de Courtenay's talk will bring to light how the collective dimension of scientific activity is inscribed in specific theories and practices of measurement. She argues that one can account for the specificities of these theories and practices only if one considers that they take into account the problems attached to the fact of sharing data. This case study discloses how theories and practices were shaped under the assumption that measures will be shared and need to remain meaningful through communication. Particularly interesting is how the circulation of measures redefines permanently a collective of work and how practices were elaborated to sustain the openness of the collective.

In a second line of inquiry, the symposium will examine critically new approaches to "ways of seeing" and "ways of hearing."

M. Kusch will contrast two approaches to the historicity of "ways of seeing," on the one hand, W. Benjamin's 1936 essay, and, on the other hand, Daston's and Galison's joint book on objectivity. The focal point of his contribution will be to discuss the kind of "collectives" and "social factors" brought into play, when these two approaches strive to account for changes in "ways of seeing."

On the other hand, J. Kursell will consider which light is shed on some of the concepts brought forward in H. J. Rheinberger's brand of historical epistemology when they are considered from the viewpoint of the history of hearing. On the one hand, J. Kursell aims at highlighting the factors that must be taken into account to show how "ways of hearing" change. On the other hand, she is interested in revisiting some of the concepts of historical epistemology from the viewpoint of hearing as an object of investigation.

E. Grosholz's contribution will focus on some fundamental modes of representation collectives of practitioners shaped as tools for reference or as tools for analysis. Taking "time" as her key example, she plans to bring to light how distinct modes of representation for reference and analysis were shaped in distinct contexts. Moreover, her emphasis is on the fact that the work done to integrate these different modes of representation in the distinct contexts awaits further research.

In a final contribution, T. Sturm will offer a critical view of the multifaceted expansion of the field of historical epistemology today. His goal is to warn

against the pursuit of too many different goals that may jeopardize the whole enterprise. He will conclude with suggestions of guiding lines that could be kept in mind for a better development of this field of inquiry whose relevance was in recent years again strongly perceived.

Contributions

- **Style concepts in historical epistemology**

  Koen Vermeir

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- **Measurement and the social dimensions of scientific knowledge**

  Nadine de Courtenay

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- **'Ways of seeing' in Benjamin and historical epistemology**

  Martin Kusch

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- **Reference and analysis in the study of time: Classical mechanics, thermodynamics, modern cosmology**

  Emily Grosholz

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- **How might epistemology be historicized? Seven lessons from the debate over naturalized epistemology**

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Symposium

Poincaré, Philosopher of science: A historical and philosophical approach

Organizer: Augusto J. Franco de Oliveira

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Henri Poincaré's mathematical work deals with different problems and their interactions. This diversity led Poincaré to a global vision of mathematics allowing him to tackle the several problems in which he has been interested from different points of view. But, further his many contributions in Mathematics Poincaré was a special sort of genius and the fact that he could make a meta-reflection on his scientific work arouse a way of thinking which is always difficult to classify. We want to divide our symposium in two main sections in order to broach the issue from two big topics in Poincaré's thinking. That is, on one side the interaction between Physics and Mathematics and some of the diverse implications of this interaction. And, on the other side the topic of intuition and the Philosophy of Mathematics.

I. The Interaction between Physics and Mathematics

Starting from a deep work on differential equations, Poincaré tried to apply the results obtained in that field to celestial mechanics, producing a "new age" on this area which led to the development of chaotic dynamical systems, and which makes him worthy of the name "Prophet of the Chaos" given by I. Peterson. In general, Poincaré's works on celestial mechanics are founded on the study of some remarkable properties of dynamical equations.

Poincaré studied the change of variables which keep invariant the canonic form of equations, and showed that this change of variables makes easier to put in equations the three-body-problem. He tackled this problem in a qualitative way and its innovative approach led him to the discovery of the science of chaos. Although the theoretical work of Poincaré was sufficiently clear from the existence of chaotic deterministic systems, this fact has became commonly accepted by the scientific community through the work of the meteorologist Edward Lorenz, many years later. However, the evidence provided by this work was only possible due to the use of a computer. So, in order to understand the importance of Poincaré's work in dynamical systems and the historical guidelines of the development of chaos theory, it is essential to take into account the new tools that technology provided to the progress of the science. In turn, the concept of chaos theory has extensive applications in current research in natural sciences and engineering. Also, in recent decades chaos theory has dominated economical thinking and decision making in financial markets and has played a key role in the development of information systems. A situation which forces us to describe how important is the integration of technology in science, which no doubt is a matter of interest to the general scientific community.

As mentioned, Poincaré's work on differential equations has been very rewarding for the affirmation of dynamical systems as sub-area of mathematics

and, in particular, for the birth of chaos theory. However, Poincaré also tried to construct a new class of functions which should allow integrating all differential linear equations. Inspired by the work of Sophus Lie, Poincaré was led to the discontinuous groups of hyperbolic transformations. The study of this group showed him the way to the idea that all the transformations constitute a group. As everybody knows, the notion of group is the ground of Poincaré's philosophy of geometry, usually known as geometric conventionalism. This philosophy was extended later to his conception of physics. Different interpretations existing in the literature discuss the coincidence (Giedymin, 1977) or divergence (Paty, 1992) about Poincaré's geometrical and physical conventionalist thesis. However every expert agrees that Poincaré's geometric conventionalism is in the origin of his conventionalism in natural science. Nevertheless, we pretend to show how an analysis of the notion and the role of hypothesis in Poincaré's main philosophical papers, would clarify Poincaré's philosophy of science in the framework of other positions regarding to science that took place at the end of the 19$^{\text{th}}$ Century, such us neo-kantism, positivism, pragmatism, etc. We suppose that the explanation of the nature and role of "hypothesis" would allow a clear interpretation of his general philosophy of natural science.

Now, it is possible to say that some of Poincaré's scientific views on physics and mathematics have always been so deeply linked that it is very difficult to disentangle which part of his positions belongs to mathematics and which to physics. On the contrary, we want to insist on Poincaré's global mathematical view in order to defend that only a universal mathematician such as Poincaré could make so many important contributions, both in Mathematics and Physics. We are referring to the comparative study of the electromagnetic theories that were held at his time. The discussion between these theories was based again on his work on differential equations, but this time, his view was increased with philosophical positions regarding science and guided by the conventionalist thesis. This research was published in the book *Electricité et Optique*. Here Poincaré compares, among others, the competing electromagnetic theories of Ampère, Helmholtz and Lorentz, showing the relevance of the mathematical structure of these theories. Accordingly, we intend to work out the relationship between physics and mathematics in Poincaré's work.

II. Intuition and Philosophy of Mathematics

This reflection could not avoid one of the essential instruments of mathematical thinking: intuition. The importance of intuition in mathematics is indisputable. Since Descartes and Kant, the role of intuition has been emphasized and pointed out. Descartes built his mathematics as well as his metaphysics on the basis of the intellectual intuition. Kant considered the *a priori* forms of intuition (space and time) as characteristics of two essential mathematical branches such as Geometry and Arithmetic. It is often said that the concept of intuition plays a fundamental role in Poincaré's philosophy of mathematics. But the problem is that Poincaré was not systematic in the task of clarifying what he meant by intuition. For example, if we take a text such as *Les Mathématiques et la Logique* (Chapter III, Book II, of *Science et Méthode*), we can identify at least five different uses of the word "intuition" in less than three pages. As a result, we suppose that a new reflection on this concept and its links with the

notion of induction would be able to shed new light on the understanding of the difficulties in which the future intuitionist positions were involved.

In fact, the role of intuition in mathematics is a common thread in all of Poincaré's stands in the philosophy of mathematics. Besides arithmetic, this is quite in evidence in his arguments based on a kind of geometric intuition (of continuity) against claims by the set theorists and against what he considers to be unintuitive axioms (infinity, choice and power set). We intend to clarify the role of intuition in Poincaré's conception of the continuum and of the foundation of the infinitesimal calculus, particularly his endorsement of infinitesimals.

The relevant role of intuition explains Poincaré's involvement in the debate with the most important mathematicians and logicians of his time. At the turn of the century, mathematics was suffering great transformations. The study of Frege's writings on the foundations of Arithmetic lead Russell to discover his famous Paradox and shook the whole logicist program as it stood then, and stirred up many foundational issues. Poincaré argued several times against the trend of removing intuition from mathematics, appealing to its essential creative role. Our purpose is to provide the understanding of the argumentations context as well as his consequences for the development of mathematics.

Poincaré's views in the philosophy of mathematics was very well known and appreciated at his time, whereas today it is less considered. Nowadays, the set of his papers seems to us rather detached, puzzling or even contradictory. To understand Poincaré's arguments it is necessary to be careful in analyzing the difficult issues that he dealt with, as well as his sometimes conflicting texts. The same problem arises when one wants to qualify Poincaré in a determinate philosophical tradition. Poincaré has sometimes been qualified as anticipatory intuitionist: "pre-intuitionism" (Brouwer, 1951) or "expansive intuitionism" (McLarty, 1997), just to name only two well known philosophical classifications. However, Poincarés position in itself is not homogeneous, so we want to analyze the different *nuances* in his intuitionism in order to give a panoramic point of view.

So, differential equations, dynamical systems, conventionalism and hypothesis in physics, the relationships between mathematics and physics, the intuition and Poincare's position towards logicism, formalism, and intuitionism are some of the points that we want to discuss in this symposium. Our aim is to show that the most eminent citizen of Nancy is still alive in today's philosophy of science. We believe that his deep insights may still be useful for both the scientific community and the layman. In doing so, we aim to reproduce the objective which encouraged Poincaré to his most popular books such as *La Science et l'Hypothèse* or *La Valeur de la Science.* All in all, we seek to keep the nobility of Poincaré's spirit and to clarify some obscure points in his thought.

Contributions

- Poincaré as founder of Deterministic Chaos Theory

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- **The role of hypothesis in Poincaré's natural philosophy**

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- **Poincaré's thinking: From mathematics to physics**

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- **Several concepts of intuition in Poincaré's philosophy of mathematics**

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- **Poincaré's anticipatory intuitionism**

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- **Poincaré and the infinitesimals**

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Symposium

Confronting French roots and current historical epistemologies

Organizer: David Rabouin
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In recent years, "Historical epistemology" has emerged as a convenient label for new ways of bringing history and philosophy of science together. On the one hand, historians of science understand historical epistemology as both a philosophical underpinning of their work and a heuristic tool. Some of them aim at uncovering the historically situated conditions of a field of knowledge, of epistemic virtues or of scientists' styles of inquiry. Others study fundamental scientific concepts, which organize knowledge in different historical periods, along with the contingent conditions for their permanence or transformation. On the other hand, philosophers of science interested in historical epistemology develop new theories of concept formation and the naturalization of epistemology and think through the philosophical consequences of the social dimensions and historicity of knowledge. This profusion of usages has however raised the question of the unity of this trend and provoked the need to reflect on it from a critical and methodological point of view. This will be the general aim of this symposium, in close connection with that organized by Karine Chemla and Koen Vermeir (laboratoire SPHERE, CNRS): "A PLURALITY OF CURRENTS IN TODAY'S HISTORICAL EPISTEMOLOGIES".

Even if some actors of this trend, like Ian Hacking (and more recently Lorraine Daston), keep a certain distance from the name itself and propose very different ways of undertaking this program, they all make central reference to the French tradition in which the term "historical epistemology" was coined. However, it is very rare that these references, which are often limited to Canguilhem and Foucault, go beyond general claims. The aim of our symposium is to assess more precisely the link between these two periods of "historical epistemology". This goal supposes, of course, going back to some of the main figures of the past, and first of all to Canguilhem, whose conceptualization of the "normal" state will be studied by Cristina Chimisso (Open University) in the context of the tradition of historical epistemology. But we would also like to pay particular attention to authors like Alexandre Koyré, who are not often mentioned in this tradition. Indeed, as will be pointed out in Maarten Van Dyck's (Ghent University) paper, Koyré played a very important role in the introduction of Hegelian philosophy in France and it is possible to detect this influence in his studies in the history of science. We would also like to contrast this French tradition with contemporary attempts to define "historical epistemology" in other countries, such as that proposed by Marx Wartofsky (which will be studied by Katharina Kinzel, from the University of Vienna). We would like also to pay attention to less studied authors from the French tradition like Gilles Gaston Granger, who proposed in 1968 a first epistemological general description of a notion which is now central in "historical epistemology", that of "style". Finally, Paolo Savoia (University of Pisa) will focus on some of the works written by American

scholars under the label of historical epistemology in the past 20 years (Daston, Galison, Hacking, Davidson), and will try to connect them to their French roots.

Contributions

- **Life, health and history: a re-assessment of Georges Canguilhem's concept of the normal state**

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- **Alexandre Koyré, Hegelian**

  Maarten Van Dyck

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- **Wartofsky's historical epistemology and the critique of philosophical problems**

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- **Mathematical style in Gilles Gaston Granger and what can be done with it**

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- **Current historical epistemology and the history of the human sciences**

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Symposium

Thomas Kuhn's "The Structure of Scientific Revolutions": Interpretations and developments

Organizer: Friedrich Stadler

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The Symposium is intended to present some results of the research project *History of Science and/or Philosophy of Science* (Austrian science fund, grant P21750, project leader: Friedrich Stadler). The goal of the project is to reconstruct the radical discontinuities and processes of re-orientation in the philosophy of science between 1965 and 1995, with a focus on the European scene. This research is projected under the aspect of the historical and pragmatic turn of philosophy of science, which has rightly been termed the "mega trend of philosophy" (Hoyningen-Huene) in the second half of the 20th century, as well as the philosophical reactions to this trend. The project wants to place this philosophical confrontation in its historical context with particular reference to previously unpublished archival materials (Stegmüller) or to such sources, which have only sporadically and superficially been considered (Feyerabend, Popper, Kuhn and Lakatos). The project's point of departure for studying this crucial turning point in the history of 20th century thought is the London Colloquium in the Philosophy of Science (1965), which was attended by several of the principal figures in our investigation. The period studied in this project ends around 1995, when some of the main figures of philosophy of science crucial for this debate died (Popper, Feyerabend, Lorenzen, Stegmüller and Kuhn).

In our symposium we exemplify the general goal of the project just described in the context of the variety of interpretations of Kuhn's *The Structure of Scientific Revolutions* that can be found especially in Europe. For that purpose we investigated a number of sources that were not sufficiently considered by the research community until now. (1) In the papers of Kuhn (Boston) and Lakatos (London) we investigated correspondence, notes and drafts of papers that document the development of the London Colloquium in the Philosophy of Science (1965) and the publication of its proceedings. (2) the project for the first time had access to the estate of Wolfgang Stegmüller which contains correspondences with Kuhn, Feyerabend, Popper, and numerous other key figures of philosophy of science. (3) Christoph Limbeck-Lilienau's talk draws i.a. on early lectures (like the Lowell Lectures from 1951) and other sources from Kuhn's papers at the MIT.

One of the key events that started the debate on Kuhn is the London Colloquium in the Philosophy of Science (1965). Thus two contributions will put the focus on its prehistory as well as on the actual debate that took part during the congress and on the development of the proceedings of the session on *Criticism and the Growth of Knowledge.*

Hans-Joachim Dahms' paper will focus on the congress on *Scientific Change* that took place in England four years prior to the London Colloquium. Kuhn appeared there for the first time on the European scene and was perceived

by many, especially the Popperians, mainly as a narrow-minded propagandist of dogmatic "normal science". The paper will show how and why this image changed dramatically during the 60ties.

Michael Schorner's paper will consider the reasons for the organization of the Colloquium at that time in London. It will also provide a closer look at the actual discussion that considerably differed from the published proceedings. Based on yet unpublished archival material, it will also scrutinize the impact the symposium on *Criticism and the Growth of Knowledge* had in the philosophy of science.

Christian Damböck's paper presents the structuralist view of theories as developed in the seventies of the last century by Joseph Sneed, Wolfgang Stegmüller and others as a formal approach to the structure of scientific theories that does not build an anti-thesis to Kuhn's historical and psychological program but rather a complementary analysis of the same topic. The idea that philosophy of science should integrate both a formally-normative and a historical perspective is considered as a fruitful perspective, especially in the context of recent developments like the so-called third wave of science studies that also tries to weaken down the dogmatic standpoint of the "strong program", on the basis of a sociology of knowledge that considers normative models again.

Christoph Limbeck-Lilienau's paper will analyze how Kuhn contested the separation of psychology from philosophy and history of science especially with his use of cognitive psychology to defend the theory ladenness of perception. The paper shows how Kuhn developed this naturalistic approach and how it was received by the logical empiricists and defendants of the standard view. Although recent debates on the theory-ladenness whole-heartedly accepted Kuhn's naturalistic approach, the specific conclusions Kuhn drew from psychology are still highly contested.

Contributions

- **Thomas Kuhn in England**
  Part I: Before and after the London Colloquium 1965

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- **Thomas Kuhn in England**
  Part II: The London Colloquium in the philosophy of science 1965

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- **Thomas Kuhn and the structuralist view of scientific theories. The formal and the historical perspective of theory change**

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- **Kuhn's naturalistic approach and the theory-ladenness of observation**

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Symposium

Carnap's linguistic pluralism and scientific methodology

Organizer: Richard Zach

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In a famous passage in his 1934 *Logical Syntax of Language,* [2] Carnap first introduced the idea into his philosophical framework that some questions of the philosophy of science were to be settled not on the basis of evidence, not as a theoretical matter, but by making a practical decision:

> Our attitude to demands of this kind may be stated generally by the principle of tolerance: we do not want to impose restrictions but to state conventions... In logic there are no morals. Everyone can construct his logic, i.e. his language form, however he wants. If he wants to discuss it with us, though, he will have to make precise how he wants to set things up. He has to give syntactic rules rather than philosophical considerations. [2, p. 45]

This "principle of tolerance" became a central doctrine of Carnap's post-Vienna philosophy of logic and science, yet Carnap's critics, including most prominently W. V. O. Quine, did not engage with it or take it seriously. The importance of the principle, and the misreadings of Carnap's philosophy due in part to insufficient attention to it, have become the topic of significant attention in recent years. The proposed symposium is a contribution to this re-evaluation of Carnap's philosophical methodology by emphasizing the role of the principle of tolerance. The contributions by Carus, Creath, and Wagner focus on the principle of tolerance at work in different aspects of the later Carnap's conception of scientific method. This is of particular importance, since while the above quotation and its ramifications in the *Logical Syntax* itself, and other writings of the early 1930s, have now been widely studied (e.g., the papers in [7]), the pervasive pluralism of Carnap's later writings is rarely taken seriously. These contributions focus on different aspects of the later period. Wagner addresses an issue that has often been used to dismiss Carnap's pluralism, the objection that if the language can be freely chosen, then this choice must be arbitrary. If it is argued that the choice can be rational, then it in turn presupposes a framework

language, and so on, so there is an infinite regress. Wagner rejects these conclusions, and shows how Carnap's notion of a *pragmatics,* distinct from syntax and semantics, is precisely what is required here. Pragmatics, in Carnap's own sketchy remarks on the subject, is the aspect of the metalanguage that pertains to the language users; it is the theory of language in *use,* not considered only abstractly in referential contexts (semantics) and considered only syntactically. Pragmatics thus includes questions of choice among entire languages and also among particular localized "explications," replacements of particular terms by more precise ones. Wagner points out that Carnap devoted considerable attention in his later years to questions of choice among theories involving the use of formal decision theory, and notes that Carnap himself never limited the scope of his decision-theoretic investigations to the theoretical realm. Accordingly, Wagner probes the consequences of extending them to the realm of rational choice of languages and meta-languages.

Carus goes even further and includes the theory of normative principles or values under Carnap's heading of pragmatics, taking his starting point from Carnap's own exposition of a logic of normative sentences, [3]. It is often suggested by theorists who reject G.E. Moore's "naturalistic fallacy" argument, or Hume's argument that no "ought" can be derived from an "is," that this, too, leaves decisions about action-guiding principles without any possible form of rational justification. Carnap's form of Hume's distinction, Carus argues, is not open to this objection, or at least not to the form of this objection that has been levelled at R.M. Hare's [4] well-known formulation, though Carnap's logic of normative statements closely parallels Hare's. Carnap's exposition, however, is motivated by his logical pluralism; indeed, it has been argued by Richardson [5] that Carnap's strict non-naturalism is rooted in Kant's distinction between practical freedom and cognitive determinism.

Creath's contribution extends the realm of pragmatics, in the context of Carnap's later pluralism, in yet another direction; in his contribution we see that the interpretation of theoretical sentences of a scientific language as their Ramsey sentences, designed originally to escape Hempel's critique of Carnap's earlier versions of an observational-theoretical distinction, is of interest in understanding his later philosophy more generally. While it may be unsuitable to answer the objections it was designed to counter, the Ramsey-sentence interpretation of theories serves to highlight Carnap's pluralism about theoretical concepts and his conception of their structural character and plasticity. Moreover, Creath observes that what has often escaped unnoticed about the Ramsey sentence interpretation is that observational concepts are equally language-relative in Carnap's later view. This will surprise those who regard the language-relativity of observational concepts to have been the invention of Kuhn. If one looks closely, however, it will be seen that the language-relativity not only of observational concepts, but even of particular observational sentences, is already explicitly put forward in the *Logical Syntax.*

One benefit of studying the later manifestations of Carnap's linguistic pluralism, then, is to discover the deep underlying continuities in his thought across the different periods. This continuity comes out even more clearly in Zach's contribution, which goes back to the period just *before* Carnap's embrace of tolerance—his attempts to mediate between different philosophies of mathematics in the late 1920s by means of a neutral "basic system" *(Grunddisziplin)* in which the rival foundational systems of logicism, intuititionism, and formal-

ism could all be articulated and compared. This was put forward as the basis of a "General Axiomatics," the project on which Carnap spent much of the period 1927-29, immediately after the completion of the *Aufbau.* In this period, prior to his adoption of a quasi-Hilbertian ("syntactic") view extended to the whole of knowledge in early 1931, Carnap still hoped to find a way of reducing the meta-language to the object language of the *Grunddisziplin,* so as to restrict all meaningful discourse to a *single* language of science, in the spirit of the Vienna Circle's "tautologicism" (as Steve Awodey has called it). However, in the course of considering how to ground the *Grunddisziplin,* in an unpublished 1929 manuscript suggesting a "new foundation of logic," Carnap considers different possibilities for the constitutive rules of the language, foreshadowing the later L-rules and P-rules of the *Logical Syntax.* While the object language itself retains its fixed interpretation in terms of atomic sentences, the idea of defining languages by syntactic (ultimately semantic) rules was already under way.

The symposium as a whole, then, casts new light on the many different aspects of Carnap's logical pluralism, which has largely been associated only with the extreme (though much-misunderstood) "syntax" doctrine put forward in 1934. What these papers seek to do is to see it more broadly as it manifests itself in many different forms both before and after the "syntax" doctrine narrowly defined, and to show how it is a pervasive theme in Carnap's philosophy through his entire career, and makes many aspects of his apparently abstruse investigations of formal languages more readily accessible, showing them to be motivated by an overall unifying theme.

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Contributions

- Carnap's principle of tolerance as a basis for reasoning about values

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- Carnap's linguistic pluralism in observation and theory

Richard Creath

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- Rational choice, decision theory, and the principle of tolerance

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- Carnap, Tolerance, and the Foundational Debate in Mathematics

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C. Methodological and Philosophical Issues of Particular Sciences

C1. Logic, Mathematics and Computer Science

Invited Lectures

Mathematical abstraction, variation and identity

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Contemporary mathematics is undeniably abstract. This assertion is meant to describe a specific feature of 20th century and contemporary mathematics, as a way of distinguishing the latter from earlier forms of mathematics. Thus, the claim is not merely that mathematical entities are abstract in an ontological sense, that is they are causally inert or lacking spatial and temporal properties or any similar attribute. Furthermore, mathematicians often talk about levels of abstraction within mathematics. In a nutshell, contemporary mathematics is, in some sense, more abstract than earlier forms of mathematics. But what does that precisely mean? Some would say that it merely reflects the rise of set theory as a foundational framework, others that it is another way to underline the systematic use of the axiomatic method. Although I do believe that these two ingredients are indeed part of the explanation, I will instead present a picture of abstraction as an epistemological process. There are, I believe, specific logical properties involved in the process of abstraction and these properties are noticeable in the rise of abstract mathematics in the first part of the 20th century. In this talk, I will first focus on some historical evidence exhibiting what I take to be the key ingredients of the abstraction process: the presence of genuinely different mathematical contexts, the difference being logically salient — in fact some contexts are even logically incompatible —, the search for the right properties at work in certain proofs and computations and, finally but of the utmost importance, the extraction of the proper criterion of identity for a new type of abstract entities. Thus, although abstraction as a process was decried by many philosophers at the beginning of the 20th century as being too psychologistic, I claim that it has distinct logical properties which allows us to distinguish it from generalization and even tie it to specific cognitive processes.

1. The author gratefully acknowledges the financial support of the SSHRC.

Synthetic connectives and their proof theory

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Gentzen's sequent calculus is equipped with three kinds of inference rules. The *identity rules*—cut and initial—can be eliminated except for atomic instances of the initial rule. The *introduction rules* are the "atoms" of inference: they describe how to reason *from* and reason *to* logical connectives via their introduction in the left and right sides of sequents. The *structural rules* of contraction and weakening provide a means to distinguish between proofs in classical, intuitionistic, and minimal logics.

Girard's linear logic has expanded and clarified the relationships between introduction rules and structural rules. In particular, the introduction rules for the propositional connectives come in *additive* and *multiplicative* forms. The logic of multiplicative and additive linear logic, MALL, forms an expressive core of logic with decidable provability. MALL can be extended to full linear logic with the addition of the *exponentials* which mark formulas that can be contracted and weakened and which relate the additives and multiplicatives in the expected way. MALL can also be extended with least and greatest fixed points in order to provide a logic more attuned to arithmetic and inductive reasoning.

In recent years, a new class of sequent calculus proof systems, called *focused proof systems*, has being used to expand our understanding of how introduction rules and structural rule relate to each other. In these proof systems, inference rules and logical connectives are polarized as negative or positive in such a way that the invertible inference rules all belong to the negative polarity. Groups of negative connectives can then be grouped into one negative synthetic connective: similarly, positive connectives can be grouped into a positive synthetic connective. Such synthetic connectives admit cut-elimination. Remarkably, focused proof systems for classical and intuitionistic logics can be organized so that negative formulas are, in fact, treated linearly. That is, if weakening or contraction is applied to a formula, that formula is positive.

Focused proof systems can be used to design richly varying collections of synthetic connectives. These proof systems also provide for new means of describing parallelism within proofs and mixing computation and deduction. The ability to treat negative formulas linearly provides important information for the design of automated theorem provers. Synthetic connectives and their associated inference rules will also allow for the design of broad spectrum proof certificates that theorem provers will be able to print and simple proof checkers will be able to validate.

Contributed Papers

Corroborations of hypotheses and experimental computer science in software testing

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This paper provides a methodological analysis of Software Testing, the process of observing programs executions to examine whether they fulfil software requirements. The practice of corroborating hypotheses concerning executions of programs is framed in a *falsificationist epistemology* according to which one selects test inputs values that are likely to falsify the requirement. Methodologies concerning inputs selection and outputs acceptance are examined in the light of an *epistemology of scientific experiments.* Finally, it is put forward the claim that computer science is akin to an experimental science of computational systems that proceeds through conjectures and attempts of falsifying them.

Pluralism and mathematical objects

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The metaphysical project in philosophy of mathematics consists in examining mathematical theory and practice to determine whether abstract mathematical objects exist or not. In his (1998), Mark Balaguer argues that this project does not work. While I agree with his conclusion under his (and most everyone's) metaphysical assumption of a complete and ready-made monistic reality, I argue that a different, more satisfying conclusion might be reached if we forego this assumption, and allow for a pluralistic conception of reality, much like the one advocated by the American pragmatist William James.

Łukasiewicz and Lorenzen as interpreters of Aristotle's assertoric syllogistic

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Łukasiewicz restored the original conception of aristotelian syllogistic. Besides the rejection of singulars terms, he showed that aristotelian syllogism is not an inference, but an implication. But he reduced syllogistic necessity to the validty of the retained moods for all variables represented by the letters A,B,C. Lorenzen did not follow Łukasiewicz on this point: he showed that "logical implication" is necessary because it is founded on the evidence that Aristotle exhibited in the syllogisms of the first figure. The originality of Lorenzen is to replace "perfect syllogisms" by "normalized syllogisms" which preserve the same evidence.

Fuzzy logics as the logics of linearly decomposable resources

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Girard's linear logic is often informally interpreted as the logic of resources, while fuzzy logics are usually understood as logics of partial truth. I shall argue that fuzzy logics are interpretable in terms of resources, too, and that in typical situations they turn out to be more adequate logics of resources than linear logic, which only captures a very general structure of resources. The resource-based interpretation of fuzzy logic makes them applicable to various areas of resource-aware reasoning, including models of the notion of feasibility and a solution to the logical omniscience paradox.

Plural Logicism

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PG (Plural *Grundgesetze*) is a consistent second-order system which is aimed to derive second-order Peano arithmetic. It deploys the notion of *plural quantification* and a few Fregean devices, among which the infamous Basic Law V. George Boolos' plural semantics is replaced with Enrico Martino's *Acts of Choice Semantics* (ACS), which is developed from the notion of arbitrary reference in mathematical reasoning. Also, *substitutional quantification* is exploited to interpret quantification into predicate position. ACS provides a form of *logicism* which is radically alternative to Frege's and which is grounded on the existence of individuals rather than on the existence of concepts.

Are mathematics and logic sciences of observation? A semiotic approach to visual thinking

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In this paper I develop a semiotic approach to visual reasoning for logical and mathematical objects: their spatial nature or the possibility to interpret them as diagrammatic relations solves many problems about their ontological status. One often wonders if semiotic does more than just adding technical jargon over a representational conception. I argue to the contrary that it rules out some metaphysical commitments of representationalism. Thus, in spite of some difficulties to reduce the whole of logic and of mathematics to visual thinking, I claim it is worth trying.

Frege on "contentful mathematics"

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The argumentation evolves around the thesis that *mathematics is contentful,* which is seen as the key to a better understanding of Frege's philosophy. The connection between Frege's views on language and mathematics are seen as an organic whole, and so the role of Frege's puzzle in his overall project becomes clear. I shall show how the distinction between sense and reference plays a crucial role in navigating safely between *formalism* and *psychologism,* and in finding a consistent middle ground between *intensionalism* and *extensionalism.* Fregean senses play an important role in the articulation of Frege's program in the foundations of arithmetic.

The computer (as a medium) in mathematics. Mathematician-computer interactions, internalization, time and space squeezing

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Media are no tools. Far more than "things at our disposal" they constitute the interaction of thinking and perception—mainly unconsciously.
Martin Carlé, 2011

Besides the fact that "*computers are eo ipso mathematical machines*", the first applications of computers were mathematical in nature. The fact that such an earthly machine has been introduced within the history of the so-called "queen of all sciences" should be seen as an opportunity to re-evaluate mathematics, its philosophy and its history. The aim of this talk is to challenge (the epistemology of) mathematics in the light of four fundamental concepts: internalization, mathematician-computer interaction, time and space squeezing. These come to the foreground if one starts from a practice-based study of computer-assisted mathematics in which the computer is really taken seriously, viz. as a medium and not as a mere instrument that stands at our disposal.

On the meaning of connectives (A propos of a non-necessitarianist challenge)

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According to logical non-necessitarianism, every inference may fail in some situation. In his defense of logical monism, Graham Priest has put forward an argument against non-necessitarianism based on the meaning of connectives.

According to him, as long as the meanings of connectives are fixed, some inferences have to hold in all situations. Hence, in order to accept the non-necessitarianist thesis one would have to dispose arbitrarily of those meanings. I want to show here that non-necessitarianism can stand, without disposing arbitrarily of the meanings of connectives, based on a minimalist view on the meanings of connectives.

Physical computability, efficiency, and the church-turing thesis

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Which functions can be computed by physical processes, in general, and how efficiently? To answer this question, one first needs a sufficiently precise specification of which physical processes under which circumstances count as "computing" a function. Second is that of "can"—in what sense must a process counting as a physical computation be possible for it to be relevant to the above question? Attention to the second part of the question reveals the difficulty of finding an unambiguous but general way of counting computational resources. For each of these points we give illustrative examples from the literature.

Motivating Wittgenstein's perspective on mathematical sentences as norms

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A motivation of the later Wittgenstein's perspective on mathematical sentences as norms is given for sentences belonging to axiomatic systems that are treated along the lines of the Hilbertian axiomatic method, the approach in which the axioms are used as implicit definitions of the concepts they contain. It is shown that in this approach the axioms are employed as norms in that they function as standards of what counts as using the concepts involved. This normative dimension of the mode of use of the axioms, it is argued, is inherited by the theorems derived from the axioms. Having been motivated along these lines, the Wittgensteinian perspective on mathematical language as normative may appear more plausible also to those who are not friends or experts of Wittgenstein's later philosophy of mathematics.

Presenting pluralism in mathematics

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I shall be presenting a version of Pluralism. I say "a version" because there are several. Versions vary along two axes. One is: what it is one is pluralist about; the other is: how one deals with the issues of truth, and conflict between claims, in and about mathematics. This Pluralism is a Pluralism about foundations in mathematics, and where truth can only make sense within a mathematical theory. A mathematical theory itself is not true. This Pluralist arranges discussions in and about mathematics into a hierarchy. She works with conflicting claims by referring to the methodology of adaptive logics.

Exlog: a non-standard logic programming language for experiment-based research

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We present a logic programming language based on a non-standard logic, which combines features of a discrete multi-valued logic with those of inductive confirmation logic. We argue that this logic is especially well suited for representing the reasoning process used by the researchers in the disciplines which rely heavily on the use of experimental methods for deriving conclusions (e.g., biology and economics). A notable feature of this logic is the use of the positive and the negative confirmation operators, denoted by the symbols $\uparrow$ and $\downarrow$, which are used to represent successful and failed experiments, respectively. Using this logic as a base, we present a logic programming language, which we call EXLOG (from EXperimental LOGic). We argue that EXLOG can be used as a fundamental expert system shell for the representation of knowledge acquired from the results of experiments. Such a shell can be used to revalue the degree of certainty of those conclusions that follow logically from a given knowledge base acquired through experiments. One valuable feature of such an expert system is that the evidential results of seemingly disconnected studies can be kept in a common knowledge base, and thus avoiding the duplication of experiments in the pursuit of new research.

Frege on the iPad

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Logic is an old subject, and since 1879 it has been a great one says Quine and refers to **1879** as the year Frege publishes his *Begriffsschrift*. We briefly summarize some historic aspects related to Frege's two-dimensional Begriffsschrift and continue with an **explanation** and **démonstration** of a **Begriffsschrift Editor and Reasoning iPad app**, showing that modern tablet and touch panel

technology resonates with foundational aspects in Mathematics and Logic. We intend to boost interest in Frege's Begriffsschrift by making it accessible on a mainstream platform.

Axiomatizing set theory

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First-order axiomatizations like ZF are inadequate theories of sets. Their models are structures of particulars which do not yield much information about structures of sets. Set-theoretically interpreted, some theorems of ZF are false. Paradoxes are due to the incapacity of the received logic to implement non-circularity requirements on set-theoretical definitions. These requirements are not implemented in ZF type theories, either. Principles of reasoning like the "axiom" of choice cannot be expressed fully as set-theoretical axioms which are true only in the models of set theory unlike logical truths. A generalized IF logic makes entire set theory theoretically dispensable.

The duality of space and function, and category-theoretic dualities

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This talk concerns the use of the term "duality" in the sense(s) of category theory in various mathematical situations. Aims are (1) to classify these situations; (2) to explore (historically and epistemologically) the relationship between category-theoretic and classical dualities like in projective geometry or boolean algebra (in the case of category theory, is it still possible to exchange parts of the language with others salva veritate? Are there features analogous to, say, points at infinity in projective geometry?) (3) to relate category-theoretic duality to a basic methodological principle of modern mathematics, namely studying "spaces" by studying functions defined on them.

The Genetic Versus the Axiomatic Method: Resolving Feferman '77

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Feferman (1977) argues that category theory cannot stand as a structuralist foundation for mathematics; because the notions of operation and collection are both epistemically and logically prior, we require a background theory of operations and collections. This claim has been modified by Hellman (2003) and Shapiro (2005) to argue that category theory requires a background assertory

theory. I borrow Hilbert's (1900) distinction between the *genetic method* and the *axiomatic method* to argue that even if the genetic method requires the notions of operation and collection, the axiomatic method does not. Thus, the category axioms can serve as a structuralist foundation.

Representing the 'universe of discourse': historical origin and philosophical relevance of a graphical convention in mathematics and logic

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It is common in modern mathematical and logical manuals to represent (with a rectangle) the universe of discourse around a Venn diagram. The aim of this paper is to discuss the historical origin and the philosophical relevance of this graphical convention. After introducing the notion of universe of discourse as understood by the nineteenth century logicians who introduced it, we will compare two graphical methods: one that represents the universe of discourse (Carroll diagrams) with another that doesn't (Venn diagrams). The aim is to determine precisely what it does change to represent the universe in a logic diagram.

Kant, Cantor, and the Burali-Forti's paradox

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The Burali-Forti's paradox demonstrates that the class of all ordinals is not a set. Cantor, however, had no concern over the paradox, since his conception of a set as "any plurality that can be thought of as unity" did not create any contradiction. It will be argued that the Burali-Forti's paradox is a *logical* paradox ('para'+ 'doxa'), which is solvable, whereas, following Saarnio, it is an *epistemic* antinomy ('anti'+ 'nomos') *á la* Kant, which remains unsolvable in principle. Thus, Cantor proposed e.g. in 1899 that the system ω of all ordinals is an *inconsistent, absolutely infinite multiplicity,* which is impossible to think as whole without contradiction.

On the indispensable premises of indispensability arguments

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We identify four different varieties of the indispensability argument (IA)—an epistemic argument for semantic realism, an epistemic argument for platonism, and a non-epistemic version of both—and for each we present a minimal

version. Current formulations of IA can be recovered by building upon the suggested minimal versions. We clarify the notion of (in)dispensability as relational in character. The minimal versions of IA show that i) both naturalism and confirmational holism can be dispensed with in the argument, and that ii) IA for semantic realism avoids some common criticism moved to standard platonist versions.

A realist modal-structuralism

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Modal-structuralism (MS) is a recent approach to the philosophy of mathematics that attempts to get away with two major issues, the *axiomatic set theory* as the foundational basis for all mathematics, and *actualism* that characterises well-known versions of commonplace *(ante rem)* structuralisms. It takes mathematics to be about properties of structures the existence of which is conditional on the assumption of the existence of those structures that they are the properties of. That is, mathematics is about *logically possible structures*. Geoffrey Hellman's proposal aims at taking into account the actual practices by which mathematics is being done. I argue that it comes close, but not close enough, to the *pragmatist* philosophy of mathematics originally proposed by Charles Peirce a century ago. Philosophy of mathematics, just as any good philosophy, should exemplify relevance to the reality of mathematical conduct.

Computational structuralism and Frege's constraint

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Computational structuralism has been presented in [1], and to some extent also in [?], as a particular type of structuralism: the authors reexamine the structuralist position under different angles and indicate that $PA1$ together with the assumption that addition and multiplication have to be computable, is the most adequate method for singling out the intended model of arithmetic. In this paper we show that also starting by reconsidering the contexts of applicability (that is, taking into account a "Frege's constraint"[2], [3], for natural numbers, leads to the similar conclusions.

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Remarks on recursive definitions of truth

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For the sake of simplicity, we adopt the same logical frame as Tarski's in his *Wahrheitsbegriff* (Wb). There, Tarski is mainly interested in the possibility of explicitely defining truth for an object-language, he does not pay much attention to recursive definitions of truth. We say why. However, recursive definitions have advantages of their own. In particular, we prove the ***positive* theorem**: *if L is of finite order ≥ 4, then a recursive definition is possible for L in a metalanguage of the same order as L.* We indicate how this result could be used for a solution of a generalized version of Frege's paradox.

Raise and fall of scientific branches: On progress in mathematics

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We reflect on the various conceptions of progress in science and emphasize its relevance to science policy in the era of big science (in the sense of de Solla Price). More precisely, we argue that the departure from exponential-growth small science results in a competition for limited resources such as funding and students which causes individual branches to progress not necessarily monotoneously. We suggest causes for a branch (particularly in mathematics) to flourish or to stagnate and even fall into (temporary) oblivion.

"Muss Logik für sich selbst sorgen?" On contrary propositions and material logical truth as problems to the neutrality of logic

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This contribution deals with the abandon of *Tractatus* which begins with the partial failure of the truth-functional paradigm imbedded in the truth table notation limitations. The truth functionality pattern was not useful to the tractarian aim to analyze *all* empirical propositions, due to its lack of sensitiveness to capture material logical truth and, mainly, contrary propositions (*e.g.* in ascription of grads to empirical qualities). Some propositions are conceptually linked; hence their interpretation cannot be entirely independent, but have to occur in a system. This assumption is linked to the new account of Logic, which cannot be any more strictly neutral or formal. It has begun to depend on a large number of substantive non logical facts.

Ontology without abstract objects: A naturalistic defense of revolutionary fictionalism

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Mathematical fictionalism has important influence on the debates over realism and anti-realism. Among all the responses to fictionalism from mathematical realists, J. Burgess's objection is especially notable. In his "Mathematics and Bleak House" (2004), Burgess confronts revolutionary fictionalism (RF) with a philosophical predicament. He argues that both strategies RF would adopt to refute realism, the denial of mathematical objects as Ultimate Metaphysical Reality and the denial of scientific theories' commitment to abstract mathematical objects, are subject to serious problem. This paper will advance two arguments against Burgess and show how his objections fail.

What can science tell us about mathematical objects?

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The most powerful weapon that the mathematical realists came across for defending their doctrine takes the form of an indispensability argument. Unfortunately for the mathematical realist, this argument is parasitic on another doctrine. The first thing I want to do in this paper is to emphasize the reasons for which the scientific realist should dislike the mathematical realist's company. In the second part of this paper I will try to show what from scientific realism allows the mathematical realist to parasitize this doctrine and I will argue that this realist is not in such a good situation as one may think at first sight.

Kant and real numbers

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Kant held that under the concept of $\sqrt{2}$ falls a geometrical magnitude, but not a number. In particular, he explicitly distinguished this root from potentially infinite converging sequences of rationals. Like Kant, Brouwer based his foundations of mathematics on the a priori intuition of time, but unlike Kant, Brouwer did identify this root with a potentially infinite sequence. In this paper I discuss the systematical reasons why in Kant's philosophy this identification is impossible.

Explanation and two kinds of investigation in the foundations of mathematics

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The paper takes up Maddy's principles of MAXIMIZE and UNIFY and their role in an important project in the foundations of mathematics, which it is claimed may also be regarded as an explanatory one on the unification view of explanation. It is argued that these principles, and the unification view, fail in characterizing various explanatory projects in foundations involving weak axiom systems. The paper describes an alternative account of mathematical explanation that is a kind of mathematical analogue of the causal theory of explanation, which better accounts for work on weak theories and solves other problems for the unification view.

How formalized are informal proofs?

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Using various developments in constructive type theory, I try to show that a philosophically salient model of informal proofs might not be as far out of hand as is widely believed—many aspects of informal proofs can be already modeled accurately using tools available within the constructive framework. This approach also prompts questions on the mutual relations of truth and proof in logic—is the notion of truth really basic for logic or should it be dethroned by the notion of proof (or even proving)?

Constructivism and metamathematics

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Constructivism is one of the major views in the foundations of mathematics. In fact, we have a variety of constructive approach to mathematics from semi-intuitionism to ultrafinitism. All constructivistic views claim that mathematical proofs should be constructive (= effective), but this idea is understood more or less radically. Logical basis of constructivism is sufficiently elaborated. For example, intuitonistic logic rejects some theorems of classical logic; the law of excluded middle is a paradigmatic case. On the other hand, the possibility of constructive metamathematics for effectively grounded formal theories is disputable. This paper discussed this question with respect to completeness and limitative theorems.

Contributed Symposia

Symposium

Theories of continua: Logical and philosophical reflections

Organizer: Philip Ehrlich
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In the decades bracketing the turn of the twentieth century the real number system was dubbed the *arithmetic continuum* because it was held that this number system is completely adequate for the analytic representation of all types of continuous phenomena. In accordance with this view, the *geometric linear continuum* is assumed to be isomorphic with the arithmetic continuum, the axioms of geometry being so selected to insure this would be the case. In honor of Georg Cantor and Richard Dedekind, who first proposed this mathematico-philosophical thesis, the presumed correspondence between the two structures is sometimes called the *Cantor-Dedekind axiom.*

Since their appearance, the late nineteenth-century constructions of the real numbers have undergone set-theoretical and logical refinement, and the systems of rational and integer numbers on which they are based have themselves been given a set-theoretic foundation. During this period the Cantor-Dedekind philosophy of the continuum also emerged as a pillar of standard mathematical philosophy that underlies the standard formulation of analysis, the standard analytic and synthetic theories of the geometrical linear continuum, and the standard axiomatic theories of continuous magnitudes more generally.

Since its inception, however, there has never been a time at which the Cantor-Dedekind philosophy has either met with universal acceptance or has been without competitors, and some authors, while embracing the Cantor-Dedekind conception, have sought to situate it within a wider conception of a continuum or a wider class of continua. The period that has transpired since its emergence as "the standard philosophy" has been especially fruitful in this regard having witnessed the rise of a variety of constructivist and predicativist theories of real numbers and corresponding theories of analysis as well as the emergence of a number of alternative theories that make use of infinitesimals or, as in the case of the first-order theory of real numbers, limit themselves to theoretically significant portions of the Cantor-Dedekind theory. Whereas the constructivist and predicativist theories have their roots in the early twentieth-century debates on the foundations of mathematics and were born from critiques of the Cantor-Dedekind theory, the infinitesimalist theories were intended to either provide intuitively satisfying (and, in some cases, historically rooted) alternatives to the Cantor-Dedekind conception that have the power to meet the needs of analysis or differential geometry, or to situate the Cantor-Dedekind system of real numbers in a grander conception of an arithmetic continuum.

The first-order (or elementary) theory of real numbers, on the other hand, was introduced to explore the part of the classical theory of (arithmetic and geometric) continua that can be formulated and established without the help of any set-theoretic devices, the portion which, as its creator Alfred Tarski demonstrated, corresponds with the theory of real-closed ordered fields (Cartesian spaces over real-closed fields).

The five papers making up the proposed symposium explore various logical and philosophical issues concerning the standard Cantor-Dedekind conception, its constructivist, predicativist and infinitesimalist competitors, and some of the wider conceptions that have been proposed.

In his paper *On the Plurality of Continua,* Geoffrey Hellman discusses the classical, constructivist, predicativist, and smooth infinitesimal analyst conceptions of the continuum and addresses the following questions: How are these conceptions differ from one another, and in what respects are they genuinely incompatible with one another, if indeed they are? Should we understand them as different ways of understanding (various aspects of) the same structure or structures, or instead as theories of mathematically distinct structures or types thereof? What is the applicability of the aforementioned different conceptions to the material world and in the empirical sciences generally?

Stewart Shapiro, in his *Continuity: metaphysics and logic,* addresses related questions. Shapiro observes that from Aristotle onward, there have been a variety of intuitions about continuous substances. That is, the intuitive, or pre-theoretic conception of "continuous" is polysemous, or a cluster concept. Moreover, the various aspects of the polysemous notion, or the items in the cluster, are not compatible with each other. Indeed, according to Shapiro, whereas some of these intuitions survive in the so-called "classical" theory of the continuum, as formulated by Dedekind and Cantor, others do not. So the notion of continuity is more up for sharpening than it is for conceptual analysis. Various mathematical theories of the continuum—in particular the intuitionistic theories that view the continuum as viscous and those, such as the classical conception that sanction the intermediate value theorem—are mutually incompatible sharpenings of the basic, intuitive notion.

In his *On the Cohesiveness of the Continuum,* John Bell directs his attention to one of the classical intuitions about the continua, their nonseparability. According to Bell,it is characteristic of the continuum that it be "all of one piece" IN THE SENSE OF BEING INSEPARABLE INTO TWO disjoint nonempty parts. By taking "part" to mean open (or closed) subset of the space, one obtains the usual topological concept of connectedness. For example, the space R of real numbers and any of its intervals are connected. Now a truly radical condition results if we take the idea of being "all of one piece" literally, that is, if it is taken to mean inseparability into any two nonempty parts whatsoever. A space satisfying this condition is called cohesive or indecomposable. While the law of excluded middle for classical logic reduces indecomposable spaces to the trivial empty space and one-point spaces, the use of intuitionistic logic makes it possible not merely for nontrivial indecomposable spaces to exist, but for every connected space to be cohesive. In his talk Bell sketches the philosophical background to cohesiveness as well as some of the ways in which the idea is modeled in contemporary mathematics.

Maximo Dickmann, in his paper on *The First-Order Logic of the Continuum,* puts forward—and illustrates on the basis of developments over the last

30 years—the idea that the first-order theory of the ordered field of real numbers reflects in a suitable manner the notion of "aggregateness" characteristic of our intuitive conception of the *continuum.* In the course of so doing, Dickmann argues that experience in the last decades has abundantly shown that the use of first-order languages and its model theory—far from putting severe restrictions, as long believed, to the application of logic to the corpus of mathematics—is sufficient to express the vast majority of the properties occurring in the investigation of real Euclidean (and projective) space, and its applications in science. At the same time it avoids the stumbling blocks arising from the use of second- and higher-order logic (incompleteness, undecidability, etc.).

Contributions

- **Shifts in the conception of the Continuum**

  José Ferreirós

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- **The first-order logic of the continuum**

  Maximo Dickmann

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- **The unification of all numbers great and small**

  Philip Ehrlich

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- **Knowledge comes from the dialectic between two worlds: The case of Fermat reals**

  Paolo Giordano

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- **Constructive and non-constructive aspects of non-standard analysis**

  Erik Palmgren

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- **On the plurality of continua**

  Geoffrey Hellman

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Symposium

Philosophy of mathematical practice [1]

Organizer: José Ferreirós, Paolo Mancosu

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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". As a reflection of this state of affairs, in 2009 a group of nine researchers in this field gathered to promote the creation of the *Association for the Philosophy of Mathematical Practice, APMP* [for more information, see: http://institucional.us.es/apmp/].

Approaches to the philosophy of mathematics that focus on mathematical practice have been thriving. They include the study of a wide variety of issues concerned with the way mathematics is done, evaluated, and applied, and in addition, or in connection therewith, with historical episodes or traditions, applications, educational problems, cognitive questions, etc. We use the label "philosophy of mathematical practice" as a general term for this gamut of approaches, clearly open to 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, cognitive science, 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 proposal is being submitted to this Congress on behalf of APMP.

The symposium will be aimed at offering philosophers of science a general overview of the area covered by APMP, emphasizing some of its most active and interesting subfields. In order to do so, we have opted for the format of a general introductory lecture, followed by a roundtable presentation and discussion. The participants in this symposium have intentionally been chosen among established and upcoming scholars in the field - thus emphasizing its expansive nature.

1. 150' symposium, consisting of one lecture and a roundtable.

Contribution

- **Fundamental mathematical objects in the late nineteenth century: A new look**

Tom Archibald
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Roundtable

- **Human agency and mathematical reasoning**

Kenneth Manders
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- **Propositional reasoning and sociological aspects**

Brendan Larvor
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- **Nonpropositional reasoning and cognitive aspects**

Valeria Giardino
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- **Symbolic reasoning and historical aspects of math practice**

Dirk Schlimm
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* *

Symposium

Are aesthetic approaches in philosophy of mathematics topical?

Organizer: Caroline Jullien

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Why is the beauty of mathematics, so often evoked by mathematicians and philosophers, not the object of systematic philosophical approach or even the subject of serious historical studies? Some answers can be found in historical considerations : since the end of the eighteenth century, aesthetic is usually treated as a part of the philosophy of art, or at least to have art as focus area, while mathematics are not institutionally considered as an art. Aesthetic in mathematics seems to be anecdotic so for philosophers or aesthetics experts, when not considered as inappropriate. On the other hand, the Kantian legacy carried with it a break between logical and aesthetic which impacts on the relationship between mathematics and aesthetics to dissolve its bases. Another historic milestone in the genesis of the eviction of aesthetic as a dimension of mathematics lies in one of the consequences of the quarrel between the Ancients and Moderns: the idea of a pure science, free from any considerations that do not fall strictly within its domain.

Despite this—institutional—distance between mathematics and aesthetics, the fact remains that the aesthetic dimension of mathematics is of significant importance for a large community of mathematicians regardless of socio-historical contexts. And this aspect of mathematics is mostly referred to not as an evaluative quality but more as inducing a method. The aesthetic in mathematics would play a dynamic role in the development of this science (Dirac), but would also be the cause of mathematical intuition (Hardy) or finally, can also be seen as a guarantee for the structural adequacy of mathematics to the real (Poincaré). Does this account of the introduction of the aesthetics in the operating modes of mathematics allow us to justify the use of a philosophical aesthetic for the epistemological and philosophical analysis of mathematics? Is the use of such a tool, usually reserved for the realm of art, topical for mathematics?

The aim of the symposium is to discuss the value of the criticisms that are obstacles to the use of an aesthetic approach in philosophy of mathematics to account for certain practices, certain phases of mathematical reasoning. The epistemology of mathematics of Henri Poincaré assumed that the ultimate aspiration of the mathematician is aesthetic and that his creative activity is fed by the same resources as those of the artist. These are the principles of such an aesthetic vision of mathematics that the symposium intends to discuss. These include offering a reflection that would allows us to identify in what form and perhaps why aesthetics could contribute to progress in understanding mathematics. It is by no means question of looking for a reconciliation at any price of mathematics with art, but the question is simply to think on the relevance of importing tools usually reserved for art to the philosophic analysis of mathematics. The purpose is then multifaceted: it includes a theoretical aspect, which aims at the justification for the eviction or otherwise for the appropriation of aesthetic in mathematics. This aspect involves thinking in particular

on the conceptual and operational differences between various philosophical approaches to aesthetics and their consequences in terms of adaptability of the tool to the field of mathematics. Another aspect of the objective is strictly illustrative and consists in examining, through practical examples, the benefits of importing aesthetics methodologies in mathematics. Finally, a last objective could be to provide a focus on the bilateral relations uniting mathematics and aesthetics.

Contributions

- **Is the expression "mathematical beauty" *only* a metaphor?**

  Roger Pouivet
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- **Necessity in works of art and in sciences**

  Maria Giulia Dondero
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- **The role of mathematics in the history of aesthetics**

  Caroline Jullien
  Archives H. Poincaré (CNRS), Nancy University (France)
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C2. Cognitive Science (including Linguistics and Psychology)

Invited Lectures

Grounding social theory on action and cognition

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Introduction of our paradigm: A Goal/Action-center view of Cognition: Cognition does not just means doxastic cognition (knowledge, beliefs), it means goals (needs, desires, intentions, plans, ...); Cognition is for Action; and Action (also social action, interaction, collective, institutional action) is grounded on cognition and meta-cognition, in humans. And knowledge is just for goals.

Given this perspective, I will possibly discuss three specific topics:

POWER: why a systematic analysis of Goals and Beliefs in the Actors' minds is crucial for a modelling of power states and relations; how power is grounded not just on a theory of "outcomes" but on a theory of goal-directed action; how social forms of "power over the others" are based on the minds of the agents).

BASIC SOCIAL MOVES: Cognition as crucial for understanding the nature and the functions of the Basic social moves (goal-adoption; goal-induction; goal-delegation), which are grounding (in the agents' minds) cooperation, exchange, conventions, norms, institutions. TRUST: as a complex mental representation (beliefs-goals pattern), not just a feeling or mere subjective probability of a favorable event.

Measuring argument strength: A Bayesian approach

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Using the catalogue of informal reasoning fallacies established over centuries within philosophy, Hahn and Oaksford [1] recently demonstrated how Bayesian probability can provide a normative standard by which to evaluate quantitatively the strength of a wide range of everyday arguments. This Bayesian approach may thus supply the long missing account of argument quality that persuasion research, reasoning research and argumentation theorists have required, thus allowing these different traditions to be brought together. Theoretical examples and experimental studies within this framework are discussed and evaluated as a means of indicating the specific requirements of psychological research and explanation.

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On anaphoraresolution: Some methodological remarks

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The relation between lexical ambiguity and disambiguation is mostly approached from an intra-sentential perspective. Thus, when analyzing the ambiguity and disambiguation of a lexical item, one tends to study its variance in interpretation when it is modified by or occurs as an argument of other lexical items. Broadening this perspective, this paper shows that there are important insights into the nature of disambiguation to be gained by studying more closely how ambiguous expressions behave in contexts spanning more than one sentence. More specifically, new data are introduced involving anaphora resolution with the following two characteristics: (i) a potentially ambiguous antecedent which is disambiguated in its local context, and (ii) anaphora which refer to one of the possible readings of the antecedent which was not selected in the local antecedent context. The paper argues that these phenomena call for a revision of how we conceive of and formalize the process of disambiguation, introducing the notion of reambiguation, which consists in reintroducing alternative interpretations which were originally excluded by disambiguation.

If time permits the analysis will be extended to examples of anaphoric links involving the notion of causality.

What is dynamic in meaning?

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While meanings used to be analyzed in terms of truth conditions, proponents of 'dynamic semantics' argued in the early 1980's that meanings are better viewed as 'context change potentials', i.e. as instructions to modify belief states. This 'dynamic turn' was precipitated by two linguistic phenomena: presuppositions and pronouns. We will discuss recent developments that cast new light on this foundational issue, and which suggest that the debate is more open than ever.

Contributed Papers

Quantification in ordinary language: From a critique of set-theoretic approaches to a proof-theoretic proposal

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We firstly show that the standard interpretation of natural quantification in mathematical logic does not provide a satisfying account of its original richness. In particular, it ignores the difference between generic and distributive readings. We claim that it is due to the use of a set theoretical framework. We therefore propose a proof theoretical treatment in terms of proofs and refutations. Thereafter we apply these ideas to quantifiers that are not first order definable like *"the majority of"*.

The autonomy of psychology in the age of neuroscience

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Suppose that scientists discover a high level property that is *prima facie* multiply realized by two distinct sets of lower level properties. They could conclude that the higher level property is multiply realized. Alternatively, they could reject the multiply realized property and postulate instead two subtypes of the higher level property and say that each is uniquely realized. How do scientists respond to this situation? A case study of normal human color vision shows that this property is not rejected and is not subtyped.

Towards a rational theory of communication

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The aim of this talk is to explain the difficulty, and benefit, of pursuing a far-fetched goal: a general theory of communication. Such a theory, it will be shown, must resolve the controversy between reductionists and emergentists, which, as far as we can tell, is irresolvable. The benefit is that by studying meaningful similarities between various emergence problems we can also observe the meaningful interaction between methodologically separate, and yet interconnected fields. For, even the study of computer networks and the study of scientific social networks—the two extremes—interact with each other much more than physics and sociology.

Are radical externalism and radical internalism the same?

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Representation is a relation between fixed structure and content. It is needed to explain how mental states can correspond to the properties of the world. We do not know, however, how structure could be distinct from content while being part of the world. I compare two approaches: the internalism of early Fodor and the externalism of Manzotti's radical enactivism. Both provide an identity between structure and content but do not solve the problem of representation, as they simply reject of one of the relata.

What linguistic nativism tells us about innateness

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Nativism is still a highly controversial topic in cognitive science. Not only because nativist claims remain controversial, but also because, at a conceptual level, the way the notion of innateness has to be defined is not clear. In cognitive science, Chomsky's research program played a foundational role. Chomsky showed the methodological contribution of a nativist perspective for the study of mental faculties and particularly language. The aim of this article is to investigate what meaning can be given to the innateness of the language faculty in Chomsky's theory and to show how it contributes to clarify the notion of innateness as used more generally in cognitive science.

The role of abduction in learning and cognition

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We suggested that logic should be redefined as the working or connecting states in human brain when he/she is thinking. Is abduction logic? Absolutely it is. Abduction is inference from effect to cause. Logic is needless to be inevitable. Our thinking does not always need or depend on the necessity. However, determine with probability is sufficient, may be better, at most tasks in our cognition. Logic has two ways to react with learning. One is so called "learning logic". Another is "logic learning". We investigated the effects of abduction in learning and cognition in this paper.

A naïve realist view of colour

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Things seem to *be* coloured. More precisely, we perceive colours as real and intrinsic (non relational) properties of the world. My aim is to clarify the ontological status of colour and to defend a common sense conception of colour property. Antirealist and Physicalist conceptions of colour lead to an *ascetic view* about colour. These conceptions maintain that *colour, as we conceive it,* is not a real property of things: either these properties are conceived as purely subjective properties of sensory experiences or as properties whose nature is only assessable to a scientific description of physical properties of things. I challenge these view and propose a more *liberal ontology* for colour property witch I call "naïve realism" about colour.

Reassessing the rationality war

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There have been debates between two apparently conflicting views about the rationality of human reasoning. One, initiated by Kahneman and Tversky, is that human reasoning under uncertainty is irrational in that people frequently violate the rules of reasoning derived from probability theory. The other is the view, championed by Gigerenzer, that human reasoning is rational in that heuristics actually operative in human reasoning are successful in solving problems of a natural and social world. Although Samuels, Stich and Bishop (2004) claimed that both sides do not really have any deep disagreement over the extent of human rationality, I suggest that there are still interesting disagreements between the two sides, and further argue that through sound adjudication of both views, we can reach a more comprehensive and balanced view of human rationality.

Reflectance physicalism and contrast colours

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Byrne & Hilbert [1] argue that colours are reflectance functions of objects surfaces. By means of a new thought experiment, inspired by Gärdenfors and by Jackson's 'Mary's room'-argument, I question the phenomenal adequacy of reflectance physicalism in a particular case of perceived (reflectance functions of) object surfaces.

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The suppositional Ramsey test in decision making

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The ability to rationally accept conditional information within one's stock of beliefs is essential in order to evaluate different plans of action. One powerful principle that regulates such processes is the Ramsey test (R). However, it has been shown that (R) is problematic. I argue for a new understanding of (R) which highlights the suppositional nature of the acceptance of conditionals. This allows a rational agent to anticipate the consequences of different courses of action without having to actually take them in order to find out. Furthermore, I show that my account has far-reaching consequences for decision-making in that it can handle the problem of decision-instability.

Algebras of the mind and algebras of the brain

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Neural Algebras are rich models of Combinatory Logic. They consist of formal objects which represent sets of cascades of firing neurons; the binary operation of application reflects their causal relation. As a combinatory model a neural algebra relates to an interpretation of combinators as thought-objects; by its construction it relates to their neural correlates. This allows the presentation of mental concepts by equations in the algebra. Consciousness, as an example, presents as a recursion equation, reflecting its self referential character, and whose lattice of solutions describes different phases and moving context of consciousness. The theory is related to evidence from the neurosciences.

Blackboard system as model of problem solving in Sudoku puzzles

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Sudoku puzzles is very popular worldwide. This paper will present a blackboard system model for solving Sudoku in order to offer a better explainations for why Sudoku is not a pure deductive puzzle and its difficulty than Louis Lee, P. Goodwin and N. Johnson-Larid's recent work.

Emergent, mental causation and downward causation

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In this paper, I will give a "positive" characterization of emergence. And then give a coherent explanation of how "downward" causation is compatible with emergence causation, and I will try to prove that downward causation is compatible with the emergence.

In contemporary discussions of the mind and body problem, worries about mental causation are prominent and obvious. Originally, the problem of mental causation was that of understanding the relationship of the mind and the body. Based on the British Emergentism, I will give some analysis on the existence of mind/soul and the mental causation.

What falsifies an NCC of specific content?

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The falsification conditions for proposals of a neural correlate of a specific conscious content are necessarily tied to first person reports, even if the direct introspection of brain states holds [1]. I distinguish between different kinds of NCCs and introspective judgements to show which instances falsify which NCC-claims. Then, I critically discuss the reliability of introspective reports in such proposals as Kriegel [2], and Schwitzgebel [3] [4]. I hold that given bayesian and modal reasoning, introspective accounts may be useful in NCC research despite their basic psychological and methodological problems.

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Is rational-emotive behavior theory based on the methodology of critical rationalism?

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Rational-emotive behavior theory (REBT) is a psychological therapeutic theory advanced by Albert Ellis in 1945. It was firstly set on the basis of logical empiricism. And then critical rationalism became its basis. This article analyzes the relationship between REBT and critical rationalism and gets conclusions that REBT is a scientific theory in the sense of critical rationalism because of its falsifiability and although REBT is essentially on the base of critical rationalism, to take falsifiability as criterion of distinguishing rational and irrational beliefs in REBT is not appropriate.

The principle based explanations are not extinct in cognitive science: the case of the basic level effects

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In a series of papers of the last 10 years, [3], [1], [2], certain philosophers of cognitive science have argued for the view that law-like statements do not play any significant role in the explanatory practice of cognitive scientists. This paper provides evidence that the appeals to explanatory principles are not rare in cognitive science and that the use of principle based explanations seem indispensable for some explanatory tasks. The latter claim is supported by a case-study of the efforts to find a proper explanatory framework for the so-called basic level effects in categorization.

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Towards linguistically-grounded spatial logics

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We propose a method to analyze the amount of coverage and adequacy of spatial calculi by relating a calculus to a linguistic ontology for space by using similarities and linguistic corpus data. This allows evaluating whether and where a spatial calculus can be used for natural language interpretation. It can also lead to 'more appropriate' spatial logics for spatial language.

Steering ontological blending

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We introduce ontological blending as a new method for combining ontologies. The approach is inspired by conceptual blending in cognitive science, and draws on methods from ontological engineering, algebraic specification, and computational creativity in general.

Representation and action: A theory of representation in the evolution-embodied cognition context

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Michael L. Anderson and Gregg Rosenberg propose the guidance theory of representation (GTR). This paper points out that based on the theory of evolution-embodied cognition, GTR takes a neo-empirical approach to illustrate the content of representation by replacing the intentionality of representation with the intentionality of action. By introducing the ideas of "standard used tokens", "assumption of information" and illustrating the representation of errors by "failed actions in their intent", GTR implicitly involves some untenable assumptions. GTR neither overcomes the fundamental flaws of some popular theories, nor provides a naturalistic explanation of content of representation without intentionality.

Two approaches to the notion of Point of View

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The intuitive notion of point of view can be approached in two main ways. One of them is based on the model of propositional attitudes; the other one is based on the notions of location and access. Whereas the question of whether points of view can exist without a bearer does not make sense in the context of the first approach, it makes perfectly good sense in the context of the second one. We analyze that difference arguing that the crucial feature has not to do with the irreducible ontological subjectivity of points of view, but with a distinction between "tokens" and "types".

Cooperative answering and inferential erotetic logic

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The aim of this talk is to address the issue of applicability of Wisniewski's Inferential Erotetic Logic (IEL) in the area of cooperative answering for databases and information systems. Some basic cooperative answering phenomena will be modelled within the framework of erotetic search scenarios (IEL tool). Also the possibility of enriching cooperative answering techniques with question posing capabilities will be considered.

The causal closure of the physical and the variable realization

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During the last few decades discussions concerning the problem of mind-body causation have been mostly related to the acceptance of the thesis that the physical realm is causally closed. This thesis conjoined with the rejection of any systematically overdetermined causation relation, allow us to conclude that the mental should, at most, supervene on the physical. Mental states can supervene on different physical compounds, that is, have a variable realization. However, is difficult to accommodate this feature with the expected determination between the physical substrates, presumably causally related. We analyze this problem, particularly related to the position of Papineau and Kim.

Computational mechanisms and models of computation

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In most accounts of computational processes as realized by physical mechanisms, one presupposes that there is one-to-one correspondence of the causally active states of the physical process and the states of the computation. Yet, such proposals either stipulate that there be only one model of computation to be implemented or do not reflect upon the possible variation of models of computation being implemented physically. In my talk, I will defend the claim that the mechanistic account of computation should also account for a possible broad variation of models of computation. In particular, certain non-standard models should not be excluded *a priori*.

Damasio, self and consciousness

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I will argue that Antonio Damasio's theory of consciousness, and particularly that his notion of core consciousness does not square with dreams, locked-in-syndrome, and our normal psychological experience. His connection between consciousness and the self detract from his insights about the self. Where Damasio should find conscious processes, we find unconscious ones instead. Indeed the self, as instantiated in the brain, should do most of its work unconsciously in order to succeed, as evolutionary neuroscience would lead us to expect.

Dual nature of consciousness

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I offer a concept of dual nature consciousness which allows to explain such enigmatic phenomena of human life as a cognition of the world, a dialogue with the Other and the deliberate deceit. I am going to prove the existence of two types of culture of thinking which are in the basis of different kinds of human activity (mathematics, physics, moral, jurisprudence and so on).

Should I split or should I lump? The epistemic-tool approach to scientific concept formation

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I investigate what I call the *mechanistic theory of natural kinds* and the *splitting-lumping model* of conceptual change suggested by the theory. Carl Craver has recently argued that the mechanistic theory collapses into conventionalism and does not solve the problem of finding natural kinds. I show that this conclusion can be avoided by adopting a *perspectival realist* position that makes the decision of whether to split or lump relative to the scientific discipline in question. Furthermore, by examining Edouard Machery's work on the notion of concept in psychology, I illustrate how my *epistemic-tool approach* to natural kind concepts can incorporate the splitting-lumping model into a general framework of interdisciplinary knowledge production.

'De dicto' versus 'de facto' attitudes

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De dicto knowledge, to be contrasted with *de re* knowledge, is standardly assumed to be knowledge of a *dictum.* It is often understood as knowledge of a fact or of a proposition. Here it is conceived of as involving linguistic understanding of the components of the *dictum.* Hence *de dicto* and *de re* knowledge should have more complex relationships than usually presumed. The paper proposes a formalization of attitude ascriptions in first-order epistemic logic together with a semantics based on Hintikka's worldlines, so that such distinctions can be regimented and classical puzzles about knowledge and identity be solved.

Can innateness assumptions avoid the tautology problem?

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With the development of cognitive sciences and psychology of development, the term "innateness" has raised a renewed interest. Philosophers (for example [3], [1], [2], [5], [4]) have however stressed out the difficulty to give this notion a precise meaning. In this paper I show first that recent critical assumptions can be reduced to an epistemological objection first raised by Locke in *His Essay on Human Understanding* (I, II), that I call the *tautology problem.* Then I argue that this problem can be weakened when innateness is conceived in a non-dichotomous way (not opposed to other kinds of development) as a *conditional primitive term* of the developmental explanation.

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The Chinese room argument and the symbol grounding problem: A new perspective

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Throughout the last thirty years we have witnessed a complex philosophical discussion around the conclusions drawn by John Searle from his celebrated Chinese Room thought experiment. Searle was trying to demonstrate that minds are not information processing machines and that thought can not be reduced to a mere formal manipulation of symbols. Some authors flatly dissmissed Searle's arguments with different thought experiments of their own. On the other hand, there are those who have tried to tackle the problems posed by Searle's experiment. In this second group we find those who argue that solving the Simbol Grounding Problem (SGP) as proposed by Steven Harnard is sufficient condition for agents to have artificial intelligence. However, the conclusion of the work presented here is that solving the SGP does not imply solving the problems posed by the Chinese Room argument. Our conclusion is drawn from studying some of the strategies proposed in the last years to try to solve the SGP and which show a residual component of computationalism on them.

The concept of "object" in the visual binding theories

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In my presentation I investigate the conceptual frameworks of the visual binding theories: Feature Integration Theory (FIT) based on behavioral data and the neural correlation theories. These theories attempts to explain how separate visual features are bound to form a representation of an individual object. Using the concepts of the contemporary analytic metaphysics I consider what ways of

analyzing the concept of "object" are consistent with the different frameworks of FIT and the neural correlation theories. I state the thesis that considered theories use contradictory concepts of "object", which can by analyzed by means of the competitive philosophical conceptions.

On levels of mechanisms

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The mechanistic paradigm in philosophy of neuroscience is often claimed to be ontologically neutral, accepting for instance downward causation. However, this paper argue that its proponents have to acknowledge that cognitive phenomena supervene on their underlying mechanisms, and, therefore, that the classical causal argument for token-identity thesis applies within the mechanistic framework. Thus, the mechanistic framework cannot be as liberal as often claimed with respect to metaphysical issues.

Abduction and rumormongering to the most coherent interpretation

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In this talk we shall outline logical structure of explanatory rumormongering, conceived as a peculiar implementation of abductive mechanism of making sense of surprising phenomena. By analogy to Inference to the Best Explanation schema we describe the structure of Rumormongering to the Most Coherent Interpretation, based on Thagard's Theory of Explanatory Coherence.

Dynamic logics of speech acts as formal simulations of social interaction

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Inspired by the development of various systems of dynamic epistemic logics in the last two decades, dynamic logics of several speech acts are developed recently. The purpose of this paper is to examine what we can learn from the fact that these "logics" are available. In order to answer this question, we propose to consider these systems as "formal simulations". This enables us to have an interesting dimension of evaluation; we can now ask how good they are as simulations. Moreover, it also allows us to ask whether and how we can improve their performance.

In defense of a multiple content structure of self-representationalism

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Some suggest that the endorsement of a dual content structure of a mental state that goes with Self-representationalism entails a dilemma. For it brings up a challenge of explaining how such a structure can be compatible with a singular state. Without such an endorsement, however, Self-representationalism seems not to be well-equipped to explain the evidence which trigger it to embrace an inner awareness view of consciousness. I resolve this dilemma by arguing that a multiple content structure is compatible with a singular state, and that multiple contents is neutral with respect to what kind of idea one might hold about one's cognitive mechanism.

C3. Biology

Invited Lectures

Pheneticism reconsidered

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The pheneticist view of species has been dismissed by most philosophers of biology in recent years, and by many biologists too. This talk aims to re-assess the strengths of pheneticism, and makes a case for pheneticism as part of a reasonable pluralism about species. First, I argue that pheneticist methods of classification are far from dead, especially in microbiology and botany where the lack of a tree-like structure in evolutionary genealogy is especially clear. Second, I argue that pheneticism—according to which species are clusters of resembling organisms—is a natural bedfellow of the homestatic property cluster (HPC) view of species. It is bizarre, then, that the HPC view should be so popular among philosophers, while pheneticism is so ill-viewed. Third, I counter a series of arguments put forward against pheneticism. Some misrepresent actual pheneticists, or they rely on particularly extreme representatives of the pheneticist school. Others exaggerate the degree to which pheneticist and non-pheneticist methods differ in their abilities to arrive at a 'natural' classification.

The rise of post-genomics and epigenetics: Continuities and discontinuities in the history of biological thought

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Biological knowledge has deeply evolved during the XXth century. Modern Synthesis, elaborated in the 1930s, has been regularly challenged in the second half of the century. The molecular revolution of the 1960s has been followed by the development of systems biology and epigenetics. These recent transformations have been described by many observers as "revolutionary". In fact, they have deep roots in the history of biological thought. For instance, the major importance attributed today to epigenetics cannot be understood without referring to the complex history of life sciences during the last centuries.

I will discuss the possibility to articulate continuities and discontinuities in the evolution of biological models.

Contributed Papers

Ecology, evolution, ethics: In search of a meta-paradigm

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Evolutionary biology, ecology and environmental ethics: at first glance, three different objects of research, worldviews and scientific communities. In reality, there are both structural and historical links between these disciplines. Some topics are obviously common across the board, and the emerging need for environmental policy management has gradually but radically changed the relationship between these disciplines.
On the basis of the history of these disciplines, it is possible to build a meta-paradigm, i.e. a connecting epistemological framework resulting from one common or convergent tendency of thought and practice shared by different disciplines. The transactional worldview (Dewey and Bentley 1949) is considered as one of these useful fictions that can help us to deal with some convergent aspects underlying the research in ecology, evolutionary studies and moral philosophy. What is clearly emerging from these studies is that the environment is acquiring new senses and values: the dichotomy between organisms and the environment is tending to disappear, and some elements emerging from the analysis of ecological, evolutionary and moral studies are converging with respect to the processes of co-determination between organisms and the environment.

Is the concept of life response-dependent?

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Six decades after Schrödinger, we still have no general account of the nature of life. For most biologists, this is not a pressing problem. Yet for the growing number of researchers exploring the possibilities of artificial life, alien life and the origins of life, it could hardly be more urgent. I explore an overlooked, deflationary possibility: might the concept of life be response-dependent? It is uncontroversial that many basic concepts implicate human responses. We call something "red" because it looks red; we call something "sour" because it tastes sour. Do we call something "living" because it looks alive?

Adaptationism: behind criticisms and typologies, the tool

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In this paper, I argue for an original and yet simple understanding of adaptationism. Against the received view (delineating different and heterogeneous types of adaptationism), I suggest adaptationism is in fact a kind of material inference (Brigandt 2010). All such adaptationist inferences share two important features: 1° their premises harbour the same logical form, independently of the empirical content they organise, and 2° to the extent it is vindicated, their conclusion identifies a *potential* adaptation. This view provides a tool to flesh out adaptationism's heuristic character, and to assess claims on potential and actual adaptations.

A moderate solution to the debate over the species concept

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No matter in biology or philosophy of biology, the species concept is so consistently controversial. There is a sway between monism and pluralism on 'species'. Due to so many different species concepts are been used by biologists, pluralism seems a proper way to interpret 'species'. On the other hand, as a scientist, we desire a unified concept of species. Here we try to argue that pluralism of species is the result of ranking needs, and base on Mayr's Biological Species Concept, we may have a proper definition for 'species' on clustering level.

Biological individuality in plants and beyond— A reconciliation for the genet-ramet dispute

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The problem of plant individuality is something which has vexed botanists through the ages, with fashion swinging back and forth from treating plants as communities of individuals [4], [2], [7], to treating them as organisms in their own right, and although the latter view has dominated mainstream thought most recently [6]; [3]; [1], a recent lively debate proves that the issues are far from being resolved [9]; [5]; [8]. I settle the matter once and for all, by showing which elements of each side are correct.

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The Darwinian muddle on the 'Division of physiological labor': An attempt of clarification

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There is of a philosophical and historical interest to specifically examine how Darwin conceived the mechanism of division of labor within natural history. Indeed, this mechanism is intrinsically linked to the process of which the concept is, according to the naturalist, one of the two 'keystones' of his theory: that being the principle of divergence.

We first show the conceptual confusion in which Darwin plunges, when using a so-called economic argument to defend his thesis of the maximization of beings in a given territory due to division of labor. Following this we propose several hypotheses to explain these shifts, recurring in Darwin's texts, from one conception and from one extension to another, of the division of labor.

An argument against the evolutionary contingency thesis

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This paper offers an argument against The Evolutionary Contingency Thesis starting from a basic concept of darwinian evolution and its principles of variation and selection. I will present two constraints on darwinian evolution: small evolutionary changes and facilitated variation. Given the two constraints, the principles of variation and selection are to be seen as core hypotheses in a general theory of evolution with a rich structure, so they are not "highly" contingent as ECT implies. Also, this perspective will show that darwinian evolution is not fully equivalent to its *a priori* mathematical models as the objectors to ECT often advocate.

Economic natural selection: What concept of selection?

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The paper examines two cases of adoption of evolutionary ways of thinking by modern economists : Nelson's and Winter's "Evolutionary theory of economic change" [1], and evolutionary game theory (1990s and after). In both cases, the authors explicitely refer to natural selection in an economic context. I show that natural selection is taken in two different senses, which correspond to two kinds of generalization of the principle of natural selection. One contains reproduction and heredity as key elements, the other does not.

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Modeling/experimenting? The combinatorial strategy in synthetic biology

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In recent philosophical discussion several authors have likened modeling to experimentation on various grounds. On the other hand, some authors have pointed out features that according to them distinguish experimentation from

modeling. We will study these different views through examining modeling practices in synthetic biology where experiments on model organisms, mathematical models (and their simulations), and synthetic models are used in a combinatorial fashion. We will argue that from the perspective of scientific practice the question of whether or not modeling is like or unlike experimentation is often beside the point, since both are needed and they accomplish partly different kinds of things.

On informational schemes in biology

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Information can be a cornerstone for interpreting all world phenomena, as confirmed by a series of philosophical studies (Schroedinger, Maynard Smith, Floridi), in which it seems to be a constitutive element for objects, environment and interactions.
It seems possible to propose an epistemological framing for interpreting the nature as a system of informative processes, both in general understanding and in more particular areas such as computer science, physics and biology.

Function as a causal role in a biological model

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Philosophers of biology usually distinguish historical and systemic accounts of functions. In many areas of experimental biology the "systemic" account is often the most relevant. Yet there are problems this account does admittedly not face up to very well. My contention is that, though two minor problems are irredeemably unsolvable for the systemic account of function, the major ones can be solved by assuming that 'function' denotes (directly) a causal role *in a model* and (indirectly) the corresponding process *in nature*. I try to show this through the analysis of the hypothalamus-pituitary-adrenal axis of stress.

Computational biophysics as a case against intertheoretic reduction

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I discuss the molecular dynamics simulations (MDSs) of protein folding. I argue that: (1) Modern atomic physics is almost absent in biophysical MDSs, and a maximally simplified treatment of atoms (or no treatment at all, in the so-called pseudoatom coarse-graining techniques) is common and seems sufficient.

(2) There is no reason to suspect that a better understanding of atoms will translate into a better understanding of biophysical systems. (3) Relevant cell-biological facts and laws determine the facts and laws in biophysics—and the role of the facts and laws of atomic theory is secondary.

Are embryos what we thought they were, and how do we know?

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Traditional understandings of embryos have built on Aristotle's epigenetic understanding, where development occurs gradually and form emerges over time through an embryological process. Alternative preformationist interpretations have viewed the embryo as somehow "already there" from the "beginning." Lively debates took place through the 18th and 19th centuries, with biologists settling on materialistic epigenetic interpretations by 1900. Part of this standard view was that embryos start without form, and the form emerges gradually through an essentially linear process of development. Eventually we get to fully formed adults. Stem cell, cloning, and other transplantation studies call our long-standing interpretations into question. This paper will examine the changing conceptions of embryos.

The evolution of punishment

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In this talk, we put forward a plausible picture of the evolution of punishment. Most importantly, we distinguish two kinds of accounts for the evolutionary function of punishment: punishment as a behavior-modification strategy (BMS) and as a cost-inflicting strategy (CIS). Although some researchers assume that punishment evolved as a BMS, we argue that the CIS account is more plausible by examining evidence from biology, anthropology, social psychology, behavioral economics, and developmental psychology.

Modelling development and evolution in three dimensions

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Despite the role of models in scientific practice is increasingly being recognized, modelling in developmental biology and evo-devo (particularly three-dimensional modelling strategies) has just recently started to be explored. Taking as a case study the so-called "lateral somitic frontier hypothesis", my presentation will explore how new techniques in 3D imaging and modelling of embryos

are contributing to an organicist and mechanical explanation of the development and evolution of animal form.

A materialist account of scientific reasoning in Ethology

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This paper will argue that the new experimentalist approaches in theory of science are capable of reconstructing the actual functioning of one special science such as Ethology in ways that other views fail to replicate. To this end, the author will describe the epistemological core contentions sustained by the Spanish philosopher Gustavo Bueno in his Teoría del Cierre Categorial and will argue that it favorably compares to other experimentalist philosophies like that of Ian Hackin. Secondly, I will analyze some of the most relevant parts of the history of Ethology from the materialist standpoint maintained by Bueno showing how it helps illuminate the internal reasons why the behavioral sciences cannot be transformed into "hard disciplines" without abandoning the sort of operational phenomena they aim to deal with. Thirdly, I will show that some research in Ethology, its soft epistemological status notwithstanding, provides key evidence as to how natural selection and behavior interact with each other in the field.

The status of the hardy-weinberg law

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The aim of this communication is to analyze the status of the Hardy-Weinberg law within (classical) population genetics. The analysis will be carried out with the notions of the structuralist view of theories, especially those of fundamental law or guide-principle, specialization, and special law, having as a background a rational reconstruction of (classical) population genetics—sketched in this communication—made within the framework of such a metatheory.

The fitness landscape metaphor: Dead but not gone

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The participants in the recent debate about the status of the fitness landscape metaphor in evolutionary biology seem to share the view that a scientific metaphor is to be treated as a testable model. By that they mean that if a metaphor suggests misleading analogies it should be considered "wrong". This

line of thought has led many to the conclusion that insofar Wright's landscape metaphor is vague in some and clearly misleading in other aspects, it should be replaced by more rigid mathematical models such as those which have been recently reviewed by Gavrilets [1]. To interpret these models, however, many use the same language, which have been once introduced by the landscape metaphor. A proper account of this situation is possible only if one makes a clear distinction between a metaphor and a model that builds on it.

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Individuation for holists: (physical) dispositions and (biological) affordances

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In this paper I argue that the traditional misgivings about individuation for holistic positions can be overcome by embracing an ontology of dispositions. Dispositions have the same kind of relational character that holism postulates for thought, language and, more generally, agency. They have also being considered unsuitable for individuation. However, if we understand the nature of a disposition not only in terms of the class towards which it tends but also in terms of the actual member of that class with which it (contingently) enters in contact, the problem of individuation can be answered.

Addressing a theory-practice gap: What can kind essentialism contribute to understanding classificatory practices in biology?

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There is a consensus among philosophers of biology that traditional forms of essentialism about biological kinds have no place in accounts of biological kinds and classification. Recently, however, several authors have attempted to resurrect essentialism about biological kinds. I analyze these attempts and argue that the explanatory role of kinds in science to some extent necessitates an essentialist understanding of kinds, albeit not one that conceives of kind essences in any strong, metaphysical manner. I argue for a non-metaphysical version of essentialism that can help us understand classificatory practices in biology without committing to a particular metaphysics of kinds.

Cellular dynamics at the beginning of prebiotic world

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It seems that the emergence of cellular dynamic structures were more feasible than previously though. From the results of recent studies, it is likely that in the origins of prebiotic world the appearance of protocells came first. In other words, contending visualizing the dawn of prelife from the perspective of spontaneous emergence of a self-organizing minimal cellularity could shed more lights to this elusive problem than the proposals on 'metabolism-first' and/or 'replication-first' scenarios.

Conservative reduction of biology

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The paper argues in favour of a reductionist strategy in the philosophy of biology. I design a conservative, non-eliminativist reductionist strategy based on the possibility of constructing functionally defined sub-concepts in biology that are coextensive with physical descriptions. This theoretical link between biology and physics makes it possible to understand the original biological concepts as abstractions from these biological sub-concepts and thereby preserves the fundamental role of physics while allowing biology its proper sphere of explanatory autonomy.

Essentialism, Darwinism and "theory theory"

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In my presentation I will introduce the clash between psychological essentialism and evolutionary theory; consider the possible parallel between 'overcoming the essentialist reasoning bias' in the course of individual development and history of biology as the result of (getting to know) evolutionary theory; and analyse whether and which versions of *theory theory* we can apply to this parallel, if it exists. I claim that the comparison between individual cognitive development and scientific development has several levels, and the applicability of *theory theory* to the comparison between cognitive development and history of biology depends on which level we are talking about.

Epistemological reconstruction of the concept of level. Some preliminaries and a proposal

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This work is an attempt to clarify the concept of level in order to make it viable to use it coherently within different conceptual frames and, especially, to allow discussion and commensurability among diverse and even alternative views related, in particular, to biological systems. Instead of proposing a definition I assume an intuitive notion in the sense that levels are those diverse states of association of elements that constitute a system by virtue of a given **relation.** Which that relation will be is precisely what demands a detailed epistemological reconstruction. The features considered are *composition, integration, emergence, control* and *organization.*

On Rosenberg's Darwinian reductionism

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According to Rosenberg (2006), philosophers have been led to embrace the untenable physicalist antireductionism by such considerations as: (1) the inapplicability of Nagelian account of reduction to biological sciences; (2) Mayr's distinction between proximate and ultimate explanations; and (3) the literal truth of Dobzhansky's dictum that nothing in biology makes sense except in the light of evolution. Rosenberg argues that philosophers can stop worrying and love Darwinian Reductionism. In this paper, I argue (1) that Rosenberg begs the question against Mayr; (2) that Rosenberg cannot win his argument concerning reductionism in biology without simultaneously losing the idealized form of reductionism in chemistry; (3) that if the Principle of Natural Selection is not a nonderived law about biological systems, then, *pace* Rosenberg, it would not be a nonderived law of chemistry.

From substantival to functional vitalism and beyond in biomedical thought: animas, organisms and attitudes

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I distinguish between 'substantival' and 'functional' forms of vitalism in the eighteenth century. Substantival vitalism presupposes the existence of a (substantive) vital force which either plays a causal role in the natural world as studied scientifically, or remains an immaterial, extra-causal entity. Functional vitalism tends to operate 'post facto', from the existence of living bodies to the

search for explanatory models that will account for their uniquely 'vital' properties better than fully mechanistic models can. I discuss representative figures of the Montpellier school (Bordeu, Ménuret, Fouquet) as functional rather than substantival vitalists, and suggest an additional point regarding the reprisal of vitalism(s) in the 20th century, from Driesch to Canguilhem: that in addition to the substantival and functional varieties, we encounter a third species of vitalism, which I term 'attitudinal', as it argues for vitalism as a kind of attitude.

Why the classical mendelian genetics are necessary? — A comparison of Lindley Darden's mechanism approach with C. Kenneth Waters' genetic approach

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This paper argues that Mendelian genetics are necessary for molecular genetics by menas of a comparison of Lindley Darden's with C. Kenneth Waters' works. Darden and Waters have made valuable philosophical analyses of the two fields respectively from different approaches. This paper discusses the difference and convergence of their views. I argue that their works together prove the necessity of Mendelian genetics to molecular genetics in spite of their different argumentative strategies.

Contributed Symposia

Symposium

Evolution of biological complexity

Organizers: Matteo Mossio, Francesca Merlin

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During the last twenty years, the notion of "evolvability"— which literally means "the capacity (or ability) to evolve"—has been increasingly invoked in Biology in relation to a variety of genetic and developmental phenomena as genetic adaptive mutations [7], genetic and developmental modularity [9], genetic and phenotypic robustness [8], phenotypic plasticity, [6]; [10] and "facilitated (phenotypic) variation", [4].

In particular, all these phenomena are supposed to increase the evolvability of living organisms to the extent that they are involved in the generation of variation at different level of biological organization. The underlying idea is that the more a biological system can vary, the more are its chances to evolve, given the fact that environment is not stable and homogeneous, but changes all the time and is spatially heterogeneous. Thus, to say that a biological system is "more evolvable" than another means that, in general terms, the former can generate more genetic and/or phenotypic variations than the latter, and so has a higher capacity to evolve.

On the basis of this very broad idea, a lively debate exists today among philosophers and biologists on the precise definition of evolvability. At least three main characterizations can be found in the literature:

1. Evolvability as the ability of a population to respond to natural selection (Houle, 1992). In this case, evolvability is nothing more than heritability and, in the same way, is measured in terms of standing variation in a population.
2. Evolvability as the ability to produce heritable and selectable phenotypic variation, [9]. In other terms, evolvability corresponds to the ability of producing potentially adaptive variation.
3. Evolvability as the ability to acquire novel functions via viable and fertile genetic changes (Maynard-Smith & Szathmary 1995). Here, evolvability is the capacity to produce something more than just (adaptive) variation, i.e. innovations.

Although they share the idea that genetic mutation is the general mechanism involved in the generation of variation, existing accounts on evolvability agree on the fact that additional properties of biological systems must be considered to provide an adequate understanding of their capacity to evolve. Accordingly, as we mentioned above, attention is drawn to features as modularity, robustness and genotype-phenotype mapping, which contribute to canalize the whole amount of possible random variations into a smaller set of viable and functional outcomes.

Indeed, the appeal to this kind of features reveals that a considerable complexity is required for a system to be able to evolve. Yet, biological and philosophical literature usually does not provide adequate characterizations of the kind and degree of complexity underlying the evolvability of biological systems. In our view, the investigations on evolvability should be integrated into a more general theoretical framework of the whole organization of biological systems and its distinctive complexity.

The central aim of this symposium is to establish an explicit connection between contemporary studies on evolvability and existing accounts of biological complexity and organization, which have been mostly developed without reciprocal influence. In particular, studies on biological complexity have put emphasis on the capacity of biological systems to create "order from chaos", to self-organize spontaneously. Principles of self-organization have been first developed in Physics and Chemistry during the 70s and 80s, with the work on *dissipative structures* by Prigogine and coworkers, [5] and on *self-organized criticality* by Per Bak [1]. Starting from the 80s, principles of self-organization have been progressively introduced into the biological domain, in particular with the work of Stuart Kauffman [3]. During the last thirty years, an increasing number of models of biological self-organization have been developed in various scientific domains, so that we can speak of a coherent contemporary field of scientific research on principles of biological organization.

One of the central contributions of this research domain is the focus on the requirements in terms of complexity that a natural system has to meet in order to be able, in far from thermodynamic conditions, to generate their own organization, identity, and self-maintain (at least to some extent) themselves. As Evelyn Fox Keller [2] has emphasized, these models constitute a fundamental tool for investigating the nature of "distinctively biological" complexity and, in particular, for getting a better understanding of its evolution through time.

The aim of connecting the issue of evolvability to the more general research trend on biological complexity is the reason why we entitled the symposium "evolution of biological complexity". Indeed, we believe that rooting evolvability into a more fine-grained account of biological complexity could allow formulating theoretical and philosophical questions in a different way, and giving different answers to classical questions. In particular, the symposium will explore the following issues:

1) *Does biological evolution imply the evolution of complexity*? This first question looks for a possible relationship between the evolution of biological systems and the evolution of their complexity in the terms just mentioned above. More precisely, some of the talks will try to get a better understanding on how constitutive properties of organization, identity, and self- maintainance constrain natural evolution. In addition, the talks will deal with the issue on whether biological systems change could be characterized as a tendency to the evolution of their complexity, namely as an increase over time. What would be the reasons for justifying the idea of such an evolutionary tendency? Does the literature on the principles of self- organization provide a crucial contribution to answer this question?

2) *How should biological complexity be understood*? The way in which biological complexity is characterized and measured is a central issue in order to answer the question above. Actually, if the complexity of a system is defined with respect to the functions it can perform, and given the fact that some new

functions can appear without the emergence of any new structures in the organization (as in the case of exaptations), natural evolution does not seem to necessarily involve a change towards an increase of complexity. However, various different measures of complexity can be adopted, and some of the talks will deal with the question to know which measure is specifically pertinent in the biological domain, and for which theoretical and philosophical reasons. In particular, a crucial aspect in this respect will concern the requirements in terms of degree and kind of complexity for providing a biological system with the capacity to evolve. As we will emphasize, not only biological complexity is the result of evolution, but the very process of evolution requires in turn a prior form of complexity to act on.

3) *Is evolvability a defining property of biological systems*? At first glance, this question can be understood as exploring the validity of the idea that a system which is not evolvable cannot be biological. Why? Because the ability to change seems to be an essential property of every living entity, and even a condition for its viability and persistence over time. Then, the additional issue is to know whether evolvability is an intrinsic or extrinsic property of biological systems, i.e., to determine whether the degree of evolvability depends on the particular context the system is embedded in or, on the contrary, it is completely independent from that. In an analogous way, a biological system that cannot change its own complexity could be considered as non biological. Why? Because natural systems are by definition entities that can evolve; moreover, they are defined by their complexity, i.e. their ability to generate their own organization, to identify and to maintain themselves. Thus, the characteristic properties of biological complexity are expected to be likely to evolve too. Yet, it is not clear if a defining property of biological systems is their capacity to change their own complexity or, more specifically, their ability to increase it over time.

4) *Which mechanisms do generate variations*? As we have seen above, genetic mutation is considered, in the literature on evolvability, as the fundamental mechanism for the generation of variation. Yet, by referring to the studies on biological complexity, some of the talks will put forward the fact that other mechanisms can generate variations in biological systems, and some of them may also have been temporally and logically prior to mutations in the history of living organisms. In particular, we will focus on what Fox Keller [2] calls, after Herbert Simon, the "evolution by composition", which would generate variations through the interplay between mutually dependent semi-stable structures in thermodynamically open systems. In addition, the symposium will focus on the issue of the levels of description at which the capacity to evolve is biologically relevant: should evolvability be understood as a capacity of genes, organisms, populations, clades...? The variety of answers given to this question calls for an analysis that pays attention both to levels of complexity and evolvability.

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Contributions

- How does organized complexity evolve?

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- Heterogeneity, evolution and complexity

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- Vicious and virtuous circles in molecular and cellular biology

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- Adaptive vs. neutralist models of the evolution of complexity

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- **Beyond flat earth systems biology: Biocomplexity research as an exemplar of multilevel, multiscale modeling**

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- **Towards an articulation of the generative and genealogical aspects of living organization**

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C4. Chemistry

Invited Lectures

Many ways of knowing

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Humans express themselves—express and develop their knowledge of the world—in a wide variety of ways. Philosophers make sentences, propositions that may be true or false, justified or not. Artists make paintings, images that "work" or do not. Musicians make music. Theoretical physicists make equations. Synthetic chemists make molecules. Instrument makers make instruments, and it is with respect to instruments that I develop the idea that there are many ways to express our knowledge in my book *Thing Knowledge.* This paper explores several of the many consequences of this idea: For philosophers this idea demands radical surgery on the concept of knowledge. This surgery goes nowhere without an appreciation for the differences between working with materials and working with propositions. For historians this idea demands a reconsideration of how boundaries are drawn. And as boundaries change, so do the demands for archival practices. This idea demands a reconsideration of how recognition and reward are allocated. I close with an examination of the "rules" or "conventions" or "cultures" of "epistemic trade," or to put it another way, I close considering the implications of the move from "knowledge" to "intellectual property."

Knowing and making in an impure science

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Physics haunts chemistry in the philosophy of chemistry, if only in that it motivates philosophical attempts to establish chemistry as a discipline in its own right. One way of doing so is to consider the production and validation of chemical knowledge which involves the capacity to synthesize chemicals. This presentation takes its cue from *Chemistry—The Impure Science* by Bernadette Bensaude-Vincent and Jonathan Simon who find fault with attempts to assign to chemistry a disciplinary identity of its own. Their reasoning has implications also for the epistemology of chemistry. In particular, I will show that Davis Baird's consideration of *Thing Knowledge* conceives too narrowly the relation of knowing and making in chemistry. On the one hand, it follows Hacking too closely by moving from representations as things that are being made to made things as representations. On the other hand, it does not tend sufficiently to the specific difference of the research context and the public communication in printed form of what chemists have succeeded to make.

Contributed Papers

The ontological autonomy of the chemical world: further arguments

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According to a traditional reductionistic assumption, chemical entities, when properly analyzed, are nothing else than extremely complex physical entities. In this work we analyze three kinds of arguments used to support this thesis: formal, historical and pragmatic arguments. On this basis, we conclude that ontological reductionism is a metaphysical thesis in a Kantian sense: it is beyond any possible evidence. Furthermore, formal, historical and pragmatic evidence, far from supporting ontological reductionism, provides further arguments for ontological pluralism.

Relational philosophy as a root for an epistemology of chemistry

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Our enquiry first deals with ways of *doing* chemistry with the view to scrutinizing patterns of "ongoing engagement" with the world. Chemical practices incorporate the objects that they are enacted with and on by tailoring networks of interdependencies. We claim that a relational philosophy enables us to develop a practical epistemology of chemistry. To this respect, we not only refer to Rom Harré's concept of 'affordances' and Rein Vihalemm's practical realism, but we also point out how a naturalized relational epistemology can work in partnership with a neo-Kantian epistemology to make this practical account become achievable.

The paradigm changes in the study in the Belousov-Zhabotinsky reaction

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Belousov's original paper, in which he described the discovery of a homogeneous oscillatory reaction and proposed a tentative mechanism for that process, was rejected by two main Soviet chemical journals. Belousov finally managed to publish a brief abstract. Belousov died in 1970. His full paper was published only posthumously in 1981.

Interestingly, "external" forces cannot be blamed for this rejection. Neither the Communist Party nor the state ideologists intervened. It was the scientific community itself, as represented by the editors of scientific journals and their referees, that refused to accept Belousov's discovery.

Philosophy of chemistry against standard scientific realism and anti-realism

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It is stressed that philosophy of chemistry can be seen as having quite a central role in the post-Kuhnian philosophy of science in general and in analysing the debate between scientific realism and anti-realism in standard philosophy of science particularly. The post-Kuhnian philosophy of science is considering science as a practice rather than a network of statements. It is shown that practical realism can avoid shortcomings of both, standard scientific realism and anti-realism. Knowledge cannot be understood as a representation of the world independent of practice and neither can practice be comprehended as not belonging to the real world.

C5. Physics

Invited Lectures

The Flow of Space?

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[No abstract available]

Einstein and the A Priori

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The development of Einstein's theory of relativity is often thought to represent a complete refutation of the Kantian theory of synthetic a priori knowledge, since both Euclidean geometry and Newtonian mechanics are now replaced. Hans Reichenbach (in 1920), however, took Einstein's theory rather to imply a "relativization" of the a priori, whereby different a priori principles are presupposed in different theories (Newtonian and Einsteinian). Such principles still count as a priori, however, since they are "constitutive" of the objects of the relevant theory. I have developed Reichenbach's idea into an essentially historicized version of the a priori, and I argue that the historical evolution of geometry, physics, and scientific philosophy from Newton to Einstein supports the view that this historicized version still counts as "constitutive" and "transcendental" in something like Kant's original sense.

Explaining the approach to equilibrium in terms of epsilon-ergodicity

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Gases reach equilibrium when left to themselves. Why do they behave in this way? The canonical answer to this question, originally proered by Boltzmann, is that the systems have to be ergodic. This answer has been criticised on different grounds and is now widely regarded as fawed. In this paper we argue that some of the main arguments against Boltzmann's answer, in particular, arguments based on the KAM-theorem and the Markus-Meyer theorem, are beside the point. We then argue that something close to Boltzmann's original proposal is true for gases: gases behave thermodynamic-like if they are epsilon-ergodic, i.e.,

ergodic on the entire accessible phase space except for a small region of measure epsilon. This answer is promising because there are good reasons to believe that relevant systems in statistical mechanics are epsilon-ergodic.

Einstein meets von Neumann: operational separability and operational independence in algebraic quantum field theory

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The talk argues that Einstein and von Neumann meet in algebraic relativistic quantum field theory in the following metaphorical sense: algebraic quantum field theory was created in the early 1960's and was based on the theory of "rings of operators", which von Neumann established in 1935-1940. In the years 1936-1949 Einstein criticized standard, non-relativistic quantum mechanics, arguing that it does not satisfy certain criteria that he regarded as necessary for any theory to be compatible with a field theoretical paradigm. It is shown in the talk that algebraic quantum field theory satisfies those criteria and hence it can be viewed as a theory in which the mathematical machinery created by von Neumann made it possible to express precisely the physical intuition about field theory by Einstein.

Contributed Papers

Special relativity prohibits spacelike causation and some implications

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We refer to the claim that the theory of special relativity Th(SR) "does not prohibit spacelike (faster-than-light) causation". Let A be the assumption of "a causal connection between spacelike events", formulated as a binary symmetric relation Q on Minkowski space-time $\mathcal{M}$ preserved under all automorphisms $g \in \mathbf{P}\uparrow_+$ of $\mathcal{M}$, where $\mathbf{P}\uparrow_+$ is the *proper orthochronous* Poincaré group. Adding the assumption A to Th(SR) leads to a *contradiction.* Since Th(SR) $\wedge$ A is *inconsistent,* it follows that Th(SR) $\vdash \neg$ A, and $\neg$ A is a *theorem* of Th(SR). Whence, Th(SR) prohibits spacelike causation.

Spacetime as a causal set: Universe as a growing block?

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The current state of development of the causal set program towards a quantum theory of gravity is situated *vis-à-vis* the long-standing debate between eternalism (block theory) and past-presentism or possibilism (growing block theory) in the philosophy of time. It is argued that despite "appearances" and declarations to the contrary, the program does not side with growing block theorists when it comes to buttressing a robust notion of Becoming—at least, not more than familiar relativistic theories on continuous spacetime manifolds. The problem stems from the postulate of discrete general covariance—a requirement imposed upon the only worked out kind of dynamics for causal sets to date, a dynamics of a *classical* stochastic process.

The conceptual meaning of reduced states: Decoherence and interpretation

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The interpretation of reduced states as quantum states is a common practice in physics. The aim of this paper is to reject this usual interpretation on the basis of a twofold argument. First, we will argue that the reduced state of a decohering system is a coarse-grained state, analogous to the coarsegrained descriptions of

unstable classical systems. Second, in the context of no-collapse interpretations we will show that, even in the case of non-interacting subsystems, the move of conceiving their reduced states as their legitimate quantum states precludes the account of consecutive measurements.

From the hole argument (A. Einstein) to the ball of clay argument (H. Weyl)

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The *hole argument* is a story invented by Albert Einstein to set out the difficulties he had in reconciling his principle of covariance with the Mach's principle. Some modern presentations insist that the bare differential manifold loses its physical status and is just an expression of the mathematical framework of the new relativistic theory.

In *Space-Time-Matter,* Hermann Weyl constructed an argument close to the modern hole argument, replacing the hole with a ball of clay. Beside some trivial technical differences, Weyl's argument is original because of the particular concept of mathematics that he inherited from the Gottingen school.

How to exchange quantum particles of the same type

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The mathematical notion of the permutation of indices in the state description admits different physical interpretations. Two interpretations analyzed in this paper are the exchange of essences and the exchange of haecceities. It is argued that adopting the first interpretation leads to the conclusion that quantum particles of the same type are discernible by their properties. The indiscernibility thesis (known as the Received View) can be supported by the alternative interpretation in terms of primitive thisness. Further arguments in favour of the discernibility claim are considered, including a formal analysis of the notion of entanglement of particles of the same type.

Fundamentality, elementariness and scales

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In this paper I consider how the fundamentality issue in physics (What is fundamental?) is connected with the issue of elementariness (What is an elementary physical object?). The idea is that scale considerations are of central relevance in discussing these issues and their interrelationship. This is particularly manifest when considering the special case of interlevel relations represented by

physical dualities, today at the core of the most important developments in theoretical physics. Here, I focus on two types of dualities: the duality of the so-called Dual Resonance Model (from which early string theory was born) and the Montonen-Olive electric-magnetic duality.

Information measures induced by partial Boolean algebras

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In the context of recent researches about the fundamental nature of quantum information and the information-theoretic interpretation of quantum mechanics, we formulate the conditions for the introduction of a quantitative approach to information as induced by the partial Boolean algebras of the properties of a quantum system and we exploit the Zeilinger's measure as a measure of *semantic information* about the maximal Boolean sublattices of quantum propositions *emergent* in a measurement process. Also, an information-theoretic interpretation of the notion of quantum state is discussed.

Many worlds, the cluster-state quantum computer, and the problem of the preferred basis

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I argue that the many worlds explanation of quantum computation is not licensed by, and in fact is conceptually inferior to, the standard neo-Everettian interpretation of quantum mechanics from which it receives its inspiration. I argue that the many worlds explanation of quantum computation is incompatible with the more recently developed cluster state model of quantum computation. Based on these considerations I conclude that we should reject the many worlds explanation of quantum computation.

From independent models to a unified theory of dynamics

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Scientific models are extraordinarily effective for exploration and appear wonderful. When they claim at explanation, they often become monsters. A fully rational monadological theory is proposed to tame the monsters of dynamics and respect its wonders. The basic models (variational, geometrical and group theoretical formulations) are deduced, with their leading parameters, from a "mother structure". This Leibnizian approach of nature, initially rejected by physicists, but lateley favored by scientists as Gödel, Weyl or Reichenbach is

revived. It allies exploration and explanation by introducing an inclusive logical framework (treelike structure) that unifies the usual exclusive logical ones (models corresponding to branches).

Explanation, explication, and interpretation of space-time theories

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Recent literature on space and time examines the distinction between fundamental and derived principles in physical theories. Focusing on these issues, I hope to illuminate the explanatory roles of space-time structures in relativity theory. A relativistic space-time does not *explain* geometrical relations, by specifying an underlying reality of which the latter are the phenomenological consequences. But relativistic space-time does explain the significance of electrodynamics and gravitation for our understanding of the nature of space-time, and the conceptual revision required by Einstein's theories. This analysis suggests a critical understanding of the relation between space-time structure and dynamical laws, especially the geodesic principle, in general relativity.

Specious present in branching space-times

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The doctrine of specious present claims that the present is not momentary, but is an interval. Most recently, specious present appeared in the discussion on famous Putnam's argument for inconsistency of special relativity and the becoming. In my presentation I shall discuss the place of specious present in branching space-times theory (BST). I will show that there exist natural candidates for specious present in the particular class of BST models (branching space time with space-time points), and that one needs no additional axioms to introduce specious present to BST. The structure will be tested as a semantics for tensed expressions of natural language.

Testing universal gravitation in the laboratory, or the significance of research on the mean

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This paper seeks to provide a historically well-informed analysis of an important post-Newtonian area of research in experimental physics between 1798 and 1898, namely the determination of the mean density of the earth and, by the end of the nineteenth century, the gravitational constant. Traditionally, research on

these matters is seen as a case of 'puzzle solving.' In this paper, I show that such focus does not do justice to the evidential significance of eighteenth- and nineteenth-century experimental research on the mean density of the earth and the gravitational constant.

On the status of temporal unidirectionality in physics

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I assess the concept that time has a privileged direction (temporal unidirectionality), and its relation to physics. The first half considers a naturalist approach to temporal unidirectionality implicit in the literature, and argues that it fails to show that we can (in principle) have direct epistemic access to temporal unidirectionality. The second half considers the role of temporal unidirectionality in physical explanations—focusing on the past hypothesis, and argues that this is insufficient for an inference to best explanation in favour of temporal unidirectionality.

Incantations of 'causation' and other philosophical sins, or: Rehabilitating Ritz

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This paper critically examines Earman's recent investigation of the so-called "arrow of radiation" and his review of the Einstein-Ritz debate on this issue. I show that Earman (like most commentators before him) misinterprets Einstein's position and that Einstein himself, in characterizing the classical electromagnetic radiation asymmetry, invoked the very production—talk disparaged by Earman. I then argue that Earman's own appeal to statistical considerations to explain the asymmetry is guilty of what Huw Price has called "the temporal double standard fallacy" and is ultimately unsuccessful. Finally, I defend a causal account of the asymmetry against Earman's charge of being ill-defined.

Objectivity and physical symmetries

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In recent philosophy of science literature, objectivity or more precisely the objective part of scientific theories has been operationally, and sometimes metaphysically, linked to symmetry and invariance. See for example [3] or [1]. But which

symmetries are appropriate to obtain representations that are more objective? In this paper, we argue that perfect symmetry [2] is one of these symmetries. However, using perfect symmetries generates counter-intuitive cases that must be explained. Some conclusions about the kind of objectivity accessible through physical theories follow.

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Bohr's model of the atom: Methodology, consistency and fruitfulness

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In this work, I show that John Norton's attempt to construct an inherently consistent version of Bohr's atomic model, through the method of 'demonstrative induction', results in depriving this model of any heuristic dynamics. On the contrary, Bohr's methodology, by transforming inconsistency into *vehicle* for approaching a consistent theory of subatomic phenomena, proved capable of guiding the scientific research, for over a decade, towards quantum mechanics, a theory fundamentally incompatible with the preceding knowledge. On these grounds, I argue that, contrary to Norton's view, Bohr's model and its gradual development offer strong evidence for the *continuous and non-cumulative* development of scientific knowledge.

Mach's principle and the philosophy of space/time: What nature is trying to tell us

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Is the absolute vs relational debate concerning space or spacetime settled, in light of General Relativity, in favor of absolutism? Philosophers may think

so, but many physicists would demur, chosing to view the apparently Machian-relational character of certain key models as the key to understanding the status of inertial spacetime structure in GR.

In this paper I will review the successes of Mach's Principle, focusing especially on recent work by Schmid and Lynden-Bell *et al.* demonstrating *exact* frame-dragging: in FRW cosmologies, when a spherical shell of cosmic matter is "set in rotation", if the shell gets big enough local inertial frames are dragged into complete lock-step with the (allegedly) rotating cosmic matter. I will argue that the attitude of the Mach-friendly physicists is methodologically more defensible than the anti-relationist philosophy consensus.

Correspondence truth and quantum mechanics

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I argue that the semantics underlying the propositional structure of classical mechanics allows truth-value assignment in conformity with the traditional conception of a correspondence theory of truth. Every proposition in classical mechanics is assigned a definite truth value, either 'true' or 'false', describing what is actually the case at a certain time. Truth-value assignment in quantum mechanics, however, differs; it is known, by means of a variety of 'no go' theorems, that it is not possible to assign definite truth values to all propositions pertaining to a quantum system. In this respect, the Bub-Clifton 'uniqueness theorem' is utilized for arguing that truth-value definiteness is restored with respect to a determinate sublattice of propositions, defined by the state of the quantum system concerned and a suitable 'preferred' observable. I point out that the resulting account of truth, subscribing to a scheme of contextual correspondence, is compatible with a realist conception of truth; it is both non-epistemic and non-relative. Such an account essentially denies that there can be a universal context of reference or an Archimedean standpoint from which to state the totality of facts of nature.

Rotating universe [1]

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Gödel solved Einstein's cosmological equations in a model of a homogeneous, isotropic, and non-expanding universe that rotates. This result guarantees that general relativity and absolute space are independent issues because there are solutions that rotate and others that do not. Gödel explored consequences for the philosophical understanding of time and causality, which he explained by appeal to possible worlds. This is unexpected, since Gödel's later metaphysical picture has been associated with actualism. But the association is mistaken. The purpose of this task is to argue that Gödel is not an actualist.

1. Thanks very much to Charles Parsons, Raul de la Fuente Marcos, Walter Johnson, and Akihiro Kanamori. Thanks to Daniel Kennefick and John Statchel. Thanks also to Kip S. Thorne, and to my research assistants Nicole T. Russell and Sarah R. Beiter.

An illustration of the importance of the epistemological point of view and of the context in Sciences: the astronomical refraction case during the 18th century

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The way stars can be located was an essential question during the 18th century. Scientists were aware that some phenomena related to astronomy and optic could disturb stars observation and thus avoid an accurate spotting. It led to an essential improvement in the knowledge of astronomical refraction phenomenon, which will be studied and discussed from an epistemological point of view. We will show the influence of the scientific and political context on the use of the analytic method. Then, we will show how the study of this questioning allows us to better understand the mathematisation process of physics and notably the contribution of the recently discovered infinitesimal calculus of Leibniz.

Going round the lack of time: Enforced entrusting and silent inter-expertise trading in time-short nanomagnetism knowledge making

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This paper is an epistemo-socio-ethnographical exploration of neutron physics experimental practices in nanomagnetism research, specifically analysing 'time'-related issues. 'Time' here refers to the 'beamtime' in chronic shortage in such experiments and to the temporal sequence of specialism-dedicated 'moments'. '*Lacking time*', in the experiment observed, is shown to enable productive collaboration across expert know-how and skills involved, e. g. magnetism, neutron physics, as lacking time led participants to entrust and trade across expertise boundaries. Briefly, I discuss the counter-intuitive possibility of scientific productivity being potentially enhanced, instead of hindered, by *lacking* time while making knowledge.

Circumveiloped by obscuritads

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The quest for finding the right interpretation of quantum mechanics is more than 80 years old. The question what an *interpretation of quantum mechanics* is has never been raised, let alone answered. We raise it and try to answer it.

The prospects for quantum state monism

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This paper defends the viability of *quantum state monism*—the view that everything there is in the world can, in principle, be specified by specification of a quantum state—against recent arguments that a quantum state specification must be supplemented by some additional primitive ontology. A distinction must be made between supplying correspondence rules, which tell us how to interpret a quantum state description as referring to a world that includes objects in threedimensional space, and adding ontology beyond the quantum state. It will be argued that we can successfully achieve the former while remaining quantum state monists.

Why quantum non-locality implies parameter dependence

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According to the standard view, the experimental violation of a Bell inequality is a consequence of the fact that the total probability distribution does not factorize into local terms and this quantum non-locality is equivalent to the disjunction 'outcome dependence or parameter dependence'. I show that this notion of quantum non-locality is too strong because there are other factorization conditions which allow deriving a Bell inequality. An appropriate weaker notion of quantum non-locality can be analyzed as a probabilistic dependence between each outcome and both its distant and local parameter, whereas a dependence between the outcomes is neither necessary nor sufficient.

Bell's Lorentzian pedagogy: A bad education

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Bell's 'Lorentzian Pedagogy' has been extolled as a constructive account of the relativistic contraction of moving rods. Bell claimed advantages for teaching relativity through the older approach of Lorentz, Fitzgerald and Larmor. However, he describes the differences between their absolutist approach and the relativistic one as philosophical, and claims that the facts of physics do not force us to choose between them. Bell's interpretation of the *physics* of motion contraction, and therefore of constructivist as opposed to principle approaches, is obscure. His flawed pedagogy never clarifies a difference between Fitzgerald and Lorentz contractions.

Relational logic and modern science

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Quantum Mechanics, the outstanding theory of the 20^{th} century shaping the most diverse natural phenomena, is lacking a foundational principle. Recently we have suggested that relational logic, a form of logic developed by C. S. Peirce in the years 1870-1880, may serve as the conceptual foundation of Quantum Mechanics. The primary irreducible notion of the Peircean logical system is the notion of relation. Two relations may be composed, giving rise to a third relation, and this composition rule leads to the essential laws of Quantum Mechanics (the probability rule, the commutation rules). A double line representation for relations leads to the depiction of the quantum logical process as a "stringy" geometry. Thus, we reach a deeper and more integrated understanding, where Logic, Quantum Mechanics, String Theory, Geometry, are brought together. The emerging new paradigm invites us to view logic as an experimental science and science as a logic argument.

Einstein's philosophy and the origins of post-critical philosophy of sciences

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This presentation aim at proposing another approach of Einstein's philosophy, which does not mean that we intend to bring out radical novelties. The purpose is rather, from already known features of Einstein, to highlight some relations with one of the main orientation in philosophy of sciences, the post-critical. Karl Popper confessed his debt to Einstein, but Kuhn, Feyerabend and Hanson should have done the same; it will be our purpose to bring out the main features of their works, through which this could be say. Exploring Einstein's Philosophy from our knowledge of post-critical philosophy, we will analyse some concepts like Normal science (Kuhn), Theory loaded facts (Hanson) or Epistemological anarchism (Feyerabend) in order to underline their Einsteinian roots. This approach is not causal, but try to present history in a kuhnian way that is, to show that there is a link between the latter and the first which manifest a same preoccupation, to solve some puzzling issues of quantum mechanics.

Would superluminal influences violate the principle of relativity?

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It is widely held that the existence of faster-than-light information transmission would undermine the Principle of Relativity in that it would allow the detection

of a putative absolute state of rest and violate the relativity of simultaneity. It can be readily shown that the first claim rests upon mistaken notions of how superluminal motion is represented in relativity, and the second claim can be answered (though less definitively) by pointing to the possibility of alternative conceptions of simultaneity.

Forces—relations or dispositions?

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Realists about Newtonian Forces are confronted with the task of assigning an ontological category to these entities. I discuss how the two main proposals in the debate, forces as relations and forces as causal powers, can deal with features that forces must have according to a standard view of Newtonian mechanics. As both of these proposals face considerable difficulties I suggest an alternative approach that takes forces as intermediaries in a chain of dispositions and their manifestations.

Causal relevance of measurement operations in the EPR paradox

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This paper addresses the ontological implications of a possible common cause model for the EPR correlations. The main characteristic feature of the model hast to do with the causal relevance, which is made explicit, of measurement operations for the postulated common causes, and hence as regards the final outcomes as well. These kind of dependences allow for the model to avoid the charge of Bell's theorem, which is commonly taken to rule out explanations of the EPR correlations in terms of common causes. The model displays however a certain non-locality which suggests an ontological revision of the events involved. Two are the interpretations proposed for the postulated common causes. On the one hand, common causes may be viewed as non-localised events which operate causally in a local manner. Alternatively, the common cause events may be taken to be well defined localised events in space-time with non-local causal powers.

Leibniz: Symmetry and harmony

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Starting from the criticism to the concept of *force* in Descartes and from the advent of the *calculus* I state that it is possible to associate the origin of the modern concept of *symmetry* to Leibniz, or better yet, to leibnizianism. However, differently from the mathematician Hermann Weyl, that relates this genesis to the *principle of the identity of the indiscernible,* I argue that the concept of *harmony* sustained by the *principle of previously established harmony* is the, *de facto,* starting principle of modern *symmetry,* and, in particular, of the concept of *translation symmetry* also known as *spatial symmetry.*

The gravity of the past hypothesis: Lessons learnt from Earman and Wallace

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In this paper I compare the analyses of gravity and the Past Hypothesis presented by Earman [1] and Wallace [2] , and consider whether Wallace's account undermines one of Earman's main objections; whether the two accounts simply refer to different cosmological epochs; and to what extent they may be seen to complement one another. I will show that a synthesis of the two, plus a rigorous cosmological history, leaves us with a clearer picture of the difficulties posed by the Past Hypothesis, and some guidance with the considerable work that is outstanding in tracing the origins of the Second Law of thermodynamics.

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Problems and promises of scientific method

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This paper discusses the problems and shortcomings of the scientific method in light of certain philosophical considerations advanced by Plato.

The dialogue between science and philosophy is both real and inevitable. For example, the search for knowledge about how to obtain knowledge is common to both scientific method and epistemology. Thus any progress in epistemology could shed light on the problems associated with scientific method.

Plato's philosophy, his metaphysics and epistemology, is a fitting solution to both the aberrations in nature which occasionally defy scientific laws and to the inconsistent relationship between fact and theory which has marked the history of scientific endeavour and undermined the authenticity of scientific method.

Bohmian mechanics without wave function ontology

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Albert [1] claims that a realist attitude towards Quantum Mechanics in any of its interpretations—and, in particular, Bohmian Mechanics—commits to the existence of configuration space. Here, I reject Albert's claim and I critically examine different three-dimensionalist interpretations of Bohmian Mechanics in order to ascertain the prospects of doing Bohmian Mechanics without wave function ontology. I present an objection against Belousek's [2] three-dimensionalist interpretation, which is based upon the quantum potential approach to Bohmian Mechanics. I then offer an alternative three-dimensionalist interpretation within the guidance approach to Bohmian mechanics.

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Is inertia explained in general relativity?

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In General Relativity, in contrast to the situation in other theories such as Special Relativity, it has been claimed that inertia receives a dynamical explanation because the geodesic principle can be derived from Einstein's field equations. This claim can be challenged in different ways, all questioning whether the status of inertia in GR is physically different from its status in previous spacetime theories. In this paper I state the original argument precisely, discuss the different objections to it and then propose a formulation that is free from the problems encountered by the original claim.

The case for quantum state realism

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One enduring strategy for addressing the so-called 'quantum measurement problem' is to endorse an antirealist interpretation of the quantum state. With recent developments in quantum information theory, a new variation on this antirealist theme has emerged which interprets the quantum state as a representation of the subjective beliefs or information of agents interacting with quantum systems, rather than a description of physical reality. Using examples taken from the evidentiary basis of quantum mechanics, I argue that this antirealist interpretation is physically unmotivated. I then attempt to articulate a version of quantum state realism that can be defended against certain obvious objections.

Proving the principle: General relativity and geodesic

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In this paper I critically review attempts to formulate and derive the geodesic principle, which claims that massive bodies follow geodesic paths in general relativity theory. I argue that if the principle is (canonically) interpreted as a law of motion describing the actual evolution of gravitating bodies, then it is impossible to apply the law to massive bodies in a way that is coherent with Einstein's field equations. Rejecting the canonical interpretation, I propose an alternative interpretation of the geodesic principle as a type of universality thesis analogous to the universality behavior exhibited in thermal systems during phase transitions.

A proposition called T0906 and Gödel's incompleteness theorems

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By the expression 'classical phenomena', we mean the physical phenomena expressed in terms of classical theory, i.e., Newtonian mechanics and Maxwell's theory of electro-magnetic field. All observable and measurable physical phenomena are known to us as the information given through the five senses. They are all 'classical phenomena'. We use 'classical phenomena' and Proposition T0906, explained in the extended abstract, to analyze the relations between classical mechanics and quantum mechanics. Using analogy with Goedel's incompleteness theorems and the Proposition T0906, we will argue that there is a layered structure, starting from classical mechanics, quantum mechanics, and more fundamental theories.

Paradoxes of transfinite cosmology

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After a short survey of the main historical concepts of infinity, especially in Aristotle, Kant and Cantor, the question is considered whether the modern cosmology has solved the first Kant's antinomy, i.e., whether the universe is finite or infinite in space and time, and in this context also some topological issues are discussed (torus etc.). Then the concept of "multiverse" is analyzed, and some recent cosmological theories of multiverse(s) are discussed from the methodological point of the set-theory. If multiverses are conceived as sets of universes, then the concept of "Multiverse of all multiverses" yields to be paradoxical, and it seems that a return to some "meta-concept" of the Universe is unavoidable. In this sense, Cantor's "Absolute" is proposed as a possible philosophical solution of the paradoxes of infinity also for cosmological multiverses, and in the conclusion of the paper, the analogy between Cantor's Absolute, which is not a "mathematical object", and Kant's conception of the Whole of the World (in space and time etc.), which is not a constitutive category of reality, but just a "regulative idea" of transcendental dialectical thought, is proposed and discussed.

Quantum theory beyond physics

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Quantum theory has been developed to deal with *physical* phenomena. However, the key quantum concepts of complementarity and entanglement can be exported *beyond the field of quantum physics,* for example to account for the complementarity of different dynamical descriptions of classical systems, and

even *beyond the material realm,* as in perception theory or to deal fruitfully with the long-standing and still unresolved mind-body problem. This direction of research can be rigorously explored within a generalized, algebraic formulation of quantum theory where a strong link can be established between the non-commutativity of generalized observables and the existence of non-local correlations.

The past of a quantum particle

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A method for analyzing the past of a quantum particle according to the weak trace it leaves is proposed. Such a trace can be observed via measurements preformed on an ensemble of pre- and post-selected particles. An example, in which this method contradicts the common sense description of the past of the particle is presented. It is argued that this naive approach has to be replaced by a description of the past of the particle by both forward and backward evolving quantum states.

Probability is composed. The frequency interpretation of probability revisited

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In the following we propose a variant of the frequency interpretation of probability of Richard von Mises; one of our aims is to address recent criticisms that have been formulated against this interpretation. Following von Mises, we will argue that (objective) probability can only be defined for events that can be repeated in similar conditions, and that exhibit 'frequency stabilization'. The central idea of the present article is that the mentioned 'conditions' should be well-defined and 'partitioned'. More precisely, we will divide probabilistic systems into object, environment, and probing subsystem, and show that such partitioning allows to solve problems. By the same token we will be able to derive a definition of what 'similar events' are—a problematic concept in traditional interpretations. Our general conclusion will be that the probability of an event or system is only defined if all subsystems that compose the latter are defined—in a slogan: *probability is composed.*

Contributed Symposia

Symposium

Epistemological perspectives on the Large Hadron Collider

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In March 2010, physicists working at the Centre Européen pour la Recherche Nucléaire (CERN) in Geneva began collecting data on the proton-proton collisions occurring inside the Large Hadron Collider (LHC). Expectations are high for the LHC experiments, for one, to discover the Higgs particle, the long sought-for closing stone of the Standard Model of Elementary Particle Physics (SM), and—independently of whether the SM is thus confirmed or not—to find indications of 'new' physics 'beyond the Standard Model' (BSM), such as supersymmetric particles or dark matter. Theoretical physicists have come up with a large variety of models covering virtually all conceivable outcomes of the LHC and other experiments reaching comparable energies, among them Tevatron. Experimental physicists at LHC, on their part, have designed two universal and some more specialized detectors that are able to precisely test the models and theories proposed, and simultaneously strive to discover new phenomena that are not covered by them. For an experimental device as theory-laden as a modern elementary particle detector, this combination of diverging objectives amounts to a delicate balance of experimental strategies and prior decisions concerning the relevance of certain data. The aim of the proposed symposium is to discuss some key issues from the wide spectrum of epistemological problems posed by the LHC-experiments and investigate to what extent this case study carries important lessons for current philosophical debates about gauge theories, models, experiments, and data evaluation.

Most models in elementary particle physics start from the SM developed by Glashow, Weinberg and Salam in the 1960s and early 1970s, but assume that the SM is not the last word, even before one reaches the lofty energy scale of string physics. Some of them imply that a Higgs particle must eventually be found by the LHC, others dispense with the need of such a particle altogether and are, accordingly, named 'Higgs-less'. On some accounts, the Higgs particle only appears as a by-product of the mass generation mechanism, while others consider it as a primary entity. All of them, however, address the basic problem of the SM, to find a way in which the particles of the SM acquire their mass, and they are—positively or negatively—related to at least one aspect of the rather involved argument by which the SM achieves this, the so-called Higgs mechanism.

Departing from a still preliminary chart of the model landscape of physics in the Higgs sector, the paper of Stöltzner investigates to what extent the models

mediate between theory and data and in what sense they can be ascribed representative features. It turns out that most models in the Higgs sector are related not only to the SM—which is a good a theory as one can expect in high energy physics—but also to various hypothetical theories BSM. This adds another dimension to the conception of models as autonomous mediators between theory and data put forward by Morgan and Morrison because the predictive content of Higgs models is related to theories of different levels. Second, many models cluster around shared heuristic principles, most of which transcend the context of a specific model or even the SM, some even of particle physics altogether, and provide stories—in Stephan Hartmann's sense—that may divide or prompt a secondary unification of the model landscape. It is important to note that in various forms such principles and stories have been present throughout the whole history of the SM. In virtue of their multi-level relationships, the representative features of the models in the Higgs sector are less evident than for quantum chromodynamics, the theory of quarks, because they both involve a complex theoretical mechanism of mass generation and predict one or more—or sometimes even none—Higgs particles.

For these reasons, models that take the Higgs particle not as a basic entity on a par with the other particles in the SM have always attracted a certain attention. The paper of Borrelli studies one such theory, called 'technicolor', from its beginning in the 1970s through a series of transformations and integration of additional ideas until its present state. She concludes that in today's high-energy physics models are usually not regarded as prototypes to be studied further—a property that Giere's semantic approach would emphasize—, but are, more often than not, set aside right after they have been used to prove or disprove some initial hypothesis. What is kept and further explored is not the model itself, but one or two of its central ideas, at times a specific mathematical construct (a 'mechanism'), at times more generic notions ('supersymmetry', 'extra dimensions') which have a formally less precise character and whose success may be due to a good 'story' in the sense of Hartmann. This dynamics has become typical for frontier research in theoretical high energy physics, and aims not so much a constructing a 'final theory', but rather at collecting formal elements and general ideas which may turn out to be useful to interpret unexpected experimental evidence found by the LHC or other experiments.

Contributions

- **Higgs models and other stories about mass generation**

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- **The practice of model-building in contemporary high energy physics: the example of "technicolor"**

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- Selectivity and robustness in experimental research: Lessons from the Large Hadron Collider

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C6. Medicine

Invited Lectures

Data driven research and large scale studies in biomedical research: What consequences for data sharing and bioethics in human genetics?

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Since the 90s the various steps of whole genome projects and high throughput technologies have led to an important change in the way part of the biological research is conducted. From the classical hypothesis driven research, a data driven research has appeared taking roots in the capacity of the technologies associated to bioinformatics tools to generate systematically and to operate large scale raw data sets. This has profound implications in the research methodology and organization as well as in relations between disciplines involved and led to collaborate. Taking the human genome project and the use of biological samples and associated databases as an example, the following steps and recent switches in conducting genetic research can be identified:

- From biological material issues to challenges about data generated from them
- From time defined use to unlimited timeframe
- From targeted testing to whole genome
- From defined uses to unpredictable uses
- From team related research to international sharing
- From separation of clinical and research use to blurred limits
- From side and ill defined activity to central strategic activity (biobank)
- From research teams to research infrastructures
- From a biobank/database to a network of biobanks or centralized/federated databases.

We shall especially analyse the consequences of such changes in human diseases genetic studies data sharing and in the ethical issues attached. An important aspect is the evolving status of genetic information available and the inadequacy of informed consent as usually practiced in clinical research. The consequences of the following elements will be analysed:

- The definitions of "what is a result?": Data versus results; raw data versus interpretated data
- Changing of degree of identifiability of persons as data are accumulated over time
- Changing in capacity of interpretation of existing data due to progress in knowledge
- Mixture of health related/non health related information
- Production of genetic information "clinically unuseful at least presently" routinely

- Issue of what is done with this "extra information" (notion of filter)
- Perception of genetic information changing in the public
- Source and access to genetic information; this is changing (direct to consumer offers, internet sharing of information)
- Evolution from ethics committees opinions prior to research only to sustainable governance structures of biobanks/databases.

Finally regarding data sharing, various models emerge and the sharing of large scale data sets, called "community resources" prior to publication is an issue of considerable debate. Some elements of this evolution have or will have consequences on evaluation of research, on relations between research participants and researchers, and on education to science.

Evidence-based medicine and mechanistic reasoning in the case of cystic fibrosis

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Evidence-based medicine and mechanistic reasoning are two distinct and powerful research methodologies. Enthusiasts of one sometimes take a dim view of the other. For example, evidence-based medicine is founded on skepticism about "pathophysiological rationale" (a broad category that includes mechanistic reasoning) and those who are knowledgeable about basic mechanisms are often unimpressed by the "empiric" results of evidence-based medicine. In this paper I plan to illustrate the importance of *both* methodologies and argue that they are not in competition with one another much of the time. I use cystic fibrosis as a case study.

In the 1950s, children born with cystic fibrosis (CF) rarely survived long enough to enter first grade. Today the mean life expectancy is almost forty years old. The increased lifespan has come incrementally, as antibiotics, airway clearance techniques, pancreatic enzymes, bronchodilators, ibuprofen and mucus thinners (Pulmozyme and hypertonic saline) were added as standards of care. The Cystic Fibrosis Foundation (begun 1955) has funded and coordinated many of the clinical studies, and uses the results to produce evidence-based guidelines for care in the 100 or so Cystic Fibrosis Care Centers. The steady progress and general consensus on proper care is impressive. In a 2004 article Atul Gawande writes that "CF care works the way we want all of medicine to work," and especially praised it for being "system based." This high praise is for interventions that are not technologically or intellectually sophisticated. It is an epistemic irony in CF research that our most precise evidence is about our crudest interventions.

The gene for cystic fibrosis was discovered in 1989, and for a period during the 1990s researchers thought that they were on the cusp of producing effective gene therapy for CF (Lindee forthcoming). Although a mouse model of CF was cured with gene therapy, clinical trials failed. Over the last ten years, we have learned much more about the mechanisms underlying CF. They turn out to be much more complex than anticipated, as well as more variable from person to person. Understanding of the role of the CFTR protein has led to attempts to fix the misfolding of the protein that is coded in the CF gene. The NEJM

recently reported successful stage 2clinical trials for a substance, VX770, that can correct one type of misfolding. Stage 3 trials are in progress. So, for the first time, we have RCTs for more sophisticated interventions, making use of our knowledge of CF genetics and genomics.

Mechanistic reasoning is highly fallible, perhaps because it generally provides a simplified or partial model of the world. Yet mechanistic reasoning is indispensible—we would have few ideas about how to design RCTs without mechanistic hypotheses about how to intervene in the disease process. Evidence-based medicine and mechanistic reasoning do not in general compete in the case of cystic fibrosis. Rather, they operate at different stages of the research process, with mechanistic reasoning in the earlier stages of discovery and evidence-based medicine typically in the later stages of developing interventional success.

Contributed Papers

Vulnerability from infectious diseases and social determinants of health: In search for an ontology to guide health policy development

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Vulnerability to infectious diseases is universal, as underscored by recent global outbreaks of new strains of viruses and existing threats from TB, HIV, etc. However, existing social determinants of health contribute differently to infectious disease emergence, distribution and effect on different population groups. This paper aims to deliberate on which ontology should guide our choices for policies and intervention strategies for addressing vulnerability to infectious diseases in the pursuit of *health for all* when our societies have wide disparities but our vulnerability is increasingly interconnected.

How to make the research agenda in the health sciences less distorted

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A well-known problem in the health sciences is the distorted research agenda: the agenda features misleading research, it features too little research that is tailored to the health problems of the poor, and it features too little research that supports the development of non-profitable solutions to health problems (e. g., change of lifestyle). This article analyzes these three sub-problems in more detail, and discusses, for each sub-problem, several strategies to deal with it, resulting in some specific recommendations on how to reform the health sciences.

Between variability of the body and determinism of the care: a "mediated" relation

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Le remède se situe entre déterminisme et indéterminisme. Les progrès de la pharmacologie moderne nous orientent vers des remèdes de plus en plus spécifiques. Or, la thérapeutique moderne implique de repenser l'homme sous l'angle de la variabilité face aux traitements. Dès lors, cette variabilité implique une

médecine de l'individu pour laquelle la catégorie de l'universel n'a guère de place. Il faudra donc considérer l'homme comme acteur de médiations multiples d'une part, avec le remède et d'autre part, avec le médecin-prescripteur. Par conséquent, ces médiations qui refondent le discours médical seront interrogées en nous portant sur leurs aspects épistémologiques et éthiques.

When society speaks to science: Politics, social representations and industrial interests in the medical definition of the concept of addiction, in the case of tobacco and nicotine

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The definition of addiction and especially tobacco addiction was influenced by industrial and political interests. They first contributed to promote tobacco, creating what is now termed a worldwide epidemic. They also made up social representations and explicitly influenced even the scientific theory and the official US and WHO medical definitions of addiction, in order to prevent health protective regulation from impeding a very profitable business. Thus, tobacco addiction and the related diseases are not just biomedical phenomena: addiction is typically biosocial, and its scientific definition embodies major socio-political aspects, as is precisely documented in recent papers.

Ontological and moral status of human-nonhuman animal mix organisms

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Creating human-nonhuman mix organisms increasingly became scientific need for stem cell research, model for drug or pathogenesis research and xenotransplantation, and some forms of chimeras and hybrid have already been created. This paper will explore ontological and moral status of these human-nonhuman animal mix organisms, and the implications that this exploration would have for public policy decision.

How and why to epistemologically study applied bioethics to nanomedicine

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In this work I will define Applied Bioethics as an instance of "Regulatory Science", which makes of it an appropriate case to be empirically analysed by

current Epistemology. Then, I will explain that, for proposing an appropriate epistemological evaluation of what might be or nor be *good* examples of this Regulatory Science, then epistemologists should follow the so-called "Political Turn" in Epistemology. Finally, I will defend that paying attention to this kind of expert *assessing knowledge* production would enrich both Bioethics and Social Epistemology, because their approaches they would better account for the actual shaping of society by current Nanomedicine.

Experimental philosophy and evidence based medicine: Two criticized ways of doing science

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Experimental philosophy (EP) and evidence based medicine (EBM) are hot topics in the current philosophical and/or medical debates. They have both been criticized for several reasons. We will examine the methodological properties of EBM and EP. By doing so our first point will be made and it will become clear that some of the objections against them aren't grounded when we analyze what they exactly are and could be in practice. The second issue is taking the problem to a different level by showing the possibility of a cooperation of both disciplines in the light of narratives and qualitative research.

Philosophical and ethical issues in use or abuse of human body and its parts in biomedical technologies

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With the wide application of biomedical technologies the philosophical and ethical issues in use or abuse of human body and its parts have been raised and debated increasingly among scientists, philosophers and regulators. This paper will address the issues of whether human body or any of its functional parts should be regarded as a property, a commodity for sale by living donors, or a gift to others.

C7. Environmental Sciences

Invited Lectures

The nature and significance of selective ignorance in environmental research

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A number of scholars have recently argued that we could learn a great deal about the nature of scientific *knowledge* by focusing more attention on the nature and causes of scientific *ignorance*. For example, historian Robert Proctor notes that ignorance can take at least three forms: (1) ignorance as a native state or starting point; (2) ignorance as a selective choice or lost realm; and (3) ignorance as a strategic ploy or active construct. Regarding the phenomenon of ignorance as a selective choice, he notes:

> We look *here* rather than *there*; we have the predator's fovea (versus the indiscriminate watchfulness of prey), and the decision to focus on *this* is invariably a choice to ignore *that*.

In this paper, I use recent agricultural research as a case study for exploring the range of factors that contribute to selective ignorance in science. I argue that these factors include not only obvious decisions to pursue some research topics rather than others but also more subtle choices about what metrics to employ, what research strategies to pursue, and even what language to use for describing phenomena. I will also consider the social significance of selective ignorance in science and some strategies for managing it in socially responsible ways.

Contributed Papers

Hybrid models, climate models and inference to the best explanation

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I examine the warrants we have in light of the successes of a kind of models I call 'hybrid models', a kind that includes among its members climate models. I argue that these warrants' strengths depend on inferential virtues that are not just explanatory virtues, contrary to what would be the case if inference to the best explanation (IBE) provided the warrants. I also argue that the warrants in question, unlike those IBE provides, guide inferences solely to model implications the accuracy of which is unclear. My conclusion provides criteria of adequacy for epistemologies of climate and other hybrid models.

Let the water flow to the city: A recent history of the water saving technologies for agriculture in China

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This article looks back on the history of water consumption in agriculture in China since 1949 to the first decade of 21st century, and gives a main focus to the more recent governmental discourse to persuade the farmers to adopt agricultural water saving technologies to prevent from water shortage that affects both rural and urban settings. By doing this historical description, there is an attempt to provide a conceptualization of the role that official discourse has given to saving water technologies and how they are link with political, economical as well ecological pressures in China.

Climate science or climate fiction? The role of fictional elements in physics and in Earth sciences

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There seems to be a boom of model-based doomsday prophecies in recent years—from the publication of *The Limits to Growth* in the 1970s to the most recent climate scenarios. Climate predictions just like predictions on the future course of the economy are based on so-called scenarios. We contend that fictional

elements play a central role even in the hard sciences such as physics and show that on this level of modeling there is no epistemic predicament introduced by scenarios in the Earth sciences. A scenario-based approach does not *per se* render the modeling less reliable as fictional elements play a key role in any scientific modeling without downgrading the sciences to mere fictions.

A new way of thinking in environmental sciences

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The theoretical physics, as it was designed by Newton, was the paradigm of modern science. From the standpoint of it nature is an inert matter, space is a homogenous environment, isotropic and unlimited, defined through the reciprocal exteriority of its parts, and the motion is a result of pushing and pulling, an effect of action and reaction. Environmental sciences challenge this model. The organisms are interrelated parts of biotic communities and the self-organized ecosystems are opened to environment. Ecosystems, as irreversible totalities, are dependent on and independent in the same time. Environmental sciences use a new mode of scientific thought, non-Newtonian, systemic and holistic.

C8. Economics and Social Sciences

Invited Lectures

Scientific Realism and Disciplinary Diversity: Revisionist Remarks

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This is an attack on an important deficiency in standard formulations of scientific realism: their failure to accommodate disciplinary diversity. In the philosophical literature, scientific realism is typically presented as a rather thick and uniform doctrine, either fit or unfit for a given discipline or field or theory, thus giving rise to piecemeal realism. The paper shows why such standard formulations of scientific realism (in terms such as mind-independent existence of unobservables and justified belief in theories), and arguments for and against scientific realism (from success, experimental manipulation, explanatory unification, underdetermination, pessimistic meta-induction, unconceived alternatives) are not appropriate for accommodating disciplinary variety, including the peculiarities of the social sciences. The formulations and arguments should be made sensitive to whether a given discipline is a social science or a natural science; whether it is an experimental or non-experimental science; whether it is a historical or non-historical science; whether it is a mature and successful science or a freshman or unlucky science. What the discipline-sensitive formulations will share is a minimal realism put in terms such as possibility of existence and truth, and constitutive science-independence. Ideas such as scientific success and mind-independent unobservables must go.

The credit crisis as a problem in the sociology of knowledge

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A number of scholars have recently been applying perspectives from the social studies of science and technology to financial markets, an activity sometimes called "social studies of finance". Amongst the questions this work throws up is how market participants evaluate financial instruments, an issue which is (amongst other things) a problem in the sociology of knowledge.

This talk will present a historical sociology of the clusters of evaluation practices surrounding three classes of financial instrument (CDOs, i.e. collateralised debt obligations; ABSs, asset-backed securities; and a fateful concatenation of the two, ABS CDOs) that together account for more than half the losses that triggered the near-collapse of the global banking system in autumn 2008. (These

clusters of evaluation practices are loosely analogous to Knorr Cetina's "epistemic cultures" and to other uses of the term "culture" in social studies of science, but one of the issues to be debated is whether the term "culture" is appropriate here.) I will suggest ways in which those clusters of practices, the interactions between them, and the ways in which they became organisational routines (especially in the rating agencies that awarded credit ratings to CDOs and ABSs) help explain the crisis.

The talk will not assume any prior knowledge of finance, and will explain what CDOs, etc. are. It will be based on a set of 90 predominantly oral-history interviews (29 conducted before the crisis and 61 after it), mainly with the constructors, traders and modellers of instruments of this kind and with employees of the rating agencies.

The evolution and strategic dynamics of individualistic norms

Don Ross

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Defenders of normative individualism—the thesis that the value of the welfare of groups is a strict function of the welfare of individuals who compose them—is often thought to rest on descriptive claims to the effect that agents themselves frame their values in terms of privately judged, idiosyncratic preferences, and generally choose actions in accordance with promotion of such preferences. The association between normative and descriptive utility is often loose, and claims that normative and descriptive individualism are logically independent are not uncommon. I defend a stronger thesis to the effect that most modern people?s strong commitment to normative individualism is best understood on the basis of recognizing the falsehood of descriptive individualism as a genetic or developmental model. It is often wrongly thought that applications of game theory necessarily presuppose descriptive individualism. In the framework of a global game-theoretic structure that instead allows individual self-construction to be strategically endogenous, I model the following related hypotheses. Hominid evolutionary selection did not have to surmount a difficult transition from selfishness to dispositions to cooperate; all hominids may have been descriptive collectivists. A more interesting problem reverses this explanatory priority and asks how humans living in complex market structures that rely on exchange among specialized contributors resist socially inefficient over-coordination that throws away valuable private information. The answer lies in cultural processes by which people are pressured into creating and maintaining boundedly idiosyncratic narrative selves governed by social norms that require distinctiveness limited by comprehensibility and stability. Successful such narratives are valuable achievements, not effortless natural characteristics of persons. This best explains the importance modern people attach to defense of their constructed individualities; descriptive collectivism is the basis for normative individualism.

Contributed Papers

Two conflicting ideas upon the nature and the goals of man's action upon social phenomena

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Hayek and Comte both have a theory about the limits put upon our power to modify the natural course of events but, while Comte thinks that our power grows with the complexity of phenomena and that, social phenomena being the more complex ones, it is where our power is maximal, Hayek thinks that the very complexity of those phenomena is a good reason to abstain from acting. I will study the objections Hayek rises against his adversary in order to see if they really affect Comte's position.

Weber's and Pareto's theories as methodological programs

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The argument of my paper is that sociological theories as those of Weber and Pareto are metatheoretical constructions of a special kind: they are methodological programs devoted to the construction of the scientific method of studying the social world. My main observation is that an important part of them is devoted to the analysis of the natural science method. On this basis, they propose a general model of the scientific method and, starting from this point and under some constraints, adoption or rejection of the natural science method in the social research.

What is social construction?

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An important debate in the philosophy of social sciences is concerned with the following question: Are human categories such as someone's gender, race, and sexual orientation *socially constructed*? There are three main theories in the literature about the nature of these categories: biological realism (according to which those categories are biologically real human kinds), anti-realism (according to which these categories are empty and nothing belongs to them), and social constructionism. In this paper I want to focus on the question of what it means to say that a category is socially constructed.

Guilbaud's reading of Arrow's theorem

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In a paper published two year after Arrow's foundation of modern social choice theory, the French mathematician Georges Théodule Guilbaud has generalized Arrow's famous impossibility result to the "logical problem of aggregation", thereby anticipating both the use of ultrafilters in social choice theory and the recent literature on judgment aggregation. We discuss the significance of Guilbaud's reading of Arrow for a better understanding of the role of mathematical logic and model theory for economic modelling.

On the conceptual clarification of "human environment", "action space" and "quality of life"

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The contribution offers a conceptual clarification of "human environment" (1), "action space" (2) and "quality of life" (3). Which concept presupposes which? We shall argue that (3) presupposes (2) and (2) presupposes (1). At what level do we have to intruduce values ? We shall argue that (3) needs values whereas (1) does not. W. r. t. (2) one may have different views; one which incorporates values, the other which does not. Besides defining (3) there is also a problem how to define lacks or deficits of (3). We shall offer several definitions with a critical discussion.

Which theory of explanation for the social sciences: Unificationist, mechanistic or manipulationist?

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There are three main approaches to scientific explanation in the philosophical literature. The unificationist approach claims that science explains by fitting the particular facts and events within a general theoretical framework. The causal/mechanistic approach claims that science explains by identifying mechanisms understood as entities and activities organized such that they are productive of regular changes from start to termination conditions. According to the manipulationist approach an explanation ought to be such that it can be used to answer a "what-if-things-had-been-different question", i. e. the explanation must enable us to see what sort of difference it would have made for the explanandum if the factors cited in the explanans had been different in various possible ways. The main goal of the paper is to examine whether these three

approaches are compatible or not in the case of the social sciences and if they are, then to explicate how exactly. In developing the argument the paper takes into account the explanatory practices in the social sciences.

Modeling expertise in economics

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In this paper I defend two models for the explanation and prediction of social phenomena, the *Delphi Method* and the *Nominal Group Technique.* While the formulation of the two models is not new, they have so far been applied almost exclusively to management and engineering problems. In this paper I start from a critical discussion of the current status of economic methodology, in particular in relation to economic applications, and argue that the two models well serve the purposes of the economic sciences intended as *applied sciences.*

Two approaches to representative voting

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This paper discusses two approaches to representative voting: the former appeals to iterated aggregation functions, and the latter to representative societies. A number of small collections of functions taken as primitive are introduced. For example, they can consist in the simple majority rule, possibly in conjunction with some unary functions. On the iterated functions approach the problem is to see which aggregation functions can be obtained by repeatedly application of the primitive functions. On the representative functions approach an aggregation function is definable in terms of some collection of primitive functions if applying it to the initial society gives at each profile the same preferences as iteratively applying the primitive functions to some appropriately chosen representative society. The main theorem shows that large classes of aggregation functions can be characterized in this way.

Reconsidering values in assessing the progress of historiography of psychology

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The aim of the paper is discuss the ideas of Avizer Tucker [1] about the determined role of the evidence in the progress of scientific historiography, and contribute to his position by the analysis of the significant part that values play

in the formulation of better hypothesis and in theoretical innovation that allows discovering nested information. I show my theses in the analyses of two examples in the historiography of psychology: the study of psychological human development since the last decades of nineteenth century, and the consideration of ethics and politics values in the production of psychological knowledge.

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Are there laws in the social sciences?

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The paper wishes to provide three arguments for laws in the social sciences. (1) It is possible to test the contraposition of a hedged law, therefore the law itself. Hedged (*Ceteris Paribus*) laws are empirically testable. (2) It is possible to revise the standard model of laws of nature, thus social sciences can have laws on a revised account of laws. (3) There are both explanation and interpretation in the natural sciences and the social sciences, so the two approaches, naturalism and interpretativism, can be compatible.

Decisions without sharp probabilities

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Adam Elga [1] argues that no principle of rationality leads from unsharp probabilities to decisions. He concludes that a perfectly rational agent does not have unsharp probabilities. This paper defends unsharp probabilities against Elga's objections. It shows how unsharp probabilities may ground rational decisions.

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Contributed Symposia

Symposium

Duality within human sciences

Organizer: Antonella Corradini, Nicolò Gaj, Giuseppe Lo Dico

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1. Duality In Between

From a methodological point of view, the monistic perspective claims that every object is in principle investigable by one and only one method: the empirically understood scientific method. This method is expected to guarantee both the validity and the standardization of human knowledge, no matter the specificity of the object of inquiry. Instead, the dualistic or pluralistic perspective attributes more significance to the specificity of the object-domain, whom the specificity of method depends upon: there are different objects in the world, and their allegedly different natures impose different methods. The adoption of a plurality of methods for studying different objects is expected to guarantee the appropriateness of human knowledge.

It can be argued in an analogous way as regards the relationship between natural sciences and human sciences. In this sense, it could be assumed that the methods of natural sciences can be useful for grasping some aspects of the human but that they are insufficient to the aim of establishing its specificity and not crucial for giving a truly interesting account of it. This would mean that we should also develop methods for human sciences which are autonomous from those of natural ones. But, what does "autonomy" mean here? For instance, does it mean that the methods of human sciences can impossibly be integrated into those of the natural ones?

About this issue, all along the years, philosophers of science have conducted many lively debates. Let us remember Robin Collingwood's proposal for a specific kind of explanation in history and Karl Popper's criticism of it based upon his "situational analysis". Or let us mention William Dray's opposition to Carl Hempel's application of the covering-law model to historical analysis. These debates can be viewed as a renewal of interest in the Eighteenth-century contraposition between *Verstehen* and *Erklären, Geisteswissenschaften* and *Naturwissenschaften,* idiographic and nomotetic method.

But precisely when human sciences split, at least methodologically, from natural sciences an essential duality emerges at their core. In general terms, human sciences hold in themselves two concerns: on the one hand, they strongly pursue the aim to preserve the specificity of their objects' features (there are *different* objects in nature), on the other, they are likewise very concerned to adopt inter-subjectively reliable methods of inquiry. These two "horns of the

dilemma" seem to be in conflict. The more we gain in object's details, the more we get far from an inter-subjective method of inquiry; the more we privilege the general validity of method, the more we lose the object's specificity in terms of details. The delicate balance between these two demands constitutes one of the central dialectics of human sciences.

2. Duality Within

The duality intrinsic to human sciences appears to be intuitively easy to grasp but philosophy of science has shown how difficult is to define it rigorously.

In this Symposium we intend to deal with the issue of duality within human sciences by addressing some of the topics that, in our opinion, are still fundamental for the development of present-day human sciences.

A first topic regards the fact that the notion of duality is often formulated in terms of a distinction between *subjective* and *objective.* In this sense, the problematic feature of the scientific disciplines having the man as their object of study is that they must take into consideration a subjective aspect that does not appear in the disciplines having the natural things as their objects. As we said above, although it appears intuitively clear what "subjective aspect" means, it seems difficult to give a rigorous account of it. To be sure, it is undeniable that this aspect is tied to the fact that every human being has her own perspective over the world. This circumstance induces some dualists or pluralists to maintain that the attempt to treat the first person's point of view as if it were a third person's one is a hopeless endeavour *in principle.* This is because it would be impossible to approach human sciences in this way without losing the specificity of the human being and thus of the real object of human sciences' inquiry. However, other pluralists fear the possibly irrational and anti-scientific consequences of their abiding to the first person perspective, so that, although they do not question this approach ontologically, they nevertheless argue against an alleged practical impossibility of reducing a first person's to a third person's perspective. An instructive example is represented by the debate about explanation within human sciences. Some philosophers of human sciences believe that what renders Hempel's model inadequate to the explanation in human sciences is not its nomological character, but, instead, its deterministic nature. As far as philosophers of human sciences can avail themselves of a correct statistical method of explanation, there is no reason to stick to an idiographic model, which could fall under the suspicion of representing the remains of an obsolete metaphysics.

To expand on the just presented topic, present-day human sciences, for example empirical psychology (cognitive, clinical, social, etc.), tend to defend the uniqueness of their object of inquiry, adopting a sort of idiographic perspective. Accordingly, in cognitive psychology the object can be considered the subject's verbal report, while in clinical psychology the subject's test profile. It is commonly said that psychology, whose object is the human being, should deal with *individual* facts. However, this idiographic perspective seems to be valid when dealing with *objects,* but not with *method.* In fact, psychological method has to fulfill the requisite of validity, that is commonly carried out through the notion of objectivity that, in this case, must be intended as intersubjectivity. This means that a fact is assumed to be objective if different individuals reach a high intersubjective agreement on the features of the phenomenon they are dealing

with in specified circumstances, even if this phenomenon is a individual fact, that means, considered in the details that constitutes its individuality.

A third topic, strictly linked to the previous ones, concerns the notions of process and content. Unlike natural sciences, which are mostly interested in natural processes from a syntactic perspective, human sciences seem to be particularly devoted to the analysis of contents: the historian wants to know the reasons why the Marshal took that decision, the psychologist to reconstruct the motives that triggered that emotions, etc. In fact, the concern for contents let the scientist grasp the uniqueness of a phenomenon, that is the complex bunch of features that makes that specific object of inquiry *different* and *peculiar* in comparison with other similar objects. On the other side, the interest in processes remains fundamental in human sciences because their study make the scientist able to answer questions as the following: on the basis of what dynamic (of thought, of behaviour, of emotion) did Marshals take that sort of decision (i.e., to invade a country)? Or, on the basis of what dynamic (of thought, of behaviour, of emotion) do human subjects feel that emotion? The individuation of the process that make a phenomenon intelligible (i.e., explainable in terms of nomological or statistical regularity) gives an account of the *universal* mechanisms underlying that phenomenon. In this sense, it is worth noting that contents are really important when a human discipline has a transformative target (for example, to lead someone from state A to state B, i.e., from a pathological state to symptoms remission), while processes are prominent when the goal of the scientific discipline is descriptive (to describe *how the object is and/or behave* in specific circumstances). On the one hand, in fact, in order to lead someone to state A to state B, it is indispensable to focus on the uniqueness of the object's (subject's) details, because the idiographic consideration of its peculiarity is the only way to find the key to transformation *in that specific case.* On the other, the demand for a inter-subjective account of a phenomenon in terms of mechanisms regarding classes of things (i.e., atoms, human beings, economic systems) require the concern for universal processes that are common to every member of that class of objects. This is the case, for example, of professional clinical psychology and empirical clinical psychology: the former has transformative goals and is mainly interested in contents (what the client/patient says in the session), the latter has descriptive goals and is mainly interested in processes (how the mind/personality/thought works in human beings).

We think that the topics we will consider in the Symposium deserve attention especially in the light of two opposite and contradictory tendencies that can be found in the debate around the status of present-day human sciences: on the one hand, the continuous refinement of proper methodologies for studying the specificity of the human being and, on the other, the various attempts to treat it as if its peculiar features could be reduced to the physical or material ones.

Contributions

- **The epistemological status of empathy in the human sciences**

Antonella Corradini

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- **The place of verbal reports in cognitive psychology**
Giuseppe Lo Dico
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- **The double soul of clinical psychology: a two-faced methodology between monism and dualism**
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Symposium

Business ethics and analytic philosophy

Organizer: Christoph Lütge
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During the past decade, business ethics has developed mainly into two directions: First, those streams oriented towards application of ethics have differentiated into numerous substreams. Second, however, the discussion on theoretical foundations of business ethics has grown substantially. Within this discussion, analytic philosophy is being taken into account more and more.

This symposium focuses on interconnections between recent developments in analytic philosophy and business ethics. In particular, we will discuss the following questions:

- Which systematic role can be assigned to 'good reasons' in business ethics? How are the contexts of justification and of implementation of norms coupled*? (How) can corporations and other organisations supplement or even substitute incentives and sanctions by good reasons, arguments or appeals?
- Which implications does the analytic discussion of the concept of responsibility have for business ethics? The CSR discussion can certainly profit from this foundational debate, regarding questions such as: In what way do corporations have a collective responsibility in big proejcts? Should corporations take over tasks of political bodies?
- Can contractarianism develop into a normative model of justification for business ethics? How does it perform compared to other models like utilitarianism or discourse ethics?
- (How) can rather practically oriented conceptions like the stakeholder approach to business ethics be improved or criticised by philosophical reflexion?

Contributions

- **Collective responsibility and its ethical implications from an analytic perspective**

Christian List
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- **Business ethics and virtue ethics—Great love or strange bedfellows?**

Lisa Herzog
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- **Reasons and incentives in the health care system**

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- **Exploring emotions' cognitive value through the analysis of moral language. Some implications for human resource management**

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- **Are good reasons and incentives compatible within a contractarian business ethics?**

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Decision theory in economics: Between logic and psychology

Organizer: Samuel Ferey
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Nowadays, experimental economics, "psychology and economics" and bounded rationality are some of the main renewals of economic theory. They challenge neoclassical economics on its very grounds (decision theory) by providing an alternative view of action. Pervasive criticisms against the traditional model of consumer's choice were more and more grounded on psychological assumptions and experimental evidences that subjects do not obey the rules of rational behaviour. The stakes of the controversies about decision theory are deep: positive as well as normative aspects of economics are concerned by these new challenges. The aim of the symposium is to deal with these issues both from a logical, epistemological and historical point of view.

We would like mainly to address the issue of the epistemic nature and the significance of axiomatics for decision theory and consequently the role of psychological hypothesis and its meaning. The first and fourth papers discusses this topic by studying the axiomatics of decision theory in the 1950? notably respective to psychology, the third paper deals with the importance of representation theorem and its function within classical decision theory, the third paper analyzes the stakes of behavioural economics from the point of view of normative economics.

Contributions

- **Between axiomatics and psychology: Probabilizing the consumer in the 1950's**

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- **On representation theorems, what does ax add?**

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- **Normative stakes for behavioral economics**

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- Savage vs. Wald on "complete ignorance": was Bayesian decision theory the only available alternative for postwar economics?

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D. Methodological and Philosophical Issues in Technology

Invited Lectures

Applications of philosophy: The bottom half

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Oxymoronic Title? Since trading the philosophy for mathematics at the Delft University of Technology in 1987, that is exactly what I have been doing. A dilated epiphany led from counting infinite cardinals, through foundations of probability and quantum mechanics, to risk and uncertainty analysis, and to my present involvement with risk and uncertainty in climate change. This talk will hit some of the highlights in this process, and focus on some currently active issues. We are interested in *pistis* (belief) and *eikasia* (conjecture), the bottom half of Plato's divided line, which may be captured in a single word—uncertainty. The bottom half is where we live and where our future will be decided. There have been many giant steps in moving from the gods of fortune to science based uncertainty quantification, but the most important, in my view, was that of Frank P. Ramsey. Ramsey showed that ratios of differences of values could be assigned absolute numerical values, and that partial belief could be quantified as subjective probability. The theory of rational decision attained its classic form in the work of L. J. Savage. As in any formalization project, there are aspects of a raw intuitive notion that escape formalization. Sometimes it's important to capture these in subsequent refinements; more often than not they just shrivel away. There remains a discussion of aspects of uncertainty that are not captured in Ramsey's program, but these discussions are marginal to the major challenges. If uncertainty is—effectively—subjective probability, the question for society facing choices under uncertainty is, whose uncertainty? Expert Judgment is denotes the activity of quantifying experts' uncertainty and rendering it for social decision making. Who is an expert? When is a problem an expert judgment problem? Can we quantify expertise? How do we combine divergent opinions? Are there rational answers to any of these questions? A great deal of work over the last several decades proceeds from positive answers to all these questions, and some of that work will be reviewed in the talk. Once we realize that science based quantification of uncertainty is possible, a whole raft of issues arise. How do represent high dimensional distributions with dependence, how to we sample and analyze such structures? Lacking sufficient data, how do we fit models to subjective uncertainty distributions? The major issue for the coming years is uncertainty quantification with regard to climate change. Carbon emissions are not only disrupting the earth's climate, they are also disrupting the scientific community, exposing or creating deep fault lines between different fields. Credible opinions range from 'no worry' to 'its now or never' to 'its already too late', and finally 'we're too dumb to survive ourselves'. Its *eikasia* writ large. We need more eyes on this problem.

Knowledge and the design and making of technical artefacts

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What, if anything, is special about technological knowledge, that is, knowledge about technical artefacts? In order to deal with this question I will start by comparing a description of a thing as a natural object (from a physical stance) with a description of the 'same' thing as a technical artefact (from a design stance). This will show that knowledge of functions plays a crucial role in our knowledge of technical artefacts. I will argue, however, that knowledge of functions is not a specific feature of technological knowledge that makes this kind of knowledge different from scientific knowledge. Technical artefacts, including their functional features, may be studied scientifically in the same way as biological organs with their functional features. There is no reason to characterize the resulting knowledge as scientific in one case and as technological in the other. Things appear to become different when we turn from *understanding* technical artefacts to the *design, making, using and optimizing* of technical artefacts. In order to explore what kinds of knowledge are involved in these activities, I will first clarify what it means to make or create a technical artefact and in what sense that the creation of technical artefacts is different from the creation of natural things (Hacking's creation of phenomena). Here, the crucial difference concerns the realization/making of functional features that are constitutive for being a technical artefact of a particular kind. These functional features have to be translated in specifications that the objects to be designed and made have to satisfy. Consequently, knowledge of how to translate functions into lists of specifications, design knowledge and knowledge of how to make things that satisfy these specifications become of primary importance. In the last part of my talk I will explore to what extent these forms of knowledge challenge the traditional, intellectually biased, interpretation of knowledge as forms of beliefs that p.

The target of testing: Models, adequacy and scientific knowledge

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Discussions of model evaluation often suggest that scientific models themselves, understood as complex hypotheses, are tested or confirmed in the course of model evaluation. I argue that usually what we can sensibly aim to test or confirm are not scientific models, but their adequacy for particular purposes. I explain why testing a model's adequacy-for-purpose can be quite difficult, involving challenges beyond those involved in testing whether a model embodies a true or empirically adequate hypothesis about the workings of a target system, and I illustrate with some examples. Finally, I offer some exploratory remarks on how the notion of adequacy-for-purpose might figure in our understanding of science more generally.

Contributed Papers

Production of intermediary objects in a collaborative network: Examples of impact on tools and methods in engineering innovation

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The cooperation work between the groups of researchers belonging to different disciplines, shows the pre-eminent role that the mobilisation and clarification of boundary objects [1] (boundary objects, [1]), qualified as "intermediary objects" (I.O.) by specialist literature in sociology of sciences and technological innovation ([4], [2], [3]). Solicited in the case of scientific network cooperation, these objects precisely enable different social universes, having their own disciplinary identity, their own *habitus,* and their own values, to cooperate and engage in common scientific practices. Their purpose is to obtain driving effects and to build common knowledge production regimes in the framework of innovative processes.

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1. Susan Leigh Star used this term. See notably [1, 387–420].

An inquiry into the character of material artifacts

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In this paper I try to develop an account of the ontological status and character of material artifacts (in order to draw wider philosophical attention to them) by means of critical analyses of Popper's theory of three worlds and the currently popular Dual-Nature theory of artifacts. Finding clear traits of human interference into the very physico-chemical composition of material artifacts I intend to make them inmates solely of World 3. On the other hand, instead of portraying artifacts as hybrid 'objects' (exhibiting a duality between the physical and functional) I , because of certain reasons, prefer to describe them as multiply utilizable, result-producing 'systems' (existing at the interface of 'inner' and 'outer' environments). This paper concludes with an observation that artifacts are more important and interesting than natural objects for philosophical exploration.

Technical malfunction in terms of states and events

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In this paper I analyze the notion of malfunction in technical artifacts. I introduce two cases of malfunction. In the first case an artifact fails to perform and, after that, it is said to be malfunctioning. In the second case an artifact is said to be malfunctioning even though it has not failed to perform yet. I analyze the difference between these two cases by means of the distinction between states and events. I present two approaches. The first is based on the distinction between types and tokens; the second is based on the notion of reliability.

Science, technology and society. An attempt to think their link through Maurice Blondel's (1861-1949) philosophy of action

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Blondel's philosophy of action makes it possible 1) to think the relation between science and technology without disparaging one of both to improve the image of the other, and 2) to link them to the life of our democratic society. He studied the cognitive process of reflection and showed that it is because of the very nature of consciousness that science and technology collaborate. They are conceived for action whose development was specified by Blondel. From the beginning to the end, scientific activity is embedded in society. That means that science and technologies are necessarily a political stake.

Technoscience: Illuminating new blue skies

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Science is traditionally concerned with "what is" and technology with "what is to be". Analysing the unified field of technoscience risks alienating fundamental scientific research. I argue that the shift from science and technology as two realms to technoscience, a single field of intertwined representation and intervention, does not compromise the aims of "blue-skies" research. Despite the image of technoscience as specifically purpose-driven, I believe it must address both concerns and therefore is a suitable framework within which to analyse fundamental scientific research. I suggest that the Gravity Probe-B is an example that shows just how this can work.

Formal ontologies and semantic technologies: A "dual process" proposal for concept representation

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The problem of concept representation is relevant for many subfields of cognitive research. In particular, in recent years, it received great attention within knowledge representation, because of its relevance for knowledge engineering and for ontology-based technologies. In this paper we propose an analysis of this state of affairs and sketch some proposal for concept representation in formal ontologies, which takes into account suggestions coming from psychological research. In our opinion a mature methodology to approach knowledge representation and knowledge engineering should take advantage also from the empirical results of cognitive psychology.

Artefacts and family resemblance

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I discuss in this paper the conceptual perspectives for a conception of artefacts based on Wittgenstein's idea of family resemblance. The history of philosophical analysis of the notion of artefact shows a number of various and sometimes conflicting intuitions on its content. I suggest that instead of arguing in favour of one of these intuitions we should consider the possibility that artefacts will evade the standard definitional methods because they compose "a complicated network of similarities overlapping and criss-crossing". I substantiate this suggestion with an outline of a family-resemblance conception focused on relations interlinking the phases of artefact life-cycle.

What is so special with technological science?

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Based on a typology of technological knowledge, a distinction is made between two major types of scientific endeavours applied to technology: natural science applied to technology and technological science *per se*, that has technology as its study object. The criteria of scientific quality for these two types of science are discussed, and it is concluded that the quality criteria for natural and technological science are only partly overlapping. Additional characteristics of technological science are identified, related to their uses of idealizations, models and functional terms and their approaches to normative issues.

How to recycle Asimov's laws in roboethics: An intermediate suggestion

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[1] maintain that robots cannot be genuinely autonomous agents. Even if they were right, robots can have a certain degree of autonomy on the phenomenological level. Roboethics should discuss the basic principles for employing and regulating this phenomenological autonomy of roboticised mind. Asimov's Three Laws can provide a reasonable starting point for roboethics, but they should be reformulated in correct narrative perspective in which not robots but human agents such as designer, manufacturer, administrator, and user of robotic systems appear as acting subject in the law sentences, in order to be the principles of reality-relevant roboethics.

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Nominalism of things and nominalism of events, from Turing Machines to functional programming

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The conflict between imperative programming, influenced by von Neumann machines, and functional programming, theorized by John Backus, is the *analogon* in computer science of the conceptual and stylistic discrepancy between two logical systems: Turing machines and lambda calculus. On one side, the world

is made up of states and transitions; on the other side, it consists in a world of functions.

We will be looking for the roots of this conflict in Jules Vuillemin's distinction between the nominalism of things and the nominalism of events:

Logic	Computer Science	Philosophical Systems
Turing machines	Imperative programming (von Neumann machines)	Nominalism of things
Lambda calculus	Functional programming	Nominalism of events

Geometric configuration in nature and in design: Is there a connection?

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There are some geometrical configurations that recur in both nature and in the design of engineered artefacts. It is natural to ask whether there might be a common principle that accounts for the same geometrical configuration arising in many different contexts: living creatures in nature (trees, lungs), nonliving natural structures (river basins and deltas), and in engineered artefacts (transportation and distribution systems, counterflow heat exchangers). We discuss a principle that has already been discussed in the fields of physics, engineering, and biology that accounts for this, but of which philosophers of science have yet to take account.

Models, commentaries, and theories

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We address the question of how to distinguish models which have only a pragmatic value from models which are equivalent to theories. We argue that the crucial point is not the realistic compromises involved in the construction and use of the models but is in the "commentaries" connecting the models with a wider context of theories. We take as a paradigmatic example the case of the construction and use of computer simulation models through System Dynamics. The construction and use of those models typically depends on very strong realistic compromises, but many times they are taken as having only a pragmatic value.

Comparing methodologies of classical, natural, field and computer experiments deployed in climate change studies

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In the philosophical literature on scientific experimentation, there is significant diversity in meaning with respect to the ways in which the term 'experiment' is used. In this paper, I hope to clarify the senses in which various scientific practices are experimental by detailing a vocabulary for a philosophical analysis of the subject, according to which different types of experimentation can be classified. I then concentrate more closely on the notion of 'computer experiments', outlining three ways in which simulation studies are sometimes regarded as experimental. The distinctions I present comprise a framework according to which one may further study the epistemology of experimentation.

Contributed Symposia

Design as a challenge for the philosophy of science

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The notion of engineering design is one of the key concepts underlying the epistemological differences between the natural and engineering sciences. It demarcates the action-oriented paradigm of the engineers from the more contemplative perspective of the natural scientists. This demarcation brings out the functional, teleological and normative dimensions of engineering, in contrast to the descriptive dimension of the natural sciences. In the latter, moreover, deduction and, more controversially, induction are the prevailing modes of reasoning, whereas the former cannot do without means-end or functional reasoning. Finally, the assessment of the outcome of a design process differs considerably from that of the 'products' of scientists. Artifacts satisfy a list of design specifications to a larger or lesser extent, whereas scientific statements are true or false, or to some level of accuracy empirically adequate or inadéquate—judgments that are hardly applicable to artifacts in a literal sense.

The designing and manufacturing of material devices or provision of services involving the manipulation of technical objects is one of the core professional activities of engineering. Until recently, however, philosophers (of science) have shown only little interest in the conceptual problems surrounding engineering design, notwithstanding the philosophical challenges of the epistemic intricacies involved. The philosophy of designing and making technical artifacts is still a marginal field within mainstream philosophy. To be sure, philosophers occasionally discuss (engineering) design or design in general, for example within the context of the argument from design, but in this sense the notion is only obliquely related to engineering practice. A systematic philosophical analysis of (engineering) design is lacking.

The disregard for matters of practical construction, subordinating it to abstract thinking about timeless truths, can be traced back to the ancient Greek philosophers, such as Plato, Aristotle and Euclid. One of the reasons for the philosophers' neglect of the engineering sciences may be the equation of engineering with just applied science. Although scientific knowledge plays an increasingly dominant role in modern design practice, it would be misleading to interpret engineering design as simply the application of scientific knowledge—or even of knowledge produced by the engineering sciences. According to [6, ch. 7], the anatomy of engineering design knowledge includes at least six different categories of knowledge, some of which do not derive from scientific knowledge at all (such as the 'know how' acquired on the shop-floor). All these various kinds of knowledge are important for turning a functional description of the object to be designed into a structural one. Another reason for the philosophers' neglect of design may be rooted in the thought that knowledge of technical possibilities is more contingent than knowledge of physical possibilities. At the moment we

leave open the question whether these considerations suffice to explain the neglect of engineering by philosophers of science. The purpose of this symposium is to bring philosophical questions regarding design to the attention of a broader philosophical public. In our age and time technology and science are so intertwined and technological progress depends to such a large extent on theoretical developments that philosophy of science cannot ignore fundamental epistemological questions in engineering and technology. And the notion of design is in dire need of further philosophical explication.

A first conceptual clarification regarding design concerns the distinction between design as a process or design as the product of a design process. We typically take the *outcome* of an engineering design process to be some material object or process and its descriptions. These objects are technical artifacts and are different from natural objects in that they involve (human) intentions, where different views of the character of this 'involvement' are possible. Artificial objects are often characterized as the material realization, the 'embodiment', of human designs (more generally, of human ideas). Exactly what a design in this sense is, is not so easy to spell out. In the 'thin' sense a design may be taken to be a blue-print for production: a description of all the physical (chemical) properties of a technical artifact that are relevant for the actual making of a token of the artifact type defined by the design. The notion of a blueprint, however, does not capture the full notion of design. The notion of design, has strong teleological connotations, since a designed object has a specific property of '*for*-ness': the designed object has been made in order to do something, [3]. The 'thick' notion of design becomes a description of a 'teleological arrangement' of physical parts that together realize a function. This notion still covers a wide variety of objects ranging from mass produced computers to unique oil platforms, from telephones to high rise buildings, from components to complex systems, from micro-organism to biotech labs, and so forth. Some of the products of engineering design arguably cannot be seen as objects at all, for example software.

Taken as a process, the design of technical artifacts is often characterized as primarily a synthetic one, in contrast to the analytic activity of scientific research. The distinction between analytic and synthetic forms of reasoning comes close to distinction between reasoning from effects to causes and the opposite one of reasoning from causes to effects. The *prima facie* appealing picture of designing as a synthetic activity, however, stands in need of further explication if it is to be of use in understanding the nature of engineering design. From a philosophical point of view little is known about the distinctive synthetic features of engineering design. After all, scientists also have to be skillful designers of experiments and the equipment needed to carry them out, and they can perhaps also be said to design the explanations and theories of science. Thus the characterization of engineering design as a synthetic activity may not, as such, help much in clarifying what kind of activity it is. It may well be that just as the distinction between analytic and synthetic statements has come under attack, [5], the distinction between analytic and synthetic methods will have to be reconsidered (see, for instance, [1]).

Another traditional topic from philosophy to which the engineering design is related is the question of (practical) rationality, which in this context comes down to the question as to which criteria measure the success of the outcome of an engineering design process. From a strictly engineering point of view,

the simplest success criterion is just meeting the list of specifications. But this assumes that a fixed list of precise specifications guides the design process from the beginning till the end, which is seldom the case. Because of problems encountered during the design process, requirements and specifications often have to be adjusted. Furthermore, decisions about what performance criteria to use and the development of methods for measuring these performance criteria are often an integral part of the design process. Additionally, design requirements are often formulated in such a way that adequate design cannot be a question of just meeting them, for instance 'as light as possible', and various participants may evaluate the outcome in different ways. In spite of these difficulties, design methodologists claim that the implementation of systematic approaches to design improves the design process (see, for instance, [4, 499–501]). It is unclear, however, how broad the spectrum of methods is from which contributions to the systematization of design methodology can be expected, and whether the notion of instrumental rationality is exhaustive for an adequately analysis of the rationality in engineering design or perhaps too narrow.

The employment of computational methods due to the growing complexity of the objects to be designed have increased the need for more formal, unambiguous representations of designs. Such representations are important in developing engineering data management systems for computer aided design (CAD). Especially the formal representation of functions has proved problematic, [2]. Much work is being done in developing taxonomies of functional primitives (a field sometimes referred to as 'functional modelling'), functional representation and functional reasoning in AI-quarters with the aim of supporting engineers in solving design problems and accurately representing design solutions.

The above considerations illustrate that a philosophical explication of engineering design will tie this notion to traditional problems in philosophy. Given the broad array of topics involved, and given that philosophical reflection on (engineering) design is still in its earliest phase, this symposium does not present a neat, systematized and exhaustive discussion of the most important topics in the philosophy of design and how they have been dealt with by philosophers up till the present. It is much more an attempt to explore the largely uncharted domain of the philosophy of (engineering) design by discussing the following topic.

In the first presentation, Richard Buchanan will address the relation between science and engineering by concentrating on a general inventory and assessment of the differences and similarities between scientific research on the one hand and engineering design on the other. The second contribution will take the wide variety of possible subjects of design as its subject. In an effort to streamline the ambiguities and many ways engineers use the notion of a system, Zwart and Franssen introduce and elaborate the notion of instrumental system, which at least has the advantage of permitting a formal treatment of the concept. The last two papers take examine two distinctive features of engineering design, viz. its functional and normative aspects. In the third paper, Riichiro Mizoguchi discusses from the perspective of functional modelling the many ways in which engineers use the notion of a function and contrasts this concept with the notion of biological function. The last contribution will focus on the normative aspects of engineering design and the problems of evaluative trade-offs that inherently beset the design and development artifacts. To unravel the various evaluative deadlocks Ibo van de Poel introduces and discusses different notions of incommensurability.

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Contributions

- Tba

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- Instrumental system as a sui generis kind of system central to engineering design

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- Functional ontology viewed from engineering point of view

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- Trade-offs and commensurability in engineering design

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Artefact Functions

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Philosophers have traditionally been concerned with analysing functional discourse insofar as it concerns biological items: how can this discourse, which seems tailor-made for the purposeful design and use of artefacts, be reconciled with a naturalistic, designer-less view of nature? Countless answers to this question have been proposed and discussed in the general philosophy of science and the philosophy of biology (see, for instance, the work collected in [3], [1]), to the extent that some now regard it as an exhausted field, memorably characterised as featuring no more than "the dull thud of conflicting intuitions" (Bigelow and Pargetter 1987).

Until recently, technical artefacts played only a subsidiary role in this debate. They were mentioned as the source domain of functional discourse, not presenting problems in its own, but showing how problematic functional claims in other domains might be (e.g., [11] and [16]). They were taken as a standard and as such unconditionally applied to those other domains [4, 5]. Or they were included in general analyses of functions, which were extensively developed and discussed for biological items, and applied to artefacts in passing. Most etiological theories of function (e.g., [12, 13] and Neander, [14, 15]) provide a case in point.

In the last decade, attention for functional claims about technical artefacts has increased markedly. Preston [17] brought technology into focus, modifying etiological theories to fit both biology and technology. Subsequently, Vermaas and Houkes, [18] argued that etiological theories fail to account for functional claims about technical artefacts. Nowadays several alternative definitions of artefact functions are on offer in the literature, [6], [7], [9]. This has effectively set a second stage on which theories of functions can prove themselves (e.g., [8]), for instance in accounting for claims of malfunctioning and claim regarding proper and accidental use.

Besides offering accounts of such claims, philosophical theories of artefact functions contribute to debates regarding artefact kinds (e.g., [10]). Many have maintained that functions are the real or nominal essences of artefacts (e.g., [19]; [2]). This function essentialism puts a premium on an accurate analysis of what these essences might be; or, turning the table, it broadens the philosophical impact of any analysis of artefact functions that suggests that functions cannot be essential.

Artefact functions therefore present nowadays a philosophical topic in their own right, and in this symposium we bring together research that advances this topic. The symposium is meant to give participants a perspective on current research, in three parts. Each of these addresses different types of questions that can be raised with the established theories of function.

Wybo Houkes' paper "Technical functions and the dynamics of innovation" presents evidence that functional characterisations of artefacts or, more broadly, of technology, are prominent in some branches of science and engineering. Philosophical analyses of artefact functions can therefore do more work within science and technology than proving their mettle in reconstructing everyday language

or in supporting or undermining function essentialism in metaphysics. Houkes' paper is about evolutionary economics, raising the question which work philosophical analyses of function can do outside of philosophy.

Françoise Longy's "Theories of artefact function facing the challenge of wrong function attributions" gives an insight in current work on the theories of function themselves. It analyses the ability of two such theories to handle problematic function attributions due to "crazy" inventors, raising the general question which issues are still outstanding in the ongoing debate about the adequacy of theories of artefact functions.

The paper "Technical functions and engineering-design methodology" by Pieter Vermaas questions the very project of giving a philosophical analysis of artefact function. It argues that the presupposition held in that analysis, being that artefact function is a well-defined and precise concept, does not reflect how this term is used in engineering. This third paper raises the general question to what extent philosophical work on artefact functions is appropriate to every domain in which functional discourse is used.

The symposium will as such enable reflection on the philosophy of artifact function, on its use, on its adequacy, and on its presuppositions.

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Contributions

- **Technical functions and the dynamics of innovation**

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- **Theories of artefact function facing the challenge of wrong function attributions**

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- **Technical functions and engineering-design methodology**

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Affiliated Program

Symposium "Conférences Pierre Duhem"

The philosophy of articial intelligence

Chairs: Max Kistler, Cyrille Imbert

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Contributions

- How to discover what the brain does

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- Evolution of mind as a feat of computer systems engineering: Lessons from decades of development of self-monitoring virtual machinery

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Beth Fundation Symposium

LOGIC, KNOWLEDGE AND AGENCY

Chairs: Sonja SMETS

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Contributions

- T.B.A.

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- A DYNAMIC ANALYSIS OF INTERACTIVE RATIONALITY

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- WHEN EPISTEMIC LOGIC MEETS SKEPTICISM

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- REASONING WITH INCOMPLETE INFORMATION IN DEL

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Association for the Philosophy of Mathematical Practice Symposium

Philosophy of mathematical practice

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Main lectures

- Mathematical practice from an historical point of view

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- Richness and cognitive value in the aesthetics of mathematics

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Shorter Communications

- Explanation and argument in mathematical practice

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- Describing mathematical practices and the interpretation of texts. Some views based on Chinese mathematical sources

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- The Recreational and the mathematical practice of Récréations Mathématiques (1624)

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- **ARGUMENTATIVE NETWORKS: INTEGRATING PHILOSOPHICAL APPROACHES TO ARGUMENTATION, AIMODELS, AND MATHEMATICAL PRACTICE**

Bart VAN KERKHOVE

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- **ROOTING (MADDIAN) REALISM IN MATHEMATICAL PRACTICE**

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- **SYMBOLIC KNOWLEDGE IN PEIRCE'S EXISTENTIAL GRAPHS**

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- **KRONECKER'S CONSTRUCTS**

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- **THE INTERFACE BETWEEN PHILOSOPHY OF MATHEMATICS AND THE EMPIRICAL STUDY OF THE MATHEMATICAL PROFESSION**

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- **PROOF AND UNDERSTANDING IN MATHEMATICAL PRACTICE**

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- **REAL RELATIONS BETWEEN POSSIBLE OBJECTS**

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- **THE APPLICABILITY OF MATHEMATICS**

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LOCI Symposium

Rebuilding logic and rethinking language in interaction terms

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Contributions

- **Tutorial on ludics**

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- **Computational tools derived from Ludics and their use in web technologies**

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- **Towards theoretical pragmatics based on ludics**

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- **Interactional quantification (Quantification in ordinary language: from a critique of set-theoretic approaches to a proof-theoretic proposal)**

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- **Proto-logical foundations of logic**

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Affiliated symposium of the EuroCoRes programme LogICCC

LOGICAL MODELLING: THE INTERFACE BETWEEN THE FORMAL AND THE EMPIRICAL

Chairs: Eva HOOGLAND, Benedikt LÖWE

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Contributions

- **DIFOS 2: FINE-GRAINED MATHEMATICAL JUSTIFICATIONS**

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- **LINT: AN EMPIRICAL PERSPECTIVE ON LOGICAL CONSTANTS**

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- **LOMOREVI 2: MULTIPLICATIVE QUANTIFIERS IN FIRST-ORDER FUZZY LOGICS**

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- **A NEW APPROACH TO FUZZY LOGICS WITH TRUTH STRESSERS AND DEPRESSERS**

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- **TOWARDS AN EXPERIMENTAL PHILOSOPHY OF UNCERTAIN REASONING**

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- LcpR 1: A simulation based analysis of logico-probabilistic reasoning systems

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- Faithfulness in formal modelling

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- Be tolerant about vagueness, because it is unavoidable!

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Société de Philosophie des Sciences Symposium

Climate science and climate change: Epistemological and methodological issues

Chairs: Anouk BARBEROUSSE, Cyrille IMBERT, Stéphanie RUPHY

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Contributions

- **Review of risk and uncertainty concepts for climate change assessments including human dimensions**

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- **Decision-making with climate models?**

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- **Conveying uncertainty: Ownership, justification and robustness**

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- **At risk of the very long run: How economics can inform climate decision**

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- **Insight or numbers? Distinguishing climate science from climate modelling**

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LORIA Symposium

Analysing programs: Logic at rescue

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Contributions

- **Logic and digital circuits: From theory to practice**
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- **From universality to interoperability**
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- **Coinduction, equilibrium and rationality of escalation**
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- **What's the craze with model checking?!**
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- **From philosophical to industrial logics**
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- **Logic without formulas: Automata as a computational notation**
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International Institute of Foundational, Interdisciplinary and Historical Problems of Science Symposium

LOGICAL AND PHILOSOPHICAL FOUNDATIONS OF SCIENCE AND TECHNOLOGY—HISTORICAL DEVELOPMENT, CONTEMPORARY INVESTIGATIONS, AND PERSPECTIVES,

Chair: Boris CHENDOV

Bulgarian Academy of Sciences (Bulgaria)

Bchendov@bas.bg, bchendov@yahoo.com

Contributions

- **INTERDISCIPLINARY APPROACH TO LOGICAL AND PHILOSOPHICAL FOUNDATIONS OF SCIENCE**

  Boris CHENDOV

  Bulgarian Academy of Sciences (Bulgaria)

  Bchendov@bas.bg, bchendov@yahoo.com

- **HARRE ON MODELING**

  John SHOSKY

  American University in Washington (USA)

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- **SEQUENTIAL LOGIC IN FOUNDATIONS OF SCIENCE: HISTORY AND CURRENT RESEARCH**

  Alexander KUZICHEV

  Moscow State University (Russia)

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- **QUANTUM DECOHERENCE AND THE PHYSICAL MEANING OF THE SUPERPOSITION PRINCIPLE**

  Michel PATY

  SPHERE, CNRS, Université PARIS 7 (France)

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- **The problems of knowledge as the core of scientific inquiry**

Peeter Müürsepp
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- **On the limits of the exact science**

Leo Näpinen
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- **The philosophical treatment of space and time in Chinese science and technology**

Wei Wang
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- **On the revolution in the mathematics in the end of XIX–the beginnings of XXth century**

Galina A. Zverkina
Moscow State University of Railway Engineering (MIIT) (Russia)
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- **The "dialogue", a rhetorical tool in social context: Galileo and Einstein**

Galina Weinstein
(Israel)
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Symposium

Methodological problems of technoscience

Chairs: Vitaly Gorokhov, Armin Grunwald

Institute for Philosophy, Russian Academy of Sciences (Russia), Institute for Technology Assessment and Systems Analysis, Karlsruhe Institute of Technology (Germany)

vitaly.gorokhov@mail.ru, armin.grunwald@kit.edu

Contributions

- **Emerging technosciences and changing relations between technology, science and nature: the case of synthetic biology**

  Armin Grunwald

  Institute for Technology Assessment and Systems Analysis, Karlsruhe Institute of Technology (Germany)

  armin.grunwald@kit.edu

- **Nanotechnoscience as combination of the natural and engineering sciences (Methodological specialty of the theoretical investigation in the nanotechnology)**

  Vitaly Gorokhov

  Institute of Philosophy of the Russian Academy of Sciences (Russia) – Institute of Technology Assessment of the Karlsruhe Institute of Technology (Germany)

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- **Postulated theoretical entities vs. technological real entities: Remarks on Haking and Giere**

  Fabrice Pataut

  Institut d'Histoire et de Philosophie des Sciences et des Techniques, CNRS, Université Paris 1, École Normale Supérieure (France)

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- **On enlightened intuition**

  Max Urchs

  EBS University (Germany)

  max.urchs@ebs.edu

- **The problem of the justification of beliefs in science vs. actions in technoscience**

Maarten Franssen
Delft University of Technology (The Netherlands)
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N.O.T. Symposium

The logic of opposition

Chair: Fabien Schang
Archives H. Poincaré (CNRS), Nancy University (France)
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Contributions

- **Import, or not import? How to handle negation inside the square**

Saloua Chatti
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- **Structures of oppositions in public announcement logic**

Lorenz Demey
University of Leuven (Belgium)
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- **Oppositions between quantifiers, colours, and numerals**

Dany Jaspers
CRISSP – Hogeschool-Universiteit Brussel (Belgium)
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- **Negative acts and negative characters: declining, preventing, refusing, and failing**

John T. Kearns
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- MULTI-DIMENSIONAL OPPOSITION

Ingolf MAX
Institut für Wissenschaftstheorie, University of Leipzig (Germany)
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- THE OPPOSITIONAL GEOMETRY OF SET-THEORETICAL INCLUSION

Alessio MORETTI
(Switzerland)
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- AN ARITHMETIZATION OF THE LOGIC OF OPPOSITIONS

Fabien SCHANG
Archives H. Poincaré (CNRS), Nancy University (France)
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- ON THE HYBRID NATURE OF THE ARISTOTELIAN SQUARE AND THE SESMAT-BLANCHÉ HEXAGON

Hans SMESSAERT
University of Leuven (Belgium)
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International Academy for Philosophy of Science Symposium

Science and rationality

Chair: Evandro Agazzi

Académie Internationale de Philosophie des Sciences, Bruxelles (Belgium) – University of Genoa (Italy) – Autonomous Metropolitan University of Mexico (Mexico)

evandro.agazzi@gmail.com

Contributions

- **The mathematical paradigm of rationality**

  Christian Thiel

  University of Erlangen (Germany)

  Christian.Thiel@sophie.phil.uni-erlangen.de

- **The analytic vs synthetic approach in the experimental sciences**

  Anne Fagot-Largeault

  Collège de France (France)

  anne.fagot-largeault@college-de-france.fr

- **Realism and rationality in the natural and in the social sciences**

  Jesús Zamora-Bonilla

  National University for Education at Distance, Madrid (Spain)

  jpzb@fsof.uned.es

- **Non-scientific forms of rationality**

  Evandro Agazzi

  Académie Internationale de Philosophie des Sciences, Bruxelles (Belgium) – University of Genoa (Italy) – Autonomous Metropolitan University of Mexico (Mexico)

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Ontology Symposium

Ontology between philosophy and computer science

Chairs: Heinrich HERRE, Roberto POLI

University of Leipzig (Germany), University of Trento (Italy)
herre@informatik.uni-leipzig.de, roberto.poli.tn@gmail.com

Contributions

- **Wholes in structural mereology**

  Thomas MORMANN
  University of the Basque Country (Spain)
  thomasarnold.mormann@ehu.es

- **Freely faceted classification for knowledge organization**

  Claudio GNOLI
  University of Pavia (Italy)
  gnoli@aib.it

- **Ontology and Terminology**

  Frédéric NEF
  École des Hautes Études en Sciences Sociales, Paris (France)
  frederic.nef@ehess.fr

- **Ontological categories**

  Roberto POLI
  University of Trento (Italy)
  roberto.poli.tn@gmail.com

- **Logical pluralism in ontology design**

  Oliver KUTZ
  University of Bremen (Germany)
  okutz@informatik.uni-bremen.de

- **An ontological foundation for the reconstruction of theories**

  Heinrich HERRE
  University of Leipzig (Germany)
  herre@informatik.uni-leipzig.de

- **Some assumptions of systems theory**
  David Hirst
  (France)
  hirstdc@gmail.com

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Symposium

Intuitionistic modal logics and applications workshop (IMLA'11)

Chairs: Natasha Alechina, Valeria de Paiva

School of Computer Science, University of Nottingham (UK), University of Birmingham (UK)

nza@cs.nott.ac.uk, valeria.depaiva@gmail.com

Invited speakers

- **A simply typed calculus for semantic knowledge bases: Curry-Howard for constructive modal logics**
  Michael Mendler
  Faculty of Information Systems and Applied Computer Sciences (WIAI), The Otto-Friedrich-University of Bamberg (Germany)
  michael.mendler@uni-bamberg.de

- **Keeping track of reasons for beliefs**
  Brian Logan
  School of Computer Science, University of Nottingham (UK)
  bsl@cs.nott.ac.uk

- **Nested sequent systems for intuitionistic and modal logics**
  Lutz Strassburger
  INRIA Research Centre Saclay – Île-de-France (France)
  lutz@lix.polytechnique.fr

- **On the truth and meaning of constructive modalities**
  Luiz Carlos Pereira
  Catholic University of Rio de Janeiro/PROCAD (Brazil)
  luiz@inf.puc-rio.br

Accepted papers

- **A multi-modal type theory and its procedural semantics for safe distributed programming**

Giuseppe PRIMIERO
Centre for Logic and Philosophy of Science, Ghent University (Belgium)
Giuseppe.Primiero@UGent.be

- **Constructive Provability Logic**

Robert SIMMONS, Bernardo TONINHO
Carnegie Mellon University (USA)
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- **A generalization of Dugundji's theorem**

Newton Marques PERON, Marcelo E. CONIGLIO
University of Campinas (Brazil)
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- **Expectations: On pragmatic interpretations of classical logic in a bi-intuitionistic environment**

Gianluigi BELLIN, Kurt RANALTER
Universita di Verona (Italy)

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