SCIENCE

PHILOSOPHY

GWP.2013

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Gesellschaft für Wissenschaftsphilosophie

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First International Conference
of the German Society for Philosophy of Science

# How Much Philosophy in the Philosophy of Science?

Leibniz University Hannover, Germany
March 11-14, 2013

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First Conference of the

German Society for Philosophy of Science

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Gesellschaft für Wissenschaftsphilosophie

Leibniz Universität Hannover

11-14 March 2013

German Society for Philosophy of Science - Gesellschaft für Wissenschaftsphilosophie

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***Organization of this conference:***

- Paul Hoyningen-Huene (Hannover)
- Dietmar Hübner (Hannover)
- Meinard Kuhlmann (Bielefeld)
- Holger Lyre (Magdeburg)
- Thomas Reydon (Hannover)
- Torsten Wilholt (Hannover)

# Contents

# Preface

Philosophy of science, in the last decades, has evolved into a number of autonomous subdisciplines, the disciplinary standards of which derive much more from the respective scientific disciplines on which they focus than from any core of common methodological, epistemological or metaphysical principles. For many, that seems to fulfill the dream of philosophy as approaching finally the firm course of science. However, the legitimate question arises as to how much philosophy there is in recent philosophy of science: How strongly has philosophy of science moved towards science and, perhaps, away from the philosophical tradition? How is this move to be valued? What are its consequences for the scientific relevance of the work that philosophers of science do? What are its consequences for philosophy of science as a unified and recognizable discipline? And in which direction should philosophy of science move in the near future? These are the focal questions of this conference.

Without any doubt, this conference will in various ways certify the increasing dissociation of philosophy of science by presenting pieces of actual debates in the philosophy of physics, the philosophy of biology, the philosophy of social sciences and other subdisciplines. Nevertheless our aim is to also furnish some evidence to the ongoing quest for unity that we think is present in contemporary philosophy of science. Despite the differentiation mentioned above, there is lots of boundary work going on between the different areas of philosophy of science. In part, this occurs because some currently intensely debated concepts, such as causality, mechanisms or complexity, transgress the divisions between philosophy of physics, biology or social sciences. Thus, participants were asked to reflect on which philosophical themes, debates, foundational ideas, methods, orienting traditions, etc. steer their way through the particulars of their research in philosophy of science. Covered by a surface of technical elaboration, philosophers of science may identify again some hidden traces of unity.

Therefore, the first international conference and kick-off meeting of the German *Society for Philosophy of Science/Gesellschaft für Wissenschaftsphilosophie (GWP)* is devoted to the question *How much Philosophy in the Philosophy of Science?*

We gratefully acknowledge financial support by:

- DFG – Deutsche Forschungsgemeinschaft
- Leibniz Universität Hannover
- Springer science+business media
- Andrea von Braun Foundation

# Practical Information

**Registration/Information Desk**

The Registration and Information Desk will be on the 14th floor of the Conti-Hochhaus (building 1502) and will be permanently open.

**Book Exhibit**

Publishers will display their books on the 14th floor.

**Internet facilities**

There will be free WiFi Service on the 14th floor of the Conti-Hochhaus. Access data will be given upon registration and can be asked for at the Information Desk.

**Conference rooms**

The Conference sessions take place in the following rooms, which are all located in the Conti-Hochhaus (building 1502):

- 14. floor (registration, coffee breaks, social event and lounge area)
- room 009 (ground floor, sessions)
- room 013 (ground floor, plenary lectures, sessions, GWP meeting)
- room 116 (first floor, sessions)
- room 415 (fourth floor, sessions)
- room 703  (seventh floor, sessions)
- room 1415 (fourteenth floor, lunchtime workshop, panel discussion)

## Lunch and dinner

The university cafeteria "Contine" is located on the courtyard across from the conference venue. You can eat there at university guest prices and designated conference tables (signposted "GWP.2013") are available. For the menu, see www.studentenwerk-hannover.de/conti.html.
Restaurants within a 5 min. walking distance include:
Chinese: Ente von Peking, Brühlstr. 17 (www.entevonpeking.de)
Greek: Platia, Am Klagesmarkt 10-11
Indian: Himalaya, Postkamp 18 (www.himalaya-hannover.de).
Various eateries (incl. Georgian restaurant Marani, organic Restaurant Flow Food, Restaurant/Cafe Spandau, several kebap houses, Subway, Bagel Brothers) can be found on Engelbosteler Damm within a walking distance of 10-15 min. from the conference venue. Bars in the same area include Mottenburg (Oberstr. 12) and Destille (Im Moore 3).

# Sessions

## Monday, 11 March • 15:00-16:15

**Plenary Lecture (R 013)**

Peter Godfrey-Smith: *On the Relation between Philosophy and Science*

## Monday, 11 March • 16:45-18:45

***Mechanisms I* (R 415)**

Jens Harbecke: *What is the Relation between the Regularity Theory of Mechanistic Constitution and Gillett's Dimensioned Realization?*
Marcin Miłkowski: *Boundaries of Systems vs. Boundaries of Mechanisms*
Carlos Zednik: *Heuristics of Mechanism-Discovery and the Limits of Mechanistic Explanation*

***General Philosophy of Science I* (R 116)**

Vincenzo Politi: *Philosophy of Science as Link and Integrative Pluralism in Action*
Hans Radder: *What Kind of Philosophy in the Philosophy of Science?*
Marie I. Kaiser: *How Normative Is Naturalistic Philosophy of Science?*

***Metaphysics* (R 009)**

Markus Schrenk: *How Much Metaphysics in the Philosophy of Science?*
Cord Friebe: *Metametaphysics: What is a Deflationary View?*
Julia Friederike Göhner: *Scientia Mensura: On Science as the Measure for Metaphysics*

***Cross-Disciplinary Analyses* (R 013)**

Eran Tal: *Comparing Uncertainties: A Cross-Disciplinary Challenge*
Wolfgang Pietsch: *Natural and Causal Laws in Physics and Engineering*
Maria Kronfeldner: *To Have an Effect of One's Own: Causal Complexity, Reconstituting the Phenomena, and Explanatory Values*

***Symposium: Philosophy of Biology***

Emanuele Ratti [Emilio M. Sanfilippo, Federico Boem]: *Ontology for and from Sciences. The Ontological Analysis of Biology*

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**JGPS Plenary Lecture (R 013):**

Wolfgang Spohn: *A Priori Principles of Reason*

Tuesday, 12 March • 9:15-10:30

**Plenary Lecture (R 013):**

Margaret Morrison: *The Scientific Nature of Philosophical Questions*

Tuesday, 12 March • 11:00-13:00

***Philosophy of Chemistry and Technology* (R 116)**

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***Cognition and Concepts* (R 703)**

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***Models and Representations*** **(R 009)**

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Michael Poznic: *Five Ultimate Arguments against Similarity Views of Scientific Representation?*
Maria Serban: *A Place for Contextualism in Philosophy of Science*

***Symposium: Mechanisms*** **(R 013)**

Phyllis Illari, Stuart Glennan and Meinard Kuhlmann: *The New Mechanical Philosophy and the Unity of Science*

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**Plenary Lecture (R013):**

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## Tuesday, 12 March • 16:45-18:45

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Matthias Unterhuber: *Less Lazy than One Might Think – Ceteris Paribus Conditions in the Context of Lewis' Best System Analysis*
Andreas Hüttemann: *In Laws We Trust*

***Philosophy of Biology*** **(R 116)**

Shunkichi Matsumoto: *Evolutionary Functional Analysis Revisited*
Predrag Šustar and Zdenka Brzović: *The Function Debate in the Light of Molecular Approaches to Evolutionary Biology: The Case of Neo-Functionalization*
Stavros Ioannidis: *Development and Evolutionary Causation*

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***Symposium: General Philosophy of Science* (R 703)**

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## Wednesday, 13 March • 9:15-10:30

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## Wednesday, 13 March • 11:00-13:00

***Experiments* (R 415)**

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Lena Hofer: *(Re)Production of Empirical Scenarios*
Jan Sprenger: *The Interpretation of Sequential Trials in Medicine. A Plea for Conditional Reasoning*

***Causality* (R 009)**

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Patryk Dziurosz-Serafinowicz: *Are Humean Chances Formally Adequate?*
Johannes Roehl: *Physical Causation, Dispositions and Processes*

***Symposium: Models and Representations*** **(R 116)**

Mathias Frisch, Rafaela Hillerbrand and Herman Russchenberg: *Uncertainty in Climate Modeling*

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## Wednesday, March 13 • 16:45-18:45

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Holger Andreas: *Descriptivism about Theoretical Concepts Implies Ramsification or Conventionalism*

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Robert Meunier: *Pluralism in the Life Sciences – Complexity of Nature or Complexity of Culture*
Fabian Lausen: *Using Insights from the Philosophy of the Life Sciences in the General Reductionism Debate*

***Philosophy of Emotions*** **(R 703)**

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Predrag Sustar: *Naturalism in Action: The Case of Positive Emotions*
Jeff Kochan: *Subjectivity and Emotion in Scientific Research*

***Philosophy of Physics I* (R 009)**

Karim Thebault: *Quantization as a Guide to Ontic Structure*
Stefan Lukits: *The Full Employment Theorem in Probability Kinematics*
Johannes Thürigen: *Theory Evaluation beyond Empirical Evidence: the Case of Research towards a Quantum Theory of Gravity*

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## Wednesday, March 13 • 18:45

**Panel discussion (R 1415):**

*Caught between a rock and a hard place –*
*Prospects and problems of careers between philosophy and science*

Panelists will include:

- Dr. Christoph-Friedrich von Braun, MSc (Andrea von Braun Foundation, Munich)
- Dr. Thomas Brunotte (Volkswagen Foundation, Hanover)
- Prof. Dr. Martin Egelhaaf (Bielefeld University)
- Prof. Dr. Paul Hoyningen-Huene (Leibniz University Hanover)
- Dr. Robert Meunier (Institute for Cultural Inquiry, Berlin)
- Prof. Dr. Sandra Mitchell (University of Pittsburgh)

The panel discussion is moderated by Dr. Marie I. Kaiser (University of Geneva)

Organisation: Jun.-Prof. Dr. Maria Kronfeldner (Bielefeld)

## Thursday, 14 March • 9:15-11:15

***Philosophy of Physics II* (R 009)**

Manfred Stöckler: *How to Divide between Physics and Philosophy of Physics?*
Emre Keskin: *Philosophy of Cosmology: Not Enough Philosophy, not Enough Cosmology.*
Thorben Petersen: *Is There too Much Philosophy in the Rietdijk/Putnam-Argument?*

***Historically Oriented Studies* (R 116)**

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Dinçer Çevik: *Meeting the Metaphysics of Geometry: The Legacy of Herbart, Gauss and Riemann*
Parzhad Torfehnezhad: *In Carnap's Defense*

***Philosophy of Social Sciences (incl. Economics)* (R 415)**

Andrei Nasta: *A Justification of the Minimalist Notion of Economy*
Kristina Musholt: *The Personal and the Subpersonal in Social Cognition*
Simon Lohse: *Social Emergentism Reconsidered*

***General Philosophy of Science III* (R 703)**

Anke Büter: *The Agnosticism-Argument for Value-Freedom*
Adam Toon: *Models, Fictions, and Emma Bovary*
Stephan Kopsieker: *Making Sense of the Distinction between Functional and Structural Modularity*

***Symposium: Philosophy of Biology* (R 013)**

Christian Sachse, Ulrich Krohs and Ellen Clarke: *Organisms and Biological Individuals – Metaphysical and Epistemological Reflections on the current Debate*

## Thursday, 14 March • 11:45-13:00

**Plenary Lecture (R013):**

Sandra Mitchell: *Proteins in Context: Relations among Multiple Models*

# Panel Discussion

## Wednesday, March 13 • 18:45-20:30

Room 1415, 14th level, Conti-Hochhaus (Building 1502)
Königswörther Platz 1, 30167 Hannover

### Caught between a rock and a hard place –

### Prospects and problems of careers between philosophy and science

The topic of the conference, "How much philosophy in the philosophy of science?", is of special relevance for aspiring young researchers. Pre-Docs as well as Post-Docs often find themselves 'between a rock and a hard place', facing demands of a dual nature: On the one hand they face the rigorous standards of a career in philosophy, while on the other hand they are expected to possess detailed knowledge of the specific sciences for their work in philosophy of science. These two poles are often difficult to bridge, and can result in a serious tension being exerted on young researchers. For instance, though the need for interdisciplinary research is impressed upon young researchers by their advisers and by funding institutions, university positions are still mainly filled based on decidedly disciplinary profiles. Is this an unresolvable conflict for up-and-coming professionals aspiring for a career in philosophy of science? This panel discussion will focus on this and similar questions concerning philosophy of science as an interdisciplinary field in general as well as the prospects and problems of careers between philosophy and science.

There will be a short reception (appetizers and drinks) in the Foyer at 18:45, before we begin at 19:00.

**Panelists will include:**

- Dr. Christoph-Friedrich von Braun, MSc (Andrea von Braun Foundation, Munich)
- Dr. Thomas Brunotte (Volkswagen Foundation, Hannover)
- Prof. Dr. Martin Egelhaaf (Bielefeld University)
- Prof. Dr. Paul Hoyningen-Huene (Leibniz Universität Hannover)
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The panel discussion is moderated by Dr. Marie I. Kaiser (University of Geneva)
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**Kindly supported by the Andrea-von-Braun-Foundation.**

The Andrea von Braun Foundation aims to contribute to the dismantling of barriers between disciplines, with particular emphasis on the cooperation of fields of knowledge that normally have no or only very little contact with one another. The idea is to create opportunities for the mutual enrichment and cross fertilization of ideas, thus opening access to new and often surprising results and understanding.

# Abstracts

# Plenary Lectures

## Monday, 11 March • 15:00-16:15

**Peter Godfrey-Smith (R013)**
***On the Relation between Philosophy and Science***

I will present some ideas about the role of philosophy in contemporary intellectual life, focusing especially on the relation between philosophy and the sciences. This will include discussion of recent criticisms of philosophy made by physicists (Hawking, Weinberg, and others). The general view of philosophy I will defend gives a central place to a "synoptic" role associated with Sellars, and gives a secondary but still important place to an "incubator" role. The treatment of role of philosophy will be guided also by ideas drawn from recent philosophy of biology; intellectual movements can be treated in typological and lineage-based ways. Both have a role in this case.

## Tuesday, 12 March • 9:15-10:30

**Margaret Morrison (R013)**
***The Scientific Nature of Philosophical Questions***

Beginning with the rise of logical empiricism the relation between philosophy of science and scientific practice has been largely dominated by philosophical issues relating to criteria for belief in scientific entities and theories, the correct form that scientific explanation ought to take, etc. In most contexts the input from science itself involved furnishing examples that would support a particular philosophical view about the nature and structure of science. If the goal of philosophy of science is to both inquire into and illuminate the philosophical foundations of particular sciences it is far from clear that those goals have been achieved in many current accounts that emphasise the epistemology or metaphysics of science. How, for example, does an account of dispositions enable us to understand the foundations of condensed matter physics? Similarly, how does Platonism in the philosophy of mathematics illuminate the nature of mathematical explanation in quantum field theory? As we know from foundational studies in fields like quantum mechanics, individual sciences, theories and methodologies often generate their own philosophical problems and questions, issues that can only be resolved through a careful analysis of theoretical presuppositions and methodological practices. There is a robust and prominent role for philosophy in this kind of investigation, but one that must take science rather than metaphysics as its starting point. My goal in this talk is to discuss some examples of scientifically motivated philosophical problems, how they differ from more traditionally defined problems in the philosophy of science, and how philosophical analysis can perhaps engage with science in a more meaningful way.

## Tuesday, 12 March • 15:00-16:15

**James Ladyman (R013)**
***Philosophy, Science and Realism***

How does scientific realism relate to discussions of realism in philosophy of physics with respect to quantum mechanics and space-time, and how do these issues relate to more general philosophical questions about realism concerning common sense and everyday ontology?

## Wednesday, 13 March • 9:15-10:30

**Stephan Hartmann (R013)**
***Philosophy of Science as Scientific Philosophy***

What is the proper method of philosophy? To what extent does the philosophical method differ from the scientific method? Many philosophers believe that philosophy is an armchair activity and that the exact methods of the natural and social sciences cannot guide philosophical research. Scientific Philosophy, on the contrary, maintains that philosophical theses and arguments should be just as clear and precise as scientific ones: philosophers ought to build theories and models as much as scientists do; and the application of mathematical methods as well as input from empirical studies are often necessary in order to gain new insights into old philosophical questions and to progress to new and deeper ones. This talk spells out Scientific Philosophy by focusing on central themes from the philosophy of science. It focuses on understanding aspects of scientific rationality and present descriptively adequate and normatively interesting models of scientific explanation, intertheoretic relations, and (time-permitting) decision-making in a scientific community. These topics have a philosophical as well as a scientific dimension, and addressing them requires a combination of methods from both areas.

## Wednesday, March 13 • 15:00-16:15

**Chrysostomos Mantzavinos (R013)**
***Explanatory Games***

A philosophical theory of explanation should provide solutions to a series of problems, both descriptive and normative. The aim of the lecture is to establish the claim that this can be best done if one theorizes in terms of explanatory games rather than focusing on the explication of the concept of explanation. The development of the precise meaning of the concept of scientific explanation occupies centre-stage in all contemporary approaches. The discussion of three examples from the social sciences - neoclassical economic theory, the theory of civil wars and econometrics – will show that the unitary models of explanation have at best limited application. The lesson that is drawn is that each of the three main models currently on offer, the unificationist, the mechanistic, and the manipulationist, can accommodate only some of the existing scientific practices in different social scientific domains.

The alternative position that seems obvious and which is adopted is that of an explanatory pluralism. At every moment of time there is a stock of explanations available in a society proposed by ordinary people “in the wild” or by specialists organized formally or semi-formally within specific organizational structures such as churches, universities, etc. This explanatory reservoir is distributed among diverse individuals and groups in the society under conditions of a cognitive division of labour. The terms of provision, control, and dissemination of explanations in this collective explanatory enterprise are regulated by the different rules that the participants have come to adopt over time. These rules incorporate the normative standards that guide the processes of discovery and justification of explanations as well as the modes of their communication, dissemination, and adoption. They constitute the rules of the explanatory game that the participants are playing. The philosophical project consists in describing and normatively appraising the rules that constitute these games. This project is fundamentally liberal, in the sense that participants and non-participants to the game alike engage in the critical discussion and revision of the rules or to put it in other terms, the project is fundamentally naturalistic - philosophers and scientists equally take part in it.

## Thursday, 14 March • 11:45-13:00

**Sandra Mitchell (R013)**
***Proteins in Context: Relations among Multiple Models***

In the early days of protein science, solving the “problem of protein folding” was characterized in a straightforwardly reductive way. The function of a protein in an organism would be determined by its structured conformation and that confirmation was determined by the atoms forming the sequence of amino acids that comprise the protein. This promise of reductive explanation has not been realized. Instead, as proteins are investigated in increasingly complex contexts, the “problem” of folding has yielded multiple models engaging more complex causal accounts and shifting ontologies. I will argue that both scientific and philosophical arguments support an integrated pluralistic view of the relations between models, supplanting reductionism.

# Sessions

## Monday, 11 March • 16:45-18:45

*Mechanisms I*

**Jens Harbecke**
***What is the Relation between the Regularity Theory of Mechanistic Constitution and Gillett's Dimensioned Realization?***

A central concept of the mechanistic approach to neurobiological explanation is that of ‘mechanistic constitution’, resp. ‘constitutive relevance’. It is at the heart of the mechanists’ model of explanation in the sense that an explanation of a phenomenon in neurobiology is taken to be informative only if an adequate description of a mechanism constituting that phenomenon is achieved (cf. Machamer&Dargen&Craver 2000, Philosophy of Science). Furthermore, the ontology promoted by the mechanists is essentially based on the constitution relation.

The pertinent literature contains several different, and in part incompatible, definitions of mechanistic constitution. For example, Carl Craver has proposed a manipulationist account of constitution, where other authors identify mechanistic constitution with the notion of supervenience. Again others appear to identify it with the parthood relation.

Probably the most detailed analysis of constitution so far has been provided by Jens Harbecke (2010, International Studies in the Philosophy of Science) and Mark Couch (2011, Synthese), who independently have defended a regularity theory of constitution. The relation is described as a second-order (relating mechanistic types) and as being expressible by a particular kind of conditional called a ‘constitutive minimal theory’ (Harbecke) or a ‘constitutive INUS-conditional’ (Couch).

An interesting aspect of these approaches consists in the fact that they share certain structural features with what in the philosophy of mind has become known as the ‘dimensioned view of realization’. The idea was introduced to the debate by Carl Gillett (2002, Analysis), who describes constitution as a systematic second-order relation between properties that is constrained by a mereological connection between the individuals instantiating the properties. In the words of the author, “a property/relation instances F1-Fn realize an instance of a property G, in an individual s, iff s has powers that are individuative of an instance of G in virtue of the powers contributed by F1-Fn to s or s's constituents, but not vice versa." (cf. p. 322)

This paper tries to understand in detail the commonalities and differences of the notions of regularity mechanistic constitution as developed by Harbecke and Couch and of dimensioned realization as proposed by Gillett.

In a first step, the similarities of the notions are made precise. It is pointed out that both are defined as second-order relations based on certain regularities and that they demand a mereological relationship between the individuals involved. In a second step, several differences are made explicit. One concerns the restriction to mechanistic properties as potential relata that the regularity approaches carry out. A further one concerns the fact that a minimality constraint is at the heart of the regularity approaches, but does not appear in the dimensioned view. Finally, the two notions differ in defining the relation either as (a)symmetrical and (ir)reflexive. Several consequences of these differences are discussed. In a final step the findings are used to assess the value of the two notions for the understanding of scientific inquiry in the neurosciences. It is argued that regularity mechanistic constitution captures better the interlevel mechanistic relations that the neurosciences often study.

**Marcin Miłkowski**

***Boundaries of Systems vs. Boundaries of Mechanisms***

Craver (2007) defended a view that a mechanism includes only components constitutively relevant for the phenomenon exhibited by the mechanism, and that the sufficient condition of constitutive relevance is that components and the mechanism are mutually manipulable (MM). At the same time, he criticized the idea developed by Simon (1962), Haugeland (1998), and Grush (2003) that complex systems are to be decomposed according to the intensity of interaction (II) among their parts. While it was argued that MM, contra Craver, makes constitutive relevance indistinguishable from causal relevance (Leuridan 2011, Menzies 2012), the claim that Simon's view is useless for mechanistic decomposition was unchallenged. I intend to do this in this talk.

Simon and Craver offered criteria for quite different entities. Simon's entity is a system (decomposable, near-decomposable and non-decomposable); it is essentially a structure. Simon's systems can host multiple overlapping mechanisms for different explananda. Craver's mechanism is always a mechanism for or of something, so it's a causal structure that exhibits an explanandum phenomenon. Epistemic interest is inextricably linked with the way the boundaries of the mechanisms are sketched (Craver 2009).

I will argue that two distinct problems are not to be confounded: (1) the problem of specification of explanandum, which is linked to epistemic interest; (2) the problem of confirmation that the causal structure of mechanism is also the structure of the explanandum. Simon's criteria are useless as a solution for (1).

According to Craver, two main problems for Simon's account are background conditions and sterile effects, or interactions irrelevant to the explanandum. Yet II can be couched in terms of causal density, which can be measured as weak Granger 'causality' (Seth 2010). I will show that Granger 'causality' is essentially equivalent to screening-off in that it automatically excludes predictively irrelevant factors. For this reason, Craver objections are no longer detrimental to II.

There are two families of measures of system integration (or autonomy) that seem to correspond to II and MM: interactivity measures and causality measures (Bertschinger et al. 2008). The former are easier to apply before forming a complete hypothesis about the causal structure. They are used to discover candidates for further experimental manipulations whose results that can be then tested with latter measures.

Yet it is too crude to say is II simply data-driven, and MM theory-driven. One cannot investigate all possible physical factors in a system, so there will be at least an implicit theoretical choice of relevant factors in data-driven II approaches to system decomposition. Also, systems are not delineated with respect to a single explanandum.

I will conclude by stressing that statistical modeling is used to discover, describe and test various hypotheses about the causal structure, as in research on measures on consciousness in neuroscience (Seth 2010). Importantly, having statistical models, one can assess various strategies of lumping or splitting the explanandum phenomenon in terms of predictive value of the proposed underlying structure. All this shows that it is rather philosophy of (neuro)science that can inform philosophy rather than the other way 'round.

Please contact the author for references.

**Carlos Zednik**
***Heuristics of Mechanism-Discovery and the Limits of Mechanistic Explanation***

In one of the most-cited works on mechanistic explanation, Bechtel & Richardson (1993) outline a framework in which heuristic strategies for mechanism-discovery are paramount for mechanistic explanation. These heuristics allow researchers to quickly specify and constrain the space of "how-possibly" models of the mechanism for a particular target phenomenon (Craver 2007), thereby allowing them to efficiently arrive at a "how-actually" model of the mechanism. Bechtel & Richardson place particular emphasis on the heuristic strategies of decomposition and localization, which allow researchers to break a mechanism apart and describe the contributions of each part to the phenomenon being explained. On their account, the successfulness of these heuristic strategies depends on the complexity of the system being investigated, and in particular, on its decomposability in Herbert Simon's (1996) much-discussed sense.

Except for Bechtel & Richardson's seminal contribution, current discussions of mechanistic explanation rarely consider the role of heuristic strategies for mechanism-discovery. As a consequence, Bechtel & Richardson's account goes largely unquestioned, and two assumptions about mechanistic explanation are fairly widespread. First, decomposability in Simon's sense is widely thought to be a precondition for mechanistic explanation; phenomena that arise from non-decomposable systems are typically deemed to lie beyond the scope of mechanistic explanation. Second, decomposition and localization are widely thought to be universal features of mechanistic explanation: abandoning the former is assumed to inevitably lead to an abandonment of the latter.

Both of these assumptions can be questioned, however. For one, the advent of novel analytic techniques from e.g. dynamical systems theory can allow for componential descriptions of non-decomposable systems: decomposability is not a precondition for successful decomposition and localization. Second, novel heuristic strategies may allow researchers to discover mechanisms not amenable to decomposition and localization. For example, in the field of evolutionary robotics computer simulations are frequently used to produce artificial mechanisms that resist decomposition and localization. Still, these artificial mechanisms can sometimes be used to reason about particular mechanisms in the real world, and thereby greatly facilitate the process of mechanism-discovery.

In summary: decomposability in Simon's sense is not a precondition for decomposition and localization, and successful decomposition and localization is not a precondition for mechanistic explanation. The limits of mechanistic explanation need to be re-examined.

## *General Philosophy of Science I*

**Vincenzo Politi**
***Philosophy of Science as Link and Integrative Pluralism in Action***

Before answering

- Q1: "How much philosophy in philosophy of science?"

I ask

- Q2: “What is the role of philosophy of science?"

The talk is in two parts. In the _rst part, I answer Q2 by comparing two views on the role of philosophy of science: the `two-way-relation-view' (TWR- view) and the `link-view' (L-view).

For TWR-view, philosophy of science and science are in a two-way relation, such as the previous is a sort of `extension' of the latter. Philosophy should mirror the methods of science, thus becoming a `scienti_c philosophy', and it's disciplinary structure, by splitting in `special philosophies of science', each devoted to a special scientific discipline.

For L-view, philosophy of science is not reducible or subordinate to science. Rather, philosophy `connects' science with something else - i.e., with other branches of philosophy (Frank), with society and its needs (Neurath, Feyerabend), with the uses of science (Cartwright).

I claim that Q1 poses some problems to TWR-view only. Not only L-view is immune from these problems, it also offers the best answer to Q2.

In the second part, I examine one version of L-view: philosophy of science as a link between science and policy.

After considering some ideas about science and policy - Cartwright (1999, 2007), Rosenberg (2000), Douglas (2009) - I focus on the recent work of Mitchell (2009). Mitchell applies her integrative pluralism, already defended in her (2003), to the analysis of the policy-process for the resolution of `complex problems'. She compares the traditional `cost/benefits-analysis' with the `scenario analysis' and the `robust adaptive planning', arguing in favour of the latter methods.

Mitchell's proposal is a good version of L-view. Her idea is that the policyprocess could benefit from a philosophical reection upon science, in particular upon the integration and of different non-reducible sciences.

My claim is that, in order to work properly, the `link' should work in both direction. A philosophical reection upon science can improve the policy process, but what is the import of a philosophical reection upon policy to our idea of science?

This question leads to two problems:

1. the Dilthey-Taylor problem: for Mitchell the difference between natural and social sciences is of grade of complexity rather than of kind. Problems with some policies in some special social setting may suggest otherwise -

as in the cases of implementation of medical treatments in non-Western societies, social experiments in Economics, and so on;

2. the Lakatos problem: Mitchell model tells which policy is more robust and more likely to work. It does not tell at which point the `actual effectiveness' of a policy should be assessed. The problem consists on finding an answer to the question: "How long should we wait to see the effects of the implemented policy?"

More than challenges to Michell, (1)-(2) aim to suggest new avenues for improving her model and making the philosophical link between science and policy more robust.

**Hans Radder**
***What Kind of Philosophy in the Philosophy of Science?***

In view of the increasing specialization and fragmentation in philosophy of science, the theme of the conference is timely and important. As I have argued elsewhere, I agree that a greater interaction between general philosophy and philosophy of science is both needed and fruitful. A problem is, however, that philosophy at large also includes a substantial diversity of approaches and views. For this reason, the question posed in the title of this paper is pertinent. I will address this question in the following way.

First, I present an account of the discipline of philosophy as theoretical, normative and reflexive. This metaphilosophical view will be discussed, first, by explaining the meaning of its three characteristics and, second through its contrast with alternative conceptions. These alternatives include "normal" philosophy (e.g., in the analytic tradition), naturalism (e.g., in cognitive approaches) and empirical philosophy (e.g., in science and technology studies).

Second, I provide brief examples of a theoretical, normative and reflexive philosophy of science. Theoretically, I will discuss the account of experimentation in terms of reproducible material realization as exemplifying ideal-typical explanation. Normatively, I will argue for a socially relevant philosophy of science on the basis of a neo-Mertonian critique of commodified academic research. Reflexively, I will demonstrate the unavoidable value-ladenness of philosophy of science, and I will discuss some examples of the values presupposed in specific philosophies, especially those pertaining to the relationship between science, society and morality.

**Marie I. Kaiser**

***How Normative Is Naturalistic Philosophy of Science?***

One of the trends that characterize the development of philosophy of science (and of philosophy of biology, in particular) in the last decades is a turn towards scientific practice. Several contemporary philosophers of science emphasize that it is important to pay close attention to actual scientific practice. The assertion is that when a philosopher theorizes for instance about reduction in biology he is supposed to take into account actual cases of reduction in biological practice – at least if he aims at understanding real science. This way of doing philosophy of science is often referred to as being "descriptive" or "naturalistic" (e.g. Craver 2007; Wimsatt 2007; Bechtel 2008; Mitchell 2009). It is opposed to what can be called normative approaches in philosophy of science, which make claims about how a certain element of science (e.g. causation or adaptation explanation) should be conceived – regardless of whether this view is in accordance with how science actually is performed (e.g. how scientists in fact draw causal inferences or explain adaptive evolutionary processes).

In this paper I argue that, although descriptive/naturalistic projects in philosophy of science can be clearly distinguished from normative ones, what in fact exists is a whole range of different kinds of projects, and that purely descriptive and purely normative approaches constitute only the two end points of this range. Furthermore, I claim that what distinguishes descriptive/naturalistic projects from normative ones is not that the former are completely devoid of normative assumptions. Rather, the kind of normativity that is involved in the two types of projects differs considerably.

I specify the kind of normativity that is involved in descriptive/ naturalistic approaches by identifying three respects in which they need to transcend "mere descriptions" of scientific practice and be critical reconstructions: first, they must focus on the analysis of paradigmatic and important examples, second, they must make explicit assumptions that are only implicit available in scientific practice, and finally, they must establish coherence between conflicting views. This implies making normative claims about what should be regarded as a paradigmatic example and about which views of scientists should be dismissed as incorrect or as too vague.

This kind of normativity can be clearly distinguished from the one that is involved in normative projects. Generally speaking, proponents of normative projects abandon the aim of understanding scientific practice altogether and develop an account of how scientific practice ideally should look like. In other words, they replace descriptive adequacy with other criteria of adequacy, like accounting for intuitions or metaphysical suitability. If empirical information about actual scientific practice is included into normative projects it does play no central role in justifying the assertions that are made.

*Metaphysics*

**Markus Schrenk**
***How much Metaphysics in the Philosophy of Science?***

Traditionally, metaphysics has been seen as the enquiry into what lies behind or comes before experience, yet, which nonetheless concerns the fundamental structure of reality. However, because metaphysical claims seem not to be empirically testable the meaningfulness of metaphysics has been contested ever since the classical empiricists. The critique culminates in science focussed 20th century logical empiricism which denounced metaphysics as nonsensical altogether.

Logical empiricism is also one of the founding fathers of modern philosophy of science, yet, ironically, some present day philosophers of science have again turned emphatically towards metaphysical reasoning and propose grand (speculative) systems in order to answer questions like what is a law of nature, what are natural kinds, what is causation, etc. (Dispositional essentialism might serve as an example of a metaphysically charged theory.)

However, in the last decade analytic metaphysics has come under fire again and is critically evaluated. Philosophers have again started to debate which kind of metaphysics is and is not allowed (cf., for example, Ladyman & Ross in Every Thing Must Go, Peter Godfrey -Smith in Philos Stud 2012, 160: 97–113), and papers in Chalmers (ed.) Meta-Metaphysics 2009).

In a first step, this paper collects and critically evaluates those sources, methods, and guidelines that were proposed as being acceptable for metaphysics in the recent literature. There is, for example, a suspicious consensus amongst philosophers when it comes to the request for coherence, simplicity, clarity, and depth of metaphysical theories. This agreement, however, extends to no more than minimal rational requirements, for who would ask of any theory to be incoherent, knotty, opaque, and shallow?

More meaty and therefore also controversial guidelines or sources for metaphysical insights to be found in the literature are:

(I) intuitions, probably of modal kind, and either gained from everyday or scientific (linguistic) practice, cf. (Meixner 1999: 128); (Callender 2011: 44);

(II) inferences to the best (available) explanation, cf. (Hawley 2006: 458 & 454); (Beebee 2009: 4); (Roberts 2008: 257); (Lipton 1991/²2004);

(III) scientific realism as (necessary) presupposition for scientific metaphysics, cf. (Lewis 1986), (Armstrong 2010), (Ellis 2001: 53-4)

(IV) being in tune with today's most advanced scientific theories and practice, cf. (Sider 2001: 42); (Ladyman and Ross 2007: 30); (Callender 2011: 48);

(V) the demand that as few as possible non-empirical hypotheses shall be postulated;

(VI) reduction and unification requests, partially inspired by Ockham's razor;

(VII) posits within the metaphysics of science shall be illuminating also in other areas of philosophy as, for example, in mental causation, free will, etc.

After having evaluated these entries, the paper will, second, argue for a tamed pluralism of metaphysical theory building which is akin to Carnap's tolerance principle for empirical theories: as long as you make explicit which of (I)-(VII) you reject or endorse we can, neutrally, evaluate how well you succeed (here the general criteria of coherence, simplicity, clarity, and depth can indeed be decisive). Metaphysical theories that are based on radically different emphasis of (I)-(VII) can, as above, be equally well evaluated internally but they cannot externally be compared to theories that praise radically different entries.

**Cord Friebe**
***Metametaphysics: What is a Deflationary View?***

Analytic Metaphysics has moved further away from science, while philosophy of physics has moved closer to mathematical physics. The reason for this increasing gap might be that mainstream philosophers of physics still are skeptical regarding ontological disputes. Such debates often allegedly express no substantial disagreements, being rather merely verbal and hence irrelevant for a better understanding of physics. What is required, instead, is a so-called "deflationary view".

But what exactly is "deflationism", beyond the platitude that it is intended to be a metaphysical view 'closest to physics'? What exactly distinguishes a substantially ontological debate from a dispute being 'merely verbal'? Are ontological claims, if well-formed, empirical claims? "Metametaphysics", a rather new field within analytic metaphysics, provides answers to these questions, which addresses the more general question: how much ontology needs philosophy of physics?

My case-study concerns the dispute between Humean metaphysicians, holding that fundamentally physical properties are categorical, and Anti-Humeans, claiming in contrast that they are essentially dispositional. Humeans are after a reductive understanding of causality, while Anti-Humeans allow for de-re modalities within our actual world. Consequently (?), the Humeans usually are considered to be the deflationists being closer to physics, while the Anti-Humeans are regarded to be ‘heavyweight’ ontological realists, claiming that there exists much more than physics implies, namely all these necessary connections between real events in our world.

My thesis will be: using the instruments from metametaphysics it can be shown, contrariwise to the first impression, that the Humeans’ ontology is by no means ‘thinner’ or more ‘lightweight’ than the Anti-Humeans’ one. Metametaphysics, to be more concrete, requires for a deflationist view a particular way to argue, namely to analyze working-physicists talk and actually physical theories rather than to apply sophisticated philosophical reasoning, like quidditas-argument or antidotes-speculations. The debate, then, reduces to the question: could, in principle, actually scientific theories imply what is metaphysically possible? If not, and I opt for “no”, Humean and Anti-Humean metaphysics are likewise ‘thick’ ontologies.

**Julia Friederike Göhner**
***Scientia Mensura: On Science as the Measure for Metaphysics***

In an age of finely articulated scientific theories and rapidly progressing specialized sciences, it appears that there is no room for the potentially unresolvable debates of a speculative “armchair metaphysics” [Jackson (1994), p.23] obscurely grounded in elusive intuitions. Therefore, among naturalistically inclined philosophers of science pondering the value of metaphysical reflection, a principle of scientia mensura has become fairly popular: “[I]n the dimension of describing and explaining the world, science is the measure of all things, of what is that it is, and of what is not that it is not.” [Sellars (1963), p. 173] Accordingly, the proponents of this principle hold that metaphysical reasoning is worth the effort only if continuous with and informed by science; otherwise, its debates are moot and should be abandoned altogether.

The objective of my talk will be twofold. First, I will explore the various forms the thesis of scientia mensura takes on when applied to metaphysics in the writings of its contenders. Generally put, “naturalistic” [Ladyman et al. (2007), p.1] or “scientific metaphysics” [Callender (2011), p.50] (as opposed to traditional, highly speculative, “autonomous metaphysics” [Ladyman (2007), p.181]) is ascribed the task of bridging explanatory gaps, either between scientifically acclaimed theories in the specialized sciences, aiming at their unification, or between these theories and our everyday experience, generating a comprehensive world-view. I shall call the first view strictly naturalistic and refer to the latter as scientifically-informed metaphysics. Defenders of both views hold that answers to certain – if not all – relevant questions traditionally subsumed under the heading “metaphysics” are capable of refutation by recourse to well-established scientific theories, as is the case with questions concerning the nature of space and time. Whereas metaphysics must thus not ignore the results of science, disagreement persists as to the role of metaphysics in relation to science: Does the value of a metaphysical claim consist solely in the service it pays to science (as postulated by [Ladyman et al. (2007), p.30]), or is metaphysical reasoning vital to science, providing theories with an interpretation and enriching the sparse, unservicable ontology they yield (see [Dorato (2011)] and [Esfeld (2006)])?

In a second step, I will critically assess the implications and restrictions placed on metaphysical reasoning by the adherents of strictly naturalistic and scientifically-informed metaphysics, respectively. Leaving open the question whether it is possible or even advisory to read off metaphysics from scientific theories (as this poses no great concern to the defenders of scientia mensura), I will argue that if a metaphysics of this ilk is to become an integral part of the philosophy of science, it must retain certain characteristics of philosophical method to remain distinguishable from the philosophically inclined physicist's Sunday morning musings. By raising this point, I hope to provide a clarifying contribution to the hotly debated issue of the status of metaphysical considerations in present-day philosophy of science.

Please contact the author for references.

## *Cross-Disciplinary Analyses*

**Eran Tal**
***Comparing Uncertainties: A Cross-Disciplinary Challenge***

Quantitative scientific results are often reported with uncertainty estimates or 'confidence intervals'. Whether such intervals are comparable to each other is not always clear, especially when the results are produced by different kinds of methods, e.g. when statistical predictions of ocean temperature are compared to infrared satellite measurements, or when estimates of chemical properties obtained by *ab initio* simulations are compared to experimental estimates obtained in the laboratory. The mutual compatibility of such results depends on their respective margins of uncertainty; however, when uncertainty originates from different sources and estimated by different methods the legitimate worry arises that reported margins lack a common measure. In the absence of a principled method of scaling uncertainties from different sources, it is difficult to tell apart genuine agreement from overestimated uncertainty and genuine disagreement from underestimated uncertainty. Moreover, it is difficult to identify which of several inconsistent results require correction and to assess the extent of the corrections required.

The problem of uncertainty comparison, I argue, can only be solved in the context of a general epistemological account of the structure and limits of quantitative estimation. Far from being a mere wrinkle in the application of statistical methods of uncertainty analysis, the problem is entangled with questions about the directionality of confirmation and the relationship between evidence and information in the sciences. Thanks to their cross-disciplinary perspective, philosophers are in an optimal position to tackle such challenges. As a first step towards such general epistemological analysis, this paper considers the role of prediction in the evaluation of measurement uncertainty. As I show, measurement uncertainty is a special case of predictive uncertainty where the relevant predictions concern the behaviour of a measurement process. My analysis draws on case studies of uncertainty evaluation of atomic clocks at the US National Institute of Standards and Technology (NIST) and of coordinate measuring machines at the German Physikalisch-Technische Bundesanstalt (PTB).

Thinking about measurement uncertainties as special instances of predictive uncertainty allows one to explain why measurement uncertainties often change when underlying assumptions about the measuring instrument are modified, despite the lack of physical alteration to the apparatus. More importantly, recognizing the modeldependence of measurement uncertainties clarifies the possibility of comparing them to uncertainty estimates associated with theoretical predictions. I conclude the paper by discussing the possible contribution of philosophy to seemingly specialized methodological concerns in the sciences.

**Wolfgang Pietsch**
***Natural and Causal Laws in Physics and Engineering***

While there are thermodynamic and mechanical theories both in physics and in engineering, even a superficial look at textbooks from these two fields shows how different the respective scientific practices are. In the paper, I argue that many of these differences can be understood by introducing a distinction between natural and causal laws. In first approximation, physicists mainly aim for those abstract natural laws that make up the axiomatic systems of physical theories, while engineers aim for the much more concrete causal laws that allow to intervene in the world.

Most current accounts of natural laws do not provide for such a distinction, notably regularity and best system approaches. Rather, for interpreting the axioms of physical theories, I take recourse to a tradition going back mainly to Mach, Poincaré, and Duhem, subsequently endorsed also in the manifesto of the Vienna Circle. According to this view, the axioms of physical theories, e.g. Newton's axioms, but also some more specific statements like the law of gravitation are implicit definitions of at least partly conventional status. Thus, many fundamental laws of physics have a largely linguistic role, they fix the conceptual relations between basic theoretical terms. While such axioms are empirically motivated, as definitions they are certainly not causal, making plausible the oft-noted relative absence of causal notions in physics.

Regarding causality, I endorse a broadly interventionist approach. Methodologically, difference-making is the core element, which links up well with historical accounts of eliminative induction like Bacon's method of exclusion or Mill's methods. Keeping in mind the crucial role of interventions and manipulations, causality seems particularly suited to address questions of the artificial world.

The analysis throws light on an old dilemma concerning the relationship between physics and engineering dating back at least to the work of Mario Bunge. Supposedly, if engineering is to be scientific, then technological rules must be derivable from the fundamental laws of physics. But this seems to imply that engineering really is just applied science, which flatly contradicts the largely independent nature of engineering knowledge. By contrast, granting the distinction between natural and causal laws, engineering knowledge can be grounded in physics while preserving a certain independence. In first approximation, physics provides the language, in which the specialized knowledge of engineering is formulated. However, much of this knowledge is independently established by eliminative induction.

Causality provides a suitable access point to many of the peculiarities of the engineering sciences. For example, one of the distinguishing features of technological artifacts is that they are associated with functions. The close relationship between the causal and the functional structure of artifacts can be understood if artifact functions are interpreted as the desired effects of causal processes connected with the artifact. Many of the characteristics of technical functions can be understood from this close relationship with causality including multiple realizability, multiple usability, the quasi-evolutionary development of artifact functions, and the fact that functions, like causal relations, can be considered on various levels of coarse-graining.

**Maria Kronfeldner**

***To Have an Effect of One's Own: Causal Complexity, Reconstituting the Phenomena, and Explanatory Values***

Causal complexity entails that an effect has many causes or a cause many effects. We can react to causal complexity in (at least) two ways: (1) by selectively focusing on particular causes, and (2) by dividing the phenomenon into parts that are more tractable. Both strategies conquer complexity by dividing either the explanans or the explanandum. As a result, we get a more simplified picture: effects that 'have a cause of their own' and causes that 'have an effect of their own'. Causal selection will largely be set aside in this talk. The focus will be on the second strategy, i.e. ways of reconstituting the phenomena. When we use it, we are guided by explanatory values, i.e. those epistemic values that we assume when we judge whether an explanation is a good explanation. But which values are these and how are they related? The talk of endophenotypes and norms of reactions in discussions about genetic causation are used as examples.

*Philosophy of Chemistry and Technology*

**Carsten Seck**
***Metaphysics within Chemical Physics: Case of ab initio Molecular Dynamics***

This paper combines naturalized metaphysics and a philosophical reflection on a recently evolving interdisciplinary branch of quantum chemistry, ab initio molecular dynamics (AIMD). Bridging the gaps among chemistry, physics, and computer science, this cutting-edge research field explores the structure and dynamics of complex molecular many-body systems through computer simulations. These simulations are allegedly crafted solely by the laws of fundamental physics, and are explicitly designed to capture nature as closely as possible.

At first sight, one could conceive of AIMD as an instrument of universal reduction that could be used to simulate higher-level properties at any scale range. Such an instrument would be a significant step towards implementing a type of universal reductionism because it would reveal impressive samples of how objects of the sciences are to be decomposed to interacting fundamental physical entities. This is, however, not supported by achievements in current quantum chemistry. A closer look shows that the models underlying simulations of molecular systems are built up of classical, quasi-classical and quantum mechanical components. As in many other computational sciences the limiting factor in quantum chemistry is the time needed to process an underlying model. Owing to the high dimensionality of the respective Schrödinger equation, it would simply be intractable to numerically calculate the time evolution of most molecular systems using only quantum mechanics. Thus, the leading idea of virtually all AIMD approaches is to treat the electronic problem by solving the Schrödinger equation to obtain the effective potential energy of the nuclei at each molecular dynamics step, whereas the motion of the nuclei is calculated through classical mechanics. All in all, the models and algorithms employed involve many approximations and significant degrees of idealization of their target systems.

Unfortunately, highlighting the modelling constraints imposed by the available computational power has become a common move to underpin an anti-realist stance. The core idea here is that we are forced to adopt instrumentalism because of the irreducible idealizations and approximations of the underlying models used in current computational sciences.

The main aim of this study is to show how and to what extent it is possible to be a realist even if one concedes, first, that current quantum chemistry yields no instrument of universal reduction and, second, the motley character of the models underlying particularly AIMD simulations.

First, I sketch a few different AIMD models in contrast to classical molecular dynamics. I show that the so-called extended Lagrangian approach of AIMD combines the advantages of the so-called Ehrenfest molecular dynamics and the Born-Oppenheimer molecular dynamics. Second, I examine recent strands in the debates on scientific realism. Finally, I offer a fair interpretation of such ab initio modelling in quantum chemistry within a naturalistic metaphysical framework that gives rise to a specific type of ontic structural realism.

**Alexandru Manafu**
***How Much Philosophy in the Philosophy of Chemistry?***

Philosophy of chemistry is aspiring to become a respectable field of philosophy of science. There are now two international journals in this field, several monographs, and annual international conferences and sessions at the meetings of the various philosophy of science associations, such as the PSA. A number of very good papers published in top journals can rightly be regarded as belonging to philosophy of chemistry. Yet, despite all this, the philosophy of chemistry is still being viewed as somewhat of a curiosity by many philosophers, including some philosophers of science. This paper argues that philosophers in general and philosophers of science in particular cannot afford maintaining this view. Moreover, there are excellent reasons why they should become more interested in this relatively young branch of philosophy of science.

How much philosophy is in the philosophy of chemistry? The answer is: much more than many philosophers would think! This paper argues that chemistry offers the best case studies for a number of traditional philosophical topics. As a first example, consider the topics of reductionism and emergence, which have been much debated in the philosophy of mind or biology. One can shine more light onto these murky topics if one takes chemistry as the paradigm case study. The fact that chemical properties can be inter-subjectively scrutinized, that they are amenable to a quantitative understanding, to measurement and experiment to a greater extent than those in psychology or biology (Scerri and McIntyre 1997, Humphreys 1997) justifies a more optimistic attitude (I will discuss in some detail a couple of such properties, and I will draw some general lessons from them). Chemistry is the discipline that is in some sense closest to physics, and therefore it is the first most fundamental domain outside physics itself where we should be able to observe irreducibility and emergence, if these truly exist.

Other classical topics in philosophy of science, such as scientific realism or scientific explanation, can also benefit greatly if one takes chemistry as a case study. More recent topics, such as idealization and modeling, can also receive exquisite treatments if one focuses on examples from chemistry. Last, but not least, the philosophy of chemistry offers the best case studies for more traditional topics in philosophy of science, such as theory change. The paper discusses in some detail what I consider to be some illuminating examples.

**Alfred Nordmann**
***How Much Philosophy of Technology in the Philosophy of Science?***

This presentation will consider the way in which complexity is generated in the field of Synthetic Biology. Though the field is ill-defined, there is general consensus (owing especially to the work of O'Malley) that one influential approach applies a software-engineering methodology to the problem of building up biological complexity e.g. to the construction of organismic structures from so-called bio-bricks. This software-engineering approach relies for its method on a design-cycle that consists of three steps (analysis-construction-evaluation) which are iterated until the construction satisfies performance-expectations.

The iterative process draws on extant knowledge and produces new knowledge. The presentation will show how the meaning of "analysis" changes in the course of iteration and how the knowledge that is incorporated into the design process differs from the knowledge that is generated by it. With reference to discussions of iteration and validation in Chang's "Inventing Temperature," the contrast of conceptions of analysis and of knowledge highlights what philosophy of science stands to gain by adopting insights from the philosophy of technology, especially where the scientific questions do not concern the reduction but the generation of complexity.

## *Induction*

**Ludwig Fahrbach**
***How to Confirm Theories without Considering Rival Theories***

Is it possible to confirm a theory by some observation without knowing or considering any concrete rival theories of the theory? Scientists certainly seem to think so. For example, when a theory makes a very precise prediction about the outcome of an experiment and that prediction turns out to be true, then scientists often judge the theory to be strongly confirmed by the correct prediction, even if they don't consider any concrete rival theories of the theory. However such confirmational judgments are a problem for standard Bayesian confirmation theory. Let $T$ be a theory and $D$ be some data. To deter-mine how well $T$ is confirmed by $D$ we need to know the likelihood $\Pr(D|\neg T)$. The problem is that $\neg T$ is a "catch-all", not a concrete theory. How can we find a value for $\Pr(D|\neg T)$ without dealing with the concrete rivals of $T$ that make up $\neg T$? In my talk I aim to provide a simple and idealized Bayesian model that aims to show that in two important kinds of cases, diversity of data and precision of data, it is often possible to find a reasonable estimate for the value of $\Pr(D|\neg T)$ without considering concrete rivals of $T$.

My Bayesian account is distinguished by two features: It uses odds of probabilities instead of probabilities themselves, and it focuses on orders of magnitude or powers of ten (for the reason that we are usually not interested in precise numbers, only in rough estimates). Probabilities and odds are then approximately related as follows:

| | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $\Pr(X)$ | … | .0001 | .001 | .01 | .1 | .5 | .9 | 99 | .999 | .9999 | … |
| $\Pr(X)$ $/\Pr(\neg X)$ | … | .0001 | .001 | .01 | .1 | 1 | 10 | 100 | 1000 | 10000 | … |

Note that typically the prior $\Pr(T)$ of a scientific theory is quite small, in the left hand side of the table, $\Pr(T)$ and $\Pr(T)\,/\Pr(\neg T)$ are approximately equal. We use Bayes's theorem in odds-form:

$$\frac{Pr(T|D)}{Pr(\neg T|D)} = \frac{\Pr(T)}{\Pr(\neg T)} \cdot \frac{Pr(D|T)}{Pr(D|\neg T)}$$

As an example let $\Pr(\mathrm{D}|\mathrm{T})/\Pr(\mathrm{D}|\neg\mathrm{T})$ equal 1000. Then the posterior odds of $T$ are three orders of magnitude bigger than the prior odds of $T$, i.e., three steps to the right in the table. Thus, if the prior odds equal $10^{-5}$ (or $10^{-4}$ or $10^{-3}$), the posterior odds equal $10^{-2}$ (or $10^{-1}$ or 1).

*Diversity of evidence.* Let $D_1$ and $D_2$ be two independent pieces of data, e.g., produced by com-pletely different kinds of experiments. We want to determine the epistemic impact of the conjunction $D_1 \wedge D_2$ on $T$. Assume that we don't know, or bother to consider, any rival theories of T. It is then natu-ral to interpret the independence of $D_1$ and $D_2$ as implying that $D_1$ and $D_2$ are probabilistically inde-pendent given $T$, and also probabilistically independent given $\neg T$. This provides a partial answer to the question how $\Pr(D|\neg T)$ can be determined without considering concrete rivals of $T$: If $D_1$ and $D_2$ are independent from each other, and we can somehow determine values of the likelihoods $\Pr(D_1|\neg T)$ and $\Pr(D_2|\neg T)$, then we can also get an estimate for the value of $\Pr(D_1 \wedge D_2|\neg T)$ namely $\Pr(D_1|\neg T) \cdot \Pr(D_2|\neg T)$. Then the likelihood odds of $D_1 \wedge D_2$ are given by:

$$\frac{Pr(D_1 \wedge D_2|T)}{Pr(D_1 \wedge D_2|\neg T)} = \frac{Pr(D_1|T)}{Pr(D_1|\neg T)} \cdot \frac{Pr(D_2|T)}{Pr(D_2|\neg T)}$$

*Precision of data*. Assume, for example, that data $D$ reports the result of a measurement which has a precision of 3 decimal places, and that $T$ predicts $D$, i.e., $\Pr(D|T)$ equals one. Assume again that we don't consider any rival theories of T. We can then reason as follows. On the assumption of $\neg T$, any possible measurement result looks like any other possible measurement result. There are 1000 of them. Hence, a reasonable estimate for $\Pr(D|\neg T)$ is $10^{-3}$. This is a version of the indifference principle, so this is the price to be paid for getting an estimate of $\Pr(D|\neg T)$ in this way. In my talk I discuss whether and in what cases this price is worth paying. For example, if the scale of the measurement is "natural", as is often the case, then the application of the indifference principle seems plausible.

Finally, if we are willing to pay this price, we do get a reward: The combined effects of diversity and precision can be very strong. For example, let the prior of $T$ be $10^{-10}$ , and assume that $T$ correctly predicts six independent pieces of data each with a precision of two decimal places. Then the odds of $T$ receive a boost of two orders of magnitude by each piece of data. Because of independence, the six boosts add up to an overall boost of 12 orders of magnitude resulting in a posterior for $T$ of .99. This shows how it is possible that probabilities of theories move from very small priors to very high posteriors, even if concrete rivals of $T$ are not considered.

**Michael Schippers**
***Coherence and (Likeness to) Truth***

Should coherence among theories be considered a scientific virtue in the sense that coherence implies verisimilitude? Are more coherent scientific theories more verisimilar? In this paper I will show that these questions, even if construed cautiously in a ceteris paribus sense, must be answered in the negative. To do so, I introduce a Bayesian framework for comparing scientific theories and establish an impossibility result to the effect that more coherence among theories does not invariably lead to more verisimilitude, ceteris paribus. This result reinforces the impossibility results from the field of Bayesian epistemology (Bovens & Hartmann 2003, Olsson 2005).

In a second step I will argue that in order to understand the utility of coherence in the context of theory assessment, we are well advised to change the focus from coherence among theories to a notion of coherence being a relation between scientific theories on the one hand and observational data on the other. In this regard, I show that the class of contrast coherence measures I introduce are a useful guide in order to assess the virtue of scientific theories. In this sense, theories that cohere better with the observational data are indeed more verisimilar. This latter notion of verisimilitude, however, is an epistemic notion relativized to a scientific community.

**Patrice Soom**
***On Metaphysical Analyses in the Philosophy of Neuroscience***

Over the last decades, the contemporary philosophy of science has progressively come closer to empirical, aiming more to describe accurately scientific practices and to analyze critically their epistemological standards than to conduct a critical analysis of their ontological commitments, the latter task constituting according to Papineau (1996) the project of the metaphysics of science.
The present contribution aims to illustrate how the absence of critical analysis of the metaphysics of scientific domains can undermine the project of providing an accurate description of their epistemological standards and of their scientific practices. A critical survey of the ongoing debate about mechanistic explanations in life and cognitive sciences shall illustrate the crucial role of metaphysical inquiries in meta-scientific analysis, especially in areas of science that are concerned with multi-level explanations.

The regulative idea of the so-called mechanistic framework (hereafter MF) is that neuroscience explains cognitive phenomena such as memory, attention, perception, and so on, by describing their underlying neurobiological mechanisms. According to the one of the most widespread account of mechanistic explanations, mechanisms are "entities and activities organized such that they exhibit the explanandum phenomenon" (Craver, 2007, p. 7). An important element of Craver's prominent version of MF is the criterion of mutual manipulability under ideal intervention, which enable to distinguish by means of bottom-up and top-down experiment, what are the constitutive components of mechanisms.

MF is often said to present the conclusive advantage over other approaches in philosophy of life and cognitive sciences of being ontologically neutral (see notably Craver, 2007, p. chapter 6), by-passing the question of the reducibility of higher-level phenomena and descriptions. The proposed contribution aims to show that, in spite of this optimism, MF is not neutral with respect to classical metaphysical issues and that it presents sever internal inconsistencies (Author 2012). The argument proceeds first by arguing that MF implies that cognitive phenomena supervene on their mechanistic basis. Therefore, Kim's famous supervenience argument (2005: chapt. 2) applies, with the result that MF is not ontologically neutral. This motivates considering levels of mechanisms as levels of description rather than ontic levels. Secondly, against the background of supervenience, Craver's criterion of mutual manipulability, which aims to distinguish between constitutive components and non-constitutive part of mechanisms, cannot fulfill the requirement manipulability under ideal intervention, because if higher-level phenomena supervene on lower-level mechanisms, then it is not possible to intervene on the latters without intervening by the same token on the formers (and vice-versa).

This result suggests that the current formulation of MF is not satisfactory and that it does not describe accurately current scientific practices and epistemological standards in neuroscience. This analysis reveals how metaphysical considerations can contribute to improve our analysis of scientific practices, especially in the context of inter-level scientific inquiries.

Please contact the author for references.

**Max Mergenthaler Canseco**
***Is Seeing Believing? The Role of Visualizations in the Cognitive Sciences***

Recently a lot of discussion has gone around the question of whether visualizations play a legitimate role in science. This questions seems particularly relevant to cognitive sciences, given the factual importance that visualizations play in that field. However, arguments departing from media theory (Mersch, 2006), history of science (Borck, 2009; Hagner and Borck, 2001) and neuroscience (Logothetis, 2008) question the validity of functional imagining (FI) in assessing cognitive states. From the other side, some philosophers take that only represenations that have a propositional structure, that is, represenations that can bear truth, can play a legitimate role in scientific explanations (Perini, 2005).

In this paper I will defend the thesis that visualizations in general, and functional imagining (FI) in particular, play a legitimate role in trying to explain the mind appealing to brain-states. I argue that the philosophical worry rises only if we understand scientific explanations as deductive-nomological arguments, and that it can be rebuted if we take the mechanistic approach to scientific explanations into consideration.

I claim that the mechanistic approach is superior by arguing that it offers an explanation on how new theories are generated and by showing how it accounts for the use of visualizations in the cognitive sciences. Theory generation is explained in two parts, first I introduce a Tool-To-Theories heuristic (Gigerenzer and Sturm, 2007), where tools can contribute to the forming of new concepts that can render a new understanding of a mechanism or motivate a new cognitive ontology to better investigate the human mind and its different cognitive functions. Secondly I show how FI can contribute to find the components that play a role in cognitive process by means of structural and functional decomposition (Bechtel and Richardson, 2010). Under decomposition I will understand the task of finding the important parts of a mechanism that bring a phenomena about.

In the next section I show how visualizations are epistemically useful and contribute to science in a valid way. I show

(a) how visualizations make use of space to convey more information simultaneously (Bechtel and Abrahamsen, 2005),

(b) offer relatively direct and iconic resources that contribute to the ease of search, pattern recognition and inference procedures,

(c) contribute significantly to the evaluation of the working fit between models and reality (Sargent, 1996),

(d) offer many levels of abstraction simultaneously (Kulvicki, 2010), and

(e) offer a valid tool (together with other tools) to study the human mind by means of forward and reverse inference. (Bogen, 2002; Craver, 2002; Klein, 2010; Piccinini and Craver, 2011; Poldrack, 2006).

Since the D-N model implies that only truth beares can play a legitimate role as premises in nomological arguments, I evaluate Perini (2005) attempt to give a Tarski-like truth definition for scientific visualizations. I follow, however, that this attempt only accounts for a small set of images, and show how it can't account for many other legitimate and important aspects of visualizations in the cognitive sciences.

Since the mentioned epistemic treats can't be accounted for in the D-N model I develop an argument for choosing the mechanistic account of explanations in science over its nomological adversary. The argument claims that visualizations are legitimate and epistemically important tools for cognitive science (points a-e), and that the D-N model implies that visualizations are non-legitimate and it can't account for their epistemic importance. Since the mechanistic approach is a valid alternative to the D-N model and it can account in a better way for the scientific practice we should prefer it.

After we assed the legitimacy of visualization and made ourselves clear about what these images can and can't do, the long lasting scientific enterprise of explaining the human mind using (between others) FI rests on theoretical safe ground.

**Iulian Toader**
***Against Weylean Skepticism***

My talk will focus on the relation between concept formation and the demand that scientific theories provide an objective and intelligible account of natural phenomena, that is, an account that justifies their mind-independent reality and, at the same time, renders them understandable.

More particularly, I consider the view of the mathematician and theoretical physicist Hermann Weyl, that this twofold demand cannot be satisfied, for it pulls science in opposite methodological directions, one driven by Husserl's pure phenomenology, the other by Hilbert's axiomatic formalism. According to Weyl, scientific understanding requires the phenomenological method of concept formation, i.e., that concepts be introduced by abstraction from experience, and that scientific reasoning be wholly contentual. Scientific objectivity, on his view, requires the method of formal axiomatics, that is, that concepts be freely introduced as mere symbols by stipulating, under certain constraints, fundamental theoretical principles, and also that scientific reasoning be partly non-contentual or purely symbolic.

This view, which I call *Weylean skepticism*, is important not only because it was suggested by one of the most influential scientists of the twentieth century, but also because it indicates how the tension that Weyl saw between objectivity and intelligibility might be dissolved.

The outline of my talk is the following. I criticize, first, the attempt at dissolving this tension by adopting a pure phenomenological approach to objectivity, which recently re-emerged in philosophy of science. On this approach, contentual reasoning is indispensable for objectivity, which entails, as Weyl already emphasized, that scientific concepts without contentual significance must be eliminated. I argue that Weyl realized that the phenomenological approach fails to account for objectivity, since it also entails the elimination of hypothetical elements, and so collapses into phenomenalism.

Secondly, I analyze Weyl's formal axiomatic approach to objectivity, and examine the requirement of categoricity, i.e., that a scientific theory, as a system of symbols, may provide objective knowledge only if its contentual interpretation is univocal up to isomorphism. I argue, on the one hand, that this requirement fails to be satisfied in quantum physics, and that recent attempts at addressing this failure render theories unable to account for natural phenomena that they were designed to account for. On the other hand, I suggest a way of thinking of objectivity without categoricity.

Finally, I submit that the alleged tension between objectivity and intelligibility could be dissolved through a formal axiomatic approach to understanding. Against Weylean skepticism, I argue that the conditions under which purely symbolic reasoning may render natural phenomena understandable are at last partly expressed by the notion of epistemic control. This obtains if one shows, by contentual reasoning, that the deviation from actual observations of results obtained through purely symbolic reasoning is smaller than experimental error.

**Krystyna Bielecka**
***Explaining Behavior with Representations***

Schulte (2011) wanted to show “how some “clash of intuitions” in the debate about teleosemantics can be overcome by combining philosophical argument with careful reflection on the empirical facts”. I will claim that what lacks in his view is combining the empirical facts in a way that is interesting for the philosophy of science. This means understanding how scientists, such as ethologists, use the concept of representation and what they explain with it. Contra Schulte, only a concept of representation that is useful in explanation of behavior can be vindicated. Only such a concept couldn’t be easily criticized by antirepresentationists who can argue that a concept that does nothing in explanation is epiphenomenal (as Chemero 2000 or Garzón & Rodriguez 2009). Moreover, showing a distinctive role of representation in explaining behavior answers the “job description challenge” posed by Ramsey (2007). Importantly, the traditional focus of philosophy of mind and language on content determination relations does not help to answer this challenge. For this, we must turn to philosophy of science.

Schulte argues against Millikan’s teleosemantics, responding to her functional interpretation of what is frog’s representational content. He argues against Millikan’s view because it yields a content ascription that does not include important perceptual properties (being small, dark and moving) and includes irrelevant functional properties (being frog food). Schulte questions the validity of causal-functional explanation in case of frogs by appealing to empirical facts discovered and interpreted by cognitive ethologists and states that a frog is too simple organism to have cognitive capacities that would enable it to recognize flies as its food. At the same time, he accuses Millikan of underestimating the role of perceptual input and surface properties of a fly to which frogs are distinctively sensitive (triggered by size-distance constancy mechanism). Schulte claims that only distinctive perceptual properties adding frog’s motivation toward an object (a hunger) is required for scientific explanation of frog’s representational content.

I will argue that Schulte’s line of argument ignores the requirements of a satisfactory explanation of frog’s cognitive behavior. The notion of representation he implies is therefore exposed to antirepresentationalist objections. Schulte does not specify any distinctive role of representation in behavior because his theory is framed in terms of narrow perceptual properties that are only triggering a snapping reaction. Positing a representation in the frog over and above perceptual properties is against parsimony considerations, and the notion of representation equivalent, roughly, to perceptual properties causally relevant to behavior, is trivialized (Ramsey 2007).

**Michael Poznic**

***Five Ultimate Arguments against Similarity Views of Scientific Representation?***

The notion of scientific representation plays a central role in current debates on modeling in the sciences. The major virtue of most successful models is their capacity to adequately represent specific phenomena, so-called target systems. Models are often studied in order to learn something about complex and inaccessible real-world targets. In these cases, one would say that models have a representational function. The overall aim of modeling in the sciences is to gain epistemic benefits; and in order to reach this aim, models should have an epistemic function as well. This epistemic function seems only to be fulfilled by models or other representational vehicles that are in some appropriate way connected to reality. It almost seems natural to ask, how can scientists gain knowledge of the natural world by studying models if these models do not resemble certain target systems? Notions of resemblance or similarity appear relevant when explaining why scientific representations can be used to foster knowledge of certain phenomena.

In his 2003 paper "Scientific representation: against similarity and isomorphism", Mauricio Suárez argues forcefully against similarity views of scientific representation. In the course of the paper, he delivers five arguments that he regards as touchstones for satisfactory theories of representation in the sciences. Suárez characterizes his opponents by endorsing a naive view of representation, namely the slogan: A represents B if and only if A is similar to B. In fact, Nelson Goodman even calls an almost identical thesis concerning pictorial representation the most naive view of representation and, in Languages of Art, he already argues against such similarity views of representation.

My key question in this paper is: are Suárez‘ arguments really effective against a non-naive view of scientific representation? In order to evaluate his arguments, I will critically analyze them and propose a new ordering. First, there are arguments against similarity as a sufficient condition for representation. Second, some arguments are directed against similarity as a necessary condition. And finally, there is one argument against the naive view, according to which this view cannot account for the various means of representation, where the means is understood as the relation between a model and a target that scientists actually employ.

The conclusion will be that similarity is indeed not sufficient for representation. Yet, similarity can be conceptualized as a necessary condition for scientific representation, granted that there are relevant respects and specific degrees of similarity. Furthermore, the arguments of the various means can be answered successfully as well. Thus, a possible non-naive slogan of a tenable view of scientific representation could be formulated as follows: only if a vehicle is similar to a target then the vehicle is an adequate scientific representation of that target.

**Maria Serban**

***A Place for Contextualism in Philosophy of Science***

One of the main attractions of the semantic view of scientific theories is its promise to deliver a uniform picture of successful scientific representation (Suppes 1957; van Fraassen 1980; da Costa and French 1990). More specifically, the semantic or model-theoretic approach characterizes theories in terms of classes of set-theoretical models. Defenders of the semantic view claim that this approach is better suited to characterize scientific practice than the syntactic view which conceives of theories in terms of lists of linguistic statements, while maintaining some degree of formalization and thereby rigour and clarity.

Seeking to accommodate as many of the features of scientific practice as possible, several advocates of the semantic view have proposed to extend the initial framework by including the notion of partial structure, as well as that of partial isomorphism and homomorphism holding between such structures (French and Ladyman 1997; Bueno, French and Ladyman 2002, 2012).

Despite the fact that prima facie partial structures seem to be well poised to account for the openness and complexity of scientific theorizing, the account has not remained uncontested. One of the main objections raised against the partial structures framework is that it fails to account for the distinct contributions that mathematics makes to successful scientific representations. Otherwise put, the partial structures account is charged with ignoring important implications of the wide variety of mathematical models used by scientists because of its uniformist demands on the philosophical image of the scientific enterprise (Suarez and Cartwright 2008; Batterman 2010).

On some versions of this argument, mathematics is said to make a significant epistemic contribution to successful science, and that in order to characterize this contribution the philosopher of science needs to take into account the content of scientists' beliefs, goals, and intentions (Pincock 2011). This latter line of criticism is usually characterized in terms of a contextualist, agent-dependent approach to debates concerning the nature of scientific representation and theories.

The main aim of this paper is to assess the legitimacy of this contextualist strategy within philosophy of science in general. I propose to analyze the answer that the ontic structuralist makes to the contextualist objection. By identifying the limits of this line of reply, I seek to characterize more precisely what are the contextualist demands from a philosophical picture of the scientific enterprise.

I claim that the contextualist urges a shift within philosophy of science itself from the more traditional debates concerning the relationship between mathematical models and their physical target systems towards debates which focus on the relationship between mathematical models and their users. This proposal can be understood in terms of a transformation of the role that philosophy has to play with respect to past and current science. Namely, from the foundational role that philosophy has traditionally assumed to a collaborative role (Chang 2004), whereby the philosopher contributes to the articulation of the models and theories that scientists deal with. In concluding, I argue that this repositioning of philosophy of science with respect to scientific theorizing sheds light on the question of the legitimacy of contextualist strategies within philosophy of science in general.

## *Natural Laws*

**Giulia Pravato**

### ***Natural Laws and Social Conventions. Exceptions as a Case Study***

Regularities, roughly understood as constant (or frequent) conjunctions of properties or events, are ubiquitous both in nature and in society. Where do these regularities come from? Some seem due to mere happenstance: accidental regularities ('All gold cubes are smaller than one cubic mile') and statistically frequent actions, i.e. mere convergent habits of behaviour ('going to the cinema on Saturday nights'). Others have a 'modal character' in that, at least prima facie, they seem to govern or guide the events in the world: philosophers talk of law-like regularities that couldn't fail to obtain, viz. laws ('All copper conducts electricity') and of rule-like regularities that guide our actions, viz. norms or conventions ('If you write in German, capitalize all nouns', 'If you drive in England, keep to the left-hand side of the road'). Philosophers of science (natural or social) have long struggled to spell out the intuitive difference between events that just happen and events that, in a certain sense, are constrained or guided. However, philosophers have generally accepted a familiar and plausible story about what distinguishes empirical laws from legal or moral laws as well as from other kinds of rules: while the latter may be broken without losing their status as laws or rules, an alleged violation or exception to the former counts as a falsification of the law (see, for example, Hart 1961 and Frege 1956). If a body doesn't behave according to Newton's laws, this means that our laws need to be revised; on the other hand, if someone doesn't keep a promise it is the violator and not the moral system that is at fault. This distinction is supported by the view that 'whatever else a law may be, it is at least an exceptionless regularity' (Lewis 1986, p. 45). This view, though, is nowadays highly contested: it is claimed that many, if not all, laws do have exceptions (see, for example, Cartwright 1983 and 1999 and Pietroski and Rey, 1995).

In this paper I consider the topic of of exceptions and violations in relation to both social conventions and natural laws and I defend two related claims. First, I argue that it is less easy to find genuine exceptions to laws than a quick glimpse at the ceteris paribus literature would suggest and that 'ceteris paribus' picks out at least two different things: in some cases, the conditions of application of a law are preliminarily restricted to an ideal model or set-up and, accordingly, there is no room for exceptions; in others, the conditions of application cannot be fully spelt out in advance because context-sensitive, or epistemologically unknown or yet metaphysically indeterminate. Second, I argue that there is no straightforward route from alleged exception-ridden laws – so called ceteris paribus laws – to an anti-realist stance on laws. To be fair there are two disambiguations of 'convention' according to which laws might turn out to be 'partially conventional' or endowed with a 'regulative force': but while one covers quite a loose sense, the other only shows – if endorsed – that all exceptions talk is wrong-headed in the first place.

Please contact the author for references.

**Matthias Unterhuber**

***Less Lazy than One Might Think – Ceteris Paribus Conditions in the Context of Lewis' Best System Analysis***

Many philosophers of science agree that prima facie laws in the special sciences (biology, chemistry, etc.) involve ceteris paribus (cp) conditions of one sort or another. It is, furthermore, rather uncontroversial that cp laws might result from one's epistemically limited perspective (or due to one's laziness) by not being able (or choosing not) to strengthen the law in question to an exception-less law. In constrast, much more controversial is the question whether there are genuine laws of nature which in principle cannot be strengthened to exception-less laws, where exception-less laws have the form $\forall x(Fx \rightarrow Gx)$ ('all Fs are Gs') from classical logic such that $\rightarrow$ is the material implication.

In my talk I will argue that the classical construction of laws of nature (as described above) is in principle misguided and that rather non-classical quantified conditional structures such as $\forall x(Fx \mathrel{\Box\!\!\rightarrow} Gx)$ ('the relative frequency of Gs among Fs is high') or P$(Gx|Fx)$ is high ('most Fs are Gs') as, for example, described in Pelletier (1997) and Schurz (2005) are called for, even under perfect knowledge of all facts. For that purpose I investigate why we might need ceteris paribus conditions also in epistemically ideal circumstances, as described by Lewis' (1994, 1983) Best System Analysis (BSA) of laws of nature. I will argue that in Lewis' BSA (i) the supposition of perfectly natural properties and (ii) the criterion of simplicity – when extended to the logical form of laws of nature – will speak in favor of an analysis of laws in terms of genuine ceteris paribus conditions, as described by quantified conditional structures. I will in particular show that under appropriate conditions a reconstruction of laws of nature in terms of quantified conditional structures is simpler than an equally strong as a classical reconstruction of laws of nature. Furthermore, when we in addition assume the existence of perfectly natural alien properties (as done by Lewis (1983)) – properties which are not instantiated our world – a reconstruction of laws of nature in terms of quantified conditional structures can avoid counter-intuitive consequences compared to a classical construction of laws of nature. I will, then, discuss which interpretation of quantified conditional structures is best warranted in the context of Lewis' BSA. My discussion will, hence, show that there are reasons to suppose that the use of cp conditions for laws of nature is less lazy and more substantial than one might think.

Please contact the author for references.

**Andreas Hüttemann**
***In Laws We Trust***

„Humean Supervenience is named in honor of the great denier of necessary connections. It is the doctrine that all there is to the world is a vast mosaic of local matters of particular fact, just one little thing and then another. ... For short we have an arrangement of qualities. And that is all." (Lewis 1986, ix-x)

In this paper I will argue (1) that the role many laws (or law-statements) play in scientific practice and in the application of science cannot be understood on the basis of humean supervenience. (2) I will make a suggestion of how to understand the necessary connection needs to be assumed to obtain in nature.

Ad (1): My starting point is that law-statements are not only used for the description of how systems actually behave. They are also used for considering possible interventions. Manipulation and policy-making relies on laws. They tell us what is impossible (perpetuum mobile, travelling faster than light). They also tell us what must happen, provided the right condition obtain. It is for this reason that we spend money on institutions that test e.g.

− Whether a certain drug will cure pneumonia without giving rise to unwanted side-effects.

If everything were „entirely loose and separate", as a Humean has it, anything might happen. There would be no reason to prefer a tested drug to an untested drug. We could not trust in laws. Similarly, if there are no modal connections in nature we have no reason to suppose that it is impossible to build a perpetuum mobile as opposed: up to now nobody happened to built one.

I will argue that the "ersatz"-modality that the Humean relies on, in order to account for these practices is insufficient.

Ad (2): What kind of necessity-relation has to be assumed in order to understand the above-mentioned practices. Postulating a relation of 'nomic necessity' runs into well-known problems that have been extensively discussed (e.g. by van Fraassen 1989 (problem of identification/problem of inference)). Scientific essentialists (e.g. Ellis 2001, Bird 2007) claim that laws are metaphysically necessary. This, however, appears to be incompatible with the observation that the (dynamic) behaviour that laws predict sometimes fails to become manifest (Schrenk 2010).

I will suggest that the notion of conditional metaphysical necessity might provide a solution. More particularly I will defend the view that – provided certain conditions obtain, e.g. the absence of interfering factors – it is metaphysically necessary that what the law says does occur. Such an account has several advantages: First, because we are dealing with metaphysical necessity, the inference and identification problems do not arise. Second, the 'appearance of contingency' that is often attributed to laws can be explained in term of the fact that the laws are only necessary provided certain conditions obtain. For this account to work it is essential that more can be said on the conditions that the necessity relation is relativized to. Laws of composition will play an essential role to deal with these conditions.

## *Philosophy of Biology*

**Shunkichi Matsumoto**
***Evolutionary Functional Analysis Revisited***

Evolutionary psychology shares its raison d'être with human sociobiology, in that it purports to naturalize human behaviors, human psychology, and human nature by providing evolutionary accounts of each. It tries to differentiate it from its forerunner, however, in order to emphasize its methodological advantages: the differentiation of evolutionary psychology as a genuinely scientific practice from sociobiology as a pseudo-science. This is now a fairly pervading conception and even critics like David Buller (2005) seem to endorse it. This paper aims to show that it is not the case. To this end, the logic of 'evolutionary functional analysis,' the methodology distinctive of evolutionary psychology, is analyzed in detail, conceptually and text-critically, and the conclusion is drawn that this methodology is not up to the expected task for the successful differentiation.

There are no less speculative elements in evolutionary psychology than in sociobiology, while at the same time there are no less empirically testable elements in sociobiology than in evolutionary psychology. As, for sociobiologists, it is not a predetermined fact that the traits they are witnessing are adaptations, so, for evolutionary psychologists, it is not a given fact whether the behavioral patterns or psychological dispositions exhibited by modern humans are functioning adaptations, currently malfunctioning adaptations, or just historical byproducts. In each case, therefore, researchers have to begin with constructing a historical scenario which bridges between what they see now and what is presumed to be its evolutionary origin. Whether this is done backwardly or forwardly does not make a significant difference because the essential part of each type of reasoning boils down to the same conditional: ‘If the selection pressure in those days was such and such, then this or that kind of characteristics would be favored by natural selection.’

We further argue that although evolutionary psychologists formally conduct their reasoning from the ancient past down to the present, they actually make shortcuts, by taking advantage of what they already know in order to posit the relevant modules and, at the same time, by projecting such knowledge onto our ancestors for making pertinent conjectures regarding their challenges. This means evolutionary psychologists do not necessarily theoretically predict the relevant mental modules for particular psychological capacities but rather only posit them as placeholders for what they observe now, thus making the detour all the way to the Pleistocene more or less redundant.

**Predrag Šustar and Zdenka Brzović**
***The Function Debate in the Light of Molecular Approaches to Evolutionary Biology: The Case of Neo-Functionalization***

The philosophical debate on functions in biology and related scientific areas can be divided into two large opposing groups of accounts: etiological theories of function and causal role theories of function. Etiological theories adopt a historical approach to functions; a trait has a function if the effects of that trait in the past contributed to the selection of organisms with that trait. On the other hand, according to the causal role theories, functions are not effects that explain why a trait is there, but rather the properties of an organism that contribute to the more complex capacities of the system that contains them. Cummings (2002), one of the main proponents of the causal role theory of biological functions, argues that a correctly understood neo-Darwinian notion of natural selection has nothing to do with functional talk in biology. This is because selection requires variation, and there is standardly no variation in function on which selection could operate, but only variation in how well the function is performed. According to Cummins, there is an exception to this – there are cases in which the target of selection is also the bearer of a function that accounts for the selection of that trait, that is, cases where we have an introduction of a genuine functional novelty. However, he holds that these cases are very rare and cannot account for the functions of complex biological traits.

In this paper we will focus on the phenomenon of neo-functionalization which presents a case of the introduction of a genuine functional novelty for which even Cummins admits that goes in favor of an etiological account of functions. Our aim is to show that a progress in the molecular approaches to evolutionary biology – specifically the scientific data available in the neo-funcionalization research offers valuable support to a kind of etiological selectionist program of functions in biological and biologically-related sciences. We will examine the two main theories

of neo-functionalization: the theory of neo-functionalitzation via gene duplication and the theory of neo-functionalization via gene sharing. We will argue that these theories suggest that Cummins' arguments about the introduction of functional novelties are not convincing. Firstly, the occurence of novel functions is a quite common phenomenon. Secondly, we will show that a novel function established even at the molecular level can bring forth a new, salient, function for highly complex multi-cellular individual organisms.
Finally, we will use the presented data to build up our own account of biological functions which tries to avoid the wrong turns taken by both major strands in the biological function debate. According to this account, the function of a certain gene or a protein in the biological system that contains it is a particular causal activity, or a group of causal activities whose manifestation is in a specific way determined by corresponding mechanisms' genetic expression and this particular expression of genetic activity was positively selected at a certain point in evolutionary history.

**Stavros Ioannidis**
***Development and Evolutionary Causation***

According to Neo-Darwinists, natural selection (NS) is the main cause of evolution. The primacy of NS is the source of the dissatisfaction of neo-Darwinists with the views of biologists working in Evo-devo (evolutionary developmental biology). According to the latter, the structure of ontogeny can have an important causal influence on the evolutionary process. The standard neo-Darwinist response is that, to the extent that development can be a cause of evolution at all, its significance is secondary to that of NS.

The aim of this paper is to argue that, against neo-Darwinism, development can be as important as selection in determining the evolutionary process. My argument has two parts. I begin by examining the neo-Darwinist argument in favour of the primacy of NS. We can distinguish between two versions of the argument: first, NS is more important because it is the only evolutionary cause that leads to adaptive complexity; second, NS is more important because it is the only directing cause in evolution (i.e. explains the direction of evolutionary change). I argue against the first version, because it arbitrarily focuses on a specific phenomenon disregarding others, as well as against the second one, since, as I show in part 2, development can also be a directing cause of evolution.

In the second part of the paper I show how recent research in Evo-devo can be used to demostrate that development can be a directing cause. I focus especially on S.B.Carroll's regulatory evolution hypothesis, according to which regulatory mutations underlie most evolutionary change, since regulatory mutations are more probable to be adaptive than structural ones. Carroll's hypothesis suggests two ways in which development can be a directing cause: first, if Carroll is correct, there are some specific respects in which organisms are more likely to change than others, and so phenotypic variation in a population and across evolutionary time will have a specific pattern: it will reflect the phenotypic changes of regulatory mutations. Secondly, there could be a relation between evolutionary change and the rate of regulatory mutations.

Lastly, I examine a stronger version of the current argument, according to which development and the structure of the genetic regulatory networks underlying it constitute the creative factor of evolution, with NS playing merely a negative role, i.e. acting as a filter for the least adaptive phenotypes. I reject this stronger version, because I argue that the distinction between negative and positive elements of causation cannot be justified. I present two arguments in favour of this: first, NS can also be creative, in the same sense that the underlying developmental mechanisms are. Secondly, on the most plausible way of distinguishing between the relative contribution of different causes, it is not the case that development is always positive and NS always negative. In most cases, the two causes interact in complex ways, and to distinguish between positive and negative causes would not make any sense.

## *Mechanisms II*

**Alexander Gebharter**
***A Formal Framework for Representing Mechanisms?***

In this paper I tackle the question of how mechanisms can be represented within a causal graph framework. I begin with a few words on mechanisms and some of their characteristic properties. I then concentrate on how one of these characteristic properties, viz. the hierarchic order of mechanisms (mechanisms frequently consist of several submechanisms), can be represented within a causal graph framework. I illustrate an answer to this question proposed by Casini et al. (2011) and demonstrate on an example that their formalism, though nicely capturing the hierarchic order of mechanisms, does not support two important properties of nested mechanisms: (i) The more of the structure of a mechanism's submechanisms is uncovered, the more accurate the predictions of the phenomena this mechanism brings about will be, and (ii) a mechanism's submechanisms are typically causally interacting with other parts of said mechanism. Finally, I sketch an alternative approach capable of taking properties (i) and (ii) into account and demonstrate this on the above-mentioned exemplary mechanism.

**Tobias A. Huber**

***Mechanisms and Mechanistic Explanations***

Mechanistic explanations have been recently developed by William Bechtel, Peter Machamer, Lindley Darden, Carl Craver and others in order to characterize explanations in neuroscience and the life sciences. This paper explores what a mechanistic approach can tell us about explanatory models in contemporary cognitive neuroscience. Mechanisms are temporally, spatially, and hierarchically distributed working parts that, under the right background conditions, produce the phenomenon to be explained. Machamer et al., for example, write that to "give a description of a phenomenon is to explain that phenomenon, i.e., to explain how it was produced" (Machamer et al. 2001: p. 3). Mechanisms are described by Machamer et al. as "entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions" (p. 3). This paper will examine as a historical case study the memory research program which exemplifies the way in which mechanisms are identified, that is, as described by Bechtel (2008), in terms of decomposition and localization. Mechanistic explanations require the decomposition of the mechanism, which produces the phenomenon to be explained, into its components and into its operations. In describing the strategies of localizing and decomposing the components of the explanandum phenomenon "memory", this paper investigates the different explanatory models of memory and the scientific identification of mechanisms for memory. It will assess the fragmentation of the memory-concept into different memory systems, such as Tulving's (1972) proposal to distinguish between episodic and semantic memory and the following taxonomies of memory systems. It will be argued that explanations in neuroscience describe mechanisms. The core requirement on mechanistic explanations is that they must account for the phenomenon to be explained. Craver (2007) provides a set of criteria to evaluate scientific identifications of the explanandum phenomenon in question. He describes three ways of how scientific explanations can fail: underspecification, taxonomical error, and misidentification (Craver: 124-128). Taxonomical errors occur, for example, when it is assumed that distinct phenomena are one, or, conversely, when it is incorrectly posited that one phenomenon is many. The failure to account for the multifaceted character of the phenomenon results in leaving the phenomenon underspecified, so that it becomes difficult for different research groups to agree upon the definition of the phenomenon under investigation. In case of misidentifications, the phenomenon cannot be explained because it simply does not exist. As I will argue, the mechanistic framework is both descriptive and normative. These failures of description in scientific practice provide normative criteria to assess the explanatory success of scientific explanations. As I will show on the basis of the historical case study of the memory research program, the ways of decomposition and localization of components and organization of the mechanism in question provide criteria to evaluate explanations in cognitive neuroscience in general. It will be argued that these criteria can be applied to other cognitive phenomena and functions in order to assess, for example, the explanatory status of the proposed mechanisms of consciousness and the scientific value of concepts such as “consciousness.”

Please contact the author for references.

**Elizabeth Irvine**

***Mechanisms, Natural Kinds, and the Boundaries of Cognition***

This talk will focus on the way that ideas from philosophy of science may have serious implications for constitutive questions posed in philosophy of mind and cognitive science. The example considered here is the debate over whether cognitive processes only occur in the head, or whether they are (or can be) extended into non-neural bodily and environmental processes. I suggest that taking inspiration from philosophy of biology and neuroscience, we need not assume that constitutive questions have clear-cut answers, both in scientific practice, and in metaphysical terms too.

Clark and Chalmers (1998) suggested that cognition need not be limited to the processes that go on inside the skull. Instead, parts of cognitive processes could extend into external objects, such as external objects manipulated to solve spatial problems. Various criteria have been offered for what should count as ‘cognitive extension’, but there is ongoing debate about their adequacy.

However, as recently suggested by Kaplan (2012), if the debate comes down to identifying constitutive parts of a process, a problem often faced in science, then philosophy of science could help. Craver (2007a, 2007b) has outlined how mechanisms are experimentally demarcated (i.e. how they are constituted), using the notion of mutual manipulability. Kaplan suggests that this generic way of identifying constitutive parts of a mechanism provides an empirical way of settling disputes about cognitive extension. While encouraging the use of ideas from philosophy of science, I suggest that, properly applied, they do not provide the resolution that Kaplan claims.

For example, as Craver himself has argued (2009), mechanisms and their demarcation cannot be used to identify natural kinds or context-independent constitutive parts of a mechanism. To be sure, there are cases where it ‘makes sense’ to include external objects as a constitutive part of a particular mechanism/cognitive process. But what counts as being constitutively relevant for a mechanism in one research context may not count in another; empirical studies will not be able to establish *the* boundaries of a cognitive process, but only specify a reasonable way to carve up a system given a specific set of research goals.

Further, related work in philosophy of biology suggests that the plurality of boundaries of a system in epistemic terms also translates into there being multiple ways of carving the same process up into ‘real’ ontological chunks. Ontological pluralism, accepting that there are multiple and equally viable ways of carving up the same bit of reality into ontological types or kinds, is one that naturally arises from the consideration of scientific methods (see e.g. Boyd, 1999; Wilson, 2005; Craver, 2009; Dupré, 1993).

This means that there is no fact of the matter about whether or not a cognitive process is extended. For some purposes it may be, and for others not, and this plurality also holds at the metaphysical level. Philosophy of science can reshape debates about constitutive questions in philosophy of mind and cognitive science, though it may do so in a rather radical way.

*Experiments*

**Johannes Lenhard**
***Shifting Balance. Experiments, Computers, and Simulations***

Simulation modeling seems to involve a particular kind of experiment - numerical or simulation experiments. The status of these experiments is a controversial topic in philosophy of science. This talk will not focus on the nature of these experiments, but on the relationship between simulation experiments, simulation modeling, and laboratory experiments. The relation between them is not fixed: Computing technology and simulation modeling have been developing in close connection. Over the course of their evolution, the conception of computational modeling has changed and the relationship between simulation and experiment has taken on different characteristics.

Computational quantum chemistry presents a telling case because it took a path that reflects the changing relationship between simulation and experiment. Three phases of its development will be discerned. It started out, prior to digital computers, from the Schrödinger equation, and turned, based on the use of mainframe computers, into an established sub-discipline of chemistry. The most recent third phase is marked by a steep rise in the distribution and application of computational chemistry. All three phases differ in the conceptions of computational modeling, in the computing technologies they use, and in the relationship between simulation and experiment. It will be argued that the recent success in applications is indeed based on the way experiments are incorporated into computational modeling.

**Lena Hofer**
***(Re)Production of Empirical Scenarios***

The general aim consists in developing a conceptual framework for analyzing how it is possible that conceptually under-determined units of empirical research still count as being reproduced. Realizing this project may lead to a better understanding how semantic entities like concepts and theories arise and develop.

My talk consists of four parts. The aim of the first part is to discuss the question if reproducibility should be demanded from every empirical scenario. I use the latter term as a generic name for pieces of research that lead to new insights, e. g. experiments, observations or the like. The answer to the question whether reproducibility should be demanded from all of them depends on what is understood by “reproducibility”. One possible interpretation is to view reproducibility as aimed repeatability. I show by examples that this strategy excludes many kinds of scientific observation, so that the criterion loses its general applicability. This consequence, however, is unsatisfying since the criterion of reproducibility was never meant to be restricted to particular research scenarios. In order to accommodate this intuition, in the second part of the talk I introduce a complex concept of reproducibility, which distinguishes four grades: (1) traceability (by independent scientists), (2) multiple registration of one and the same finding (by independent scientists), (3) multiple registration of different findings of the same kind (by independent scientists), and (4) multiple production (by independent scientists).

The challenge of the third part is to expound the conceptual basis for the notion of empirical scenario. The term is defined as a type notion for empirical arrangements, which are introduced in turn as the token units of scientific research. Empirical arrangements are discussed and characterized by identifying their central components: measuring instruments, test objects, results, and methods.

In the fourth part, I motivate further research on the identification of general mechanisms of reproduction within empirical science. Special attention is given to fields of research which are not yet sufficiently well described by scientific theories. Lacking established empirical theories, the reproduction of empirical scenarios may develop into a central research problem within theses areas. So the question arises, how it is possible that conceptually under-determined empirical scenarios still count as being reproduced.

A theory of mechanisms of reproduction can be developed within a holistic semantics by using formal-semantic methods. In this talk, however, I close with diagrammatically presenting and discussing two examples of general mechanisms of reproduction: the matrix-mechanism and the self-reproducing-mechanism.

**Jan Sprenger**

***The Interpretation of Sequential Trials in Medicine. A Plea for Conditional Reasoning***

Clinical trials in medicine are often conducted as *sequential trials*, that is, as trials where the  ample size is not fixed in advance, but depends on the results to be observed. This is primarily due to the ethical issues involved in evidence-based medical research. Trials have to be stopped as soon as overwhelming evidence for the superiority of the tested treatment comes  in, or as soon as there is evidence that this treatment is harmful to the patients.

However, the proper interpretation of sequential trials is a tricky issue. Should the chosen sampling plan have any effect on the conclusions that one draws? From a frequentist point of view (e.g., Mayo 1996), the answer is yes. After all, frequentist measures of evidence such as (unconditional) error probabilities depend by definition on the sampling plan. Moreover, it has been argued that by neglecting the sampling plan, a malicious experimenter could “reason to a foregone conclusion” (Mayo and Kruse 2001). For example, if after every step, we check whether a significant result has been achieved, we will finally reject the null hypothesis (=no treatment effect) with probability one, even if there is no effect.

However, unforeseen developments often demand that the original sampling plan be violated, making impossible a strict frequentist interpretation of the results. In fact, this feature of frequentist methodology is currently perceived as a problem for the reliability of sequential trials stopped early (Montori et al. 2005). Whereas for a Bayesian, a sampling plan is just an intention of an experimenter, and does not affect the post-experimental evidential conclusions. Bayesian measures of evidence are invariant under different sampling plans. This is a consequence of the Conditionality Principle (Birnbaum 1962). It has therefore been argued that switching to Bayesian reasoning would improve the post-experimental assessments of sequential trials, including sensitive issues such as overestimation of treatment effects (Goodman 2007).

On the other hand, frequentist reasoning is at present dominant in the evaluation of clinical trials and unlikely to disappear from practice. Moreover, most practitioners are not used to Bayesian reasoning. Therefore, I would like to promote a proposal that allows for staying within a broadly frequentist framework while taking into account some lessons from Bayesian statistics. This is the *conditional frequentist approach* (Berger 2003): to calculate error probabilities *after* conditioning on the observed results, instead of using the unconditional error probabilities that frequentist orthodoxy (and some philosophers of statistics such as Deborah Mayo) recommend.

It will be argued that conditional frequentist measures of evidence improve, from an epistemic point of view, on their unconditional counterparts, e.g. because they are sensitive to the exact strength of the evidence contained in the data and less vulnerable to the problem of overestimation. At the same time, they allow for an intuitive interpretation of the results since trials stopped early will generally yield higher (conditional) error rates than those conducted to completion.

This research is based on joint work with Cecilia Nardini, University of Milan.

Please contact the author for references.

# *Causality*

**Simon Friederich**

## ***Local Causality in the Light of the Principal Principle***

There is a long-standing debate as to whether there is a fundamental tension between quantum theory and relativity theory. Bell's theorem is one of the main motivations for believing that there is such a tension. It implies that correlations predicted by quantum theory are incompatible with constraints on probabilities in theories which, according to Bell, conform to the principle of local causality: the idea that "direct causes (and effects) of events are near by, and even the indirect causes (and effects) are no further away than permitted by the velocity of light." ([Bell 2004] p. 239)

Bell regards this principle as "not yet sufficiently sharp and clean for mathematics" ([Bell 2004] p. 240) and spells it out in probabilistic terms to apply it to theories that make probabilistic predictions. His resulting criterion of local causality ("(BLC)" for "Bellian local causality") says that, for events A and B in space-like separated space-time regions 1 and 2, the probability P(A) must be fully determined by a complete specification E of what happens in the backward light cone of 1 (where A potentially occurs) in the sense that whether or not B at space-like distance from 1 occurs is irrelevant for P(A) in that $P(A|E) = P(A|EB)$.

I will show that this criterion (BLC) is inadequate as a formulation of local causality, given the following two assumptions:

(1) Objective probability ("chance") can only be what imposes constraints on rational credences (see Lewis' Principal Principle, [Lewis 1986]).

(2) An agent located in a space-time region 1 can have evidence about events in a different space-time region 2 only if there are causal influences from 2 to 1. (proviso needed here, which I neglect for the sake of brevity)

In a nutshell, the argument from these assumptions against (BLC) goes as follows:

In a world described by a locally causal theory, an agent located in a space-time region 1 cannot have any evidence about an event B in region 2 at space-like separation. For, according to (2), having such evidence would require the existence of superluminal causal influences from 2 to 1. Therefore, it cannot be rationally required for an agent in region 1 to take into account B when forming her credence about A. In the language of the Principal Principle, evidence about B in region 2 is "inadmissible" for that agent. Using (1), this means that events in region 2 have no impact on the chances of events in region 1. Consequently, due to B's inadmissibility, whether $P(A|E) = P(A|EB)$ is irrelevant for whether local causality holds. It follows that, given (1) and (2), (BLC) is not the correct criterion of local causality.

I conclude with some reasons for thinking that quantum theory actually does conform to local causality.

Please contact the author for references.

**Patryk Dziurosz-Serafinowicz**
***Are Humean Chances Formally Adequate?***

It is generally agreed that any concept of chance (objective probability) should be formally adequate. Under the predominant view, formal adequacy requires chance to obey the classical Kolmogorov's axioms of probability (see, e.g., Salmon 1967, Eells 1983, Schaffer 2007). This demand has been met by the frequency theory (in its finite and hypothetical version) and by various kinds of the propensity theory. Surprisingly, so far little attention has been paid to demonstrate that the Humean theory of chance (Lewis1986, Hoefer 2007) is formally adequate.

To alter this situation, this paper aims to present a particular way of vindicating the formal adequacy of Humean chances. It shows that chances posited in the best-system stochastic theories should obey the classical axioms of probability because formally adequate chances are better experts. More precisely, the argument goes along the following lines:

1. Following David Lewis, it is argued that chances posited in the best-system stochastic theories should be responsive to the Principal Principle requirement. That is to say, chances are to be identified by playing the role of experts for epistemic agents whose evidence is admissible. Such chances are called the expert chances. The truth of this premise is motivated by a doctrine called chance functionalism.

2. It is argued that expert chances should minimize expected predictive inaccuracy defined as the expected 'distance' from any possible observational outcome of a chancy process. An expert chance that minimizes inaccuracy does not 'expect' that any other expert chance would do better as a predictor.

3. If (1) and (2) are correct, then we can prove the following result: for any formally inadequate expert chance there is a formally adequate one that minimizes the expected predictive inaccuracy. This can be shown once we adopt a certain class of scores—the non-distorting scoring rules—that measure the distance between chance prediction and the truth about the outcome of some chancy process.

4. Therefore, in order to decrease expected predictive inaccuracy it is necessary for an expert chance to obey the classical axioms of probability.

By weaving together chance functionalism and the requirement of minimizing expected predictive inaccuracy, the argument avoids certain quibbles surrounding Lewis' argument for formal adequacy. While Lewis (1980) argued, in a controversial way, that the vindication of formal adequacy requires to equate chances with objectified credences, the argument I defend is silent on the ontological status of chances. This is because once we accept chance functionalism as the correct view on chance, the temptation to think of a chance as reducible to only one sort of entity loses its allure; as far as the functional role is concerned it is left open what the nature of occupants of this role is. Therefore, the argument can be accepted by those who interpret Humean chances as objectified credences as well as by those who see Humean chances as mind-independent best-system regularities.

**Johannes Roehl**
***Physical Causation, Dispositions and Processes***

Since Mach and Russell philosophers of science have often denied that the concepts of cause and effect have any role in fundamental physics or other theories of systems with continuous time evolution. There are no causally connected events, but states of a system, and their time development is described by operators or equations. This claim apparently conflicts with causal descriptions in special sciences and with philosophical accounts that hold causation to be a relation between events. Such a divergence in a central concept could lead to a disunity of science and world. Furthermore, proponents of a metaphysics of dispositions or powers as fundamental causal features (Ellis, Mumford, Bird) have mostly disregarded this issue and not shown how their approach can be reconciled with the continuous time evolution of mathematical physics. Process theories of causation as advanced by Salmon and Dowe seem in many respects more appropriate for physical systems. Most recently Andreas Hüttemann has suggested an approach which bases process theories on dispositions which provide the most basic structure for causation. While I agree with the general thrust of his suggestions, that a world view with causal powers is also able to accommodate the viewpoint of mathematical physics, there are some points that should be improved and expanded upon.

Hüttemann's main argument for dispositions is based on the fact that they can deliver part-whole explanations of complex systems: The dispositions of the systems' components combine in the behavior of the complex system which is a partial manifestation of each of them. In his account pragmatic, explanatory and ontological considerations seem to run together, whereas I argue for a stronger thesis that the ontic basis of a mathematical description can be analysed in terms of dispositions, because these determine the functions that determine the time evolution which is ontologically captured as their joint manifestation process. Also Hüttemann's distinction of dispositional and categorical properties and the relation of dispositions to their manifestations seem somewhat idiosyncratic and in need of clarification. Against his analysis of the combination of „default“ manifestation processes (in absence of disturbances) and intervening factors I argue that this is too restrictive and enabling conditions (like the striking of a match) should also be admitted as causes for the realization process. This is better in line both with conceptions of dispositions in the literature and with accounts of „common sense“ event causation. Furthermore, Hüttemann classifies dispositions as „contributors“ and intervening factors as causes, which seems to run against the idea that the general feature, the disposition that grounds the law of nature, should not be any less important than the accidental factor. A more democratic conception of causal factors can be argued for. As an application I will show how classical forces can be understood as dispositions, conform to the central features of forces (causal relevance, directedness, superponibility, dependence) and fit into a framework of dispositions and their manifestation processes. Thus, an ontology with dispositions as relevant causal factors allows a unified ontology for both continuous processes and causal links of discrete events.

*General Philosophy of Science II*

**Stephan Kornmesser**

***Scientific Revolutions without Paradigm-Replacement and the Coexistence of Competing Paradigms in Linguistics***

T. Kuhn's descriptive and diachronic analysis of natural sciences led to the well known results that a) in normal science, scientific research is always based on a particular paradigm, b) in revolutionary science, due to unsolvable anomalies, a present paradigm is replaced by a particular new paradigm, which leads scientific research from then on, and c) all non-natural sciences are considered to be non-mature pre-paradigmatic sciences.

However, applying Kuhn's pioneering ideas 50 years after The Structure of Scientific Revolutions to the status quo of non-natural sciences, its requirement of descriptive adequacy enforces completely new results, contrary to that of a) to c). Thus, it comes out that on the one hand Kuhn's philosophy of science still works well to diachronically analyse the evolution of natural as well as of non-natural sciences, but on the other hand, the widely accepted claims a) to c) are to be refuted. For this, I will apply Kuhn's philosophy of science to two competing paradigms of linguistics, Generative Grammar (GG) and Construction Grammar (CG), the relation of which is discussed extensively in cognitive linguistics (Croft and Cruse [2004], Evans and Green [2006], Part III) and is subject of the current debate about the theoretical foundations of the discipline (Cognitive Linguistics, 20(1), [2009]). Hence, an analysis of GG and CG from the point of view of the philosophy of science yields a twofold output: Firstly, it contributes to linguistics by clarifying the relation of both approaches, especially concerning the epistemological status of their fundamental assumptions. Secondly, it contributes to the philosophy of science, since it disproves the generally unquestioned statements a) to c).

c.1) In order to prove that linguistics is a paradigm-led science, I will introduce the paradigms of GG and CG with respect to Kuhn's notion of a disciplinary matrix. By means of this, I firstly give an overview of the central claims and paradigmatic examples of GG and CG, and secondly show that GG and CG satisfy all conditions to be a disciplinary matrix. Therefore, contrary to c), I conclude that there are non-natural sciences that include paradigm-led research.

b.1) On the contrary to b), I will argue that scientific revolutions do not always include the replacement of an old paradigm by a new one, but that there are scientific revolutions without paradigm-replacement. For this, I will at first analyse Kuhn's concept of scientific revolutions, in order to show that scientific revolutions are not defined through the process of paradigm-replacement, in case of which my claim of scientific revolutions without paradigm-replacement would be false on conceptual grounds. Secondly, I will explicate a sufficient conditions for scientific revolutions with respect to Kuhn's What Are Scientific Revolutions, according to which scientific revolutions are characterised as extensional as well as intensional concept-shifts between the scientific languages of the particular paradigms that generate semantic incommensurability. On the basis of this, I will thirdly reconstruct the relation of GG and CG as being that of a scientific revolution. However, neither of the paradigms is replaced by the other one. Rather, both paradigms are upheld by wide spread scientific communities and exist parallel to each other.

a.1) According to b.1), GG and CG come out to be coexisting paradigms. Thus, contrary to a), scientific research is not always based on a particular paradigm, but normal science can rather be considered to be of a multiple-paradigm constellation. Since GG and CG try to explain the same research objects on the basis of theoretical assumptions and model ideas contradicting each other, GG and CG are reconstructed as coexisting competing paradigms (Schurz [1998]).

**Holger Andreas**

***Descriptivism about Theoretical Concepts Implies Ramsification or Conventionalism***

A proper semantics for theoretical terms should answer the following question: how do we come to refer successfully to theoretical concepts and theoretical entities? General philosophy of language has outlined three different, major strategies to give such an answer:

(i) The descriptivist account going back to Frege and Russell.
(ii) Kripke's "causal" account of reference.
(iii) Hybrid accounts that combine descriptivist with causal elements.

The descriptivist picture is highly intuitive when applied to our understanding of expressions referring to theoretical entities. According to this picture, an electron is a sptiotemporal entitiy with such and such a mass and such and such a charge. We detect and recognize electrons when identifying entities having these properties. The descriptivist explanation of meaning and reference makes use of theoretical functions, mass and electric charge in the present example. The semantics of theoretical entities, therefore, rests upon the semantics of theoretical relations and functions. The Kripkean story about an original baptism and causal chains of communication that transmit the reference from speaker to speaker are difficult to tell for theoretical relations and functions (Papineau 1996).

Consequently, philosophers of science have rarely been persuaded by Kripke's influential attack on the descriptivist picture. Psillos (1999, p. 296), for example, combines this picture with causal elements in a sophisticated way:

(i) A term $t$ refers to an entity $x$ if and only if $x$ satisfies the core causal description associated with $t$.

(ii) Two terms $t'$ and $t$ denote the same entity if and only if (a) their putative referents play the same causal role with respect to a network of phenomena; and (b) the core causal description of $t'$ takes up the kind-constitutive properties of the core causal description associated with $t$.

In the exposition of this account it is clearly stated that the *core-causal description* associated with a theoretical term is to be taken from scientific theories in their state-of-the-art at the time. The account of reference thus relies on the kind-constitutive properties of the causal role of putative referents being *correctly* described by any theory being used for the purpose of reference. For if one uses an incorrect description of an entity in order to refer to that entity, one will not succeed in doing so. What could be meant by asserting or presupposing the correctness of such descriptions? To answer this question, I shall distinguish three readings of the semantics of descriptions:

(i) Descriptions are factual statements.

(ii) Descriptions are denoting phrases in the sense of Russell (1905).

(iii) Descriptions are conceptual truths.

 (a) In the sense of Poincarean semantic conventions.

 (b) In the sense of a priori knowable metaphysically necessary truth.

I shall show that the first option leads to *semantic agnosticism*, using patterns of argumentation by Putnam (1980). In brief, the presumed truth of descriptions of theoretical entities becomes unknowable if these descriptions are taken as factual statements.

To get clear about the second option, it is helpful to recall the syntax of Russell's original account of denoting. Claiming of an individual being definitely described by the property $F$ that it has the property $G$ is expressed in Russell (1905) as follows:

$$\exists x(F(x) \wedge G(x) \wedge \forall y(F(x) \rightarrow x = y)) \quad (1)$$

If one remains faithful to the Russellean syntax in descriptivist and causaldescriptivist accounts of reference to theoretical entities and concepts, then one has to Ramsify the scientific theory T, where T is used to describe the theoretical concepts and entities under consideration. This will be shown with concrete and non-trivial examples. A consequence of Ramsification is that only the Ramsified theory (in place of the original theory) must be true in order to ensure success of reference. So, semantic agnosticism can be circumvented.

Finally, I shall try to show that we could equally well interpret the axioms that we use to refer to theoretical entities and concepts as semantic conventions. Semantic agnosticism is circumvented, as in the second reading of the semantics of descriptions.

Please contact the author for references.

## *Reduction*

**Ramiro Glauer**
***Emergence: a Lot of Philosophy and a Lot of Science***

\`Emergence' is a notion that is used in different philosophical and scientific disciplines in order to capture phenomena in which some properties of composed wholes seem to \`go beyond' the properties of the components and their arrangement. As the notion is notoriously vague, attempts to clarify what emergence amounts to are made by philosophers as well as scientists. Emergence is a prime example for an interplay between philosophy of science and science. My attempt is to show that the involvement of science and philosophy is fruitful in both directions. How much philosophy in the philosophy of science? Well, a lot. Along with a lot of science.

While attempts at clarification of the notion of emergence, such as Kim's (1999;2006), as well as general conceptions of the phenomenon (cf. e.g. Humphreys 1997) mainly lean on metaphysical considerations, accounts of particular emergent phenomena more centrally appeal to scientific considerations. In the classical debate, life was taken to be emergent from chemical systems because, for instance, it could not be explained how purely chemical substances could reproduce. With the progress of biological explanations, such phenomena became explainable and others became candidates for being emergent. As a corollary, the understanding of what emergence amounts to also changed.

Currently, the assumption that emergence has something to do with the complexity of a system's organization and the micro-derivability of system dynamics is relatively widespread. When spelling out what Stephan (1999) terms diachronic emergence and Bedau (1997) calls weak emergence both clearly have in mind a certain range of complex dynamical systems whose behavior can only be explained in terms of their components' behavior in certain laborious ways. But phenomena that seem to exhibit some form of top-down causation or holistic determination are also considered as emergent. With close attention to particular physical explanations Bishop (2012), for instance, argues for what he calls contextual emergence. And Seth (2010) introduces a quantifiable notion of emergence based on Granger causality that can be used to determine the extent to which some systemic property depends on component properties but is nonetheless independent of it in certain respects.

Arguably, these scientifically informed notions of emergence currently provide the biggest theoretical gain. Just as in the classical debate about the reducibility of life to chemistry, emergence is appealed to in order to conceptualize, and ultimately resolve, concrete explanatory challenges. Back then, the difficulties in explaining biological phenomena in terms of chemical goings-on made it reasonable to appeal to emergence. Now it's systems whose micro-behavior exceeds tractability, explanations that involve whole-to-part determination, or explanations in which global properties affect the local behavior of components which pose difficulties for standard reductive explanatory schemes. In science, such a philosophically sharpened scientific notion of emergence can be used to guide research and assess methodologies. In philosophy such scientifically informed notions shape bigger philosophical questions, for instance, about reduction, causation, and explanation.

**Robert Meunier**
***Pluralism in the Life Sciences – Complexity of Nature or Complexity of Culture***

In the proposed talk I will critically analyse a widely held view according to which the plurality of approaches in the life sciences reflects the complexity of the biological world. I will contrast this view with an anthropological pluralism that emphasizes the change of human practice in the development of culture as the driving force in generating new perspectives on the phenomena of life. To put it differently, the common version of pluralism holds that complexity fosters different interests and not that different interests make the world appear complex. In contrast, I content that pluralism grounds in the plurality of interests and practices, that is, if it grounds in any form of complexity then it is is the complexity of human culture.

I will address pluralism in terms of classification, decomposition and the individuation of properties of organisms. However, since scientific explanations presuppose or motivate the individuation of classes, parts or properties, and since individuation is mediated by methods, I take what might be called ontological pluralism, explanatory pluralism and methodological pluralism as being different aspects of the same problem. Furthermore, my account on pluralism is meant to be descriptive, not normative.

In the first part of my talk I will show how most accounts on pluralism in the life sciences rely on a notion of the complexity of the phenomena of life, which is taken to be the reason for an irreducible plurality of approaches in this domain. I will argue that such accounts require but usually fail to define simplicity and complexity independently of the plurality of approaches that make a domain appear complex. Furthermore, I will point out how such views typically maintain a distinction between natural and artificial classification (or decomposition), despite their rejection of essentialism and their allowance of conflicting classifications.

In a second part, I attempt to present an anthropological version of pluralism, which locates the reasons for the diversification of approaches to living phenomena and the resulting variety of classifications and decompositions in the diversification of human practices. I will show how cultural and technological change and the transfer of concepts and material between contexts create ever new contexts in which new perspectives on organisms arise and new parts and properties become visible accordingly.

The position implies that there is neither a clear-cut divide between science and the rest of culture, nor a principle difference between basic and applied biology. Accordingly, contexts such as agriculture, industry, transportation etc., where new perspectives on organisms arise and new properties of organisms become identified - obviously due to cultural change – are not only often historical starting points in the development of biological disciplines, but should also stand as models for understanding how the various perspectives of the core disciplines of biology, such as taxonomy, anatomy, embryology, physiology, ecology, etc. came about and changed in the course of changing practices involving organisms.

**Fabian Lausen**

***Using Insights from the Philosophy of the Life Sciences in the General Reductionism Debate***

Reductionism is a hotly debated topic in the philosophy of the life sciences. Regardless of what stance one takes on the nature and merits of reduction and reductive strategies, one philosophical point seems to be uncontroversial: The insights we have gained by paying more attention to the life sciences have greatly enriched our conception of reduction and reductionism in general. The insight that Nagel's classical model (Nagel 1961) and its derivatives are not easily applicable to the kinds of explanation that are salient in biological sciences have led to a different perspective on reduction; it now shows up as an important but not dominant factor in the construction of mechanistic explanations. This richer account of reductions allows us to evaluate the prospects of reductionism in a more differentiated way instead of just arguing for or against reduction and reductionism as a whole.

In my talk, I wish to pursue this way of thinking a little bit further. I shift attention away from the question of what a reduction is towards the question of how to evaluate the heuristic fruitfulness of reductionist approaches. This topic has received by far less philosophical attention than the nature of reduction or questions concerning reducibility in principle.

I conceptualize reductionistic research strategies as consisting of and making use of three epistemic activities (By using this term, I refer to Chang 2009): (1) Construction of identities, which aims at establishing relations between abstract, theoretical terms and physical entities. (2) Decomposition, which takes things apart and looks at the interactions of their parts. (3) Unification, in the sense of deriving a large number of statements using only a small set of basic statements and inference patterns.

I use this distinction to expand William Wimsatt's work on the biases of reductionistic research strategies (Wimsatt 2006). Wimsatt has put forward an impressive analysis of the ways in which reductionistic strategies in the life sciences can push our research in a particular direction. The point is that knowing about those biases enables us to use the respective strategies more wisely. Wimsatt mainly focuses on the biases that arise in conjunction with decomposition, as he is primarily concerned with the life sciences. In other branches of science, however, the other two activities seem to be more salient. For example, in physics, the concept of reductionism is more strongly tied to unification than to decomposition.

The main intention of my talk is to discuss some possible biases that can be associated with the construction of identities and unification. For example, construction of identities can be related to the overstressing of analogies and metaphors, which might rob these conceptual tools of their creative potential by assuming too many similarities between the domains they link. Unification can be associated with a tendency towards essentialism, for example when we try to characterize a multifaceted phenomenon like cancer by providing a small set of necessary and jointly sufficient properties that all types of cancer should share.

By using Wimsatt's approach on the other two reductionistic activities, I intend to continue on the promising path of using insights from the philosophy of the life sciences for more general questions in the philosophy of science.

Please contact the author for references.

**Malte Dahlgrün:**
***Emotions and Natural Kindhood***

Philosophers widely view Paul Griffiths' *What Emotions Really Are* (1997) as the classic scientifically informed statement of the eliminativist ideas (i) that emotion fails to form a natural kind and (ii) that some specific emotions fail to do so too. This conforms with what Griffiths has professed to be arguing for. I offer a critical reassessment. While Griffiths' basic subdivision of the emotional realm too could be argued to be untenable, I expect to limit myself in this talk to the following overarching points.

**1.** Contrary to how Griffiths has presented his position, eliminativism about emotions is a far from unorthodox view. It has been routinely advocated in mainstream emotion psychology since the inception of the field. In suggesting otherwise, Griffiths relies on a strawman. Affective scientists have not usually treated "the emotions" as a unitary category to which novel findings can be reliably extrapolated from samples (the hallmark of a natural kind, for Griffiths). And the idea that at least some folk categories of specific emotions need to be replaced in scientific discourse is almost a default idea among affective scientists. **2.** Griffiths' specific eliminativist claim in fact misrepresents his central empirical commitments: Endorsing basic emotions in the Darwin-Ekman tradition as the cornerstone of his theory, he in fact holds a view on which many emotions are indeed natural kinds. **3.** On the other hand, what is naturally taken to be the real challenge from empirical psychology to ordinary realism about emotions is almost entirely ignored by Griffiths: the longstanding research program of dimensional theories of emotions.

**Predrag Sustar**
***Naturalism in Action: The Case of Positive Emotions***

In this paper, I will focus on the subset of so-called positive emotions, most notably, joy, interest, contentment, and love, which have been significantly less examined in the contemporary psychology than their more famous counterpart, such as anger, fear, disgust and many other negative emotions. One of the main reasons, although there are others that also will be pointed out, for this asymmetry consists in the way in which psychologists more easily classify negative emotions as evolved adaptations, whereas the classification of this kind is, at least apparently, far less obvious for positive emotions.

In that regard, I will examine leading scientific models that try to account for positive emotions, especially the emotions instantiated above, in particular, how these models relate to evolutionary biology as its background belief system. More specifically, I will examine a current version of the broaden-and-build model of positive emotions (see, e.g., Catalino and Fredrickson 2011; Fredrickson and Cohn 2008) as scientifically most elaborate project. The model in question will be taken into consideration for the following reasons: (i) for the way in which it assesses empirical evidence, which is extremely thin with respect to the evidence gathered for psychological and physiological models accounting for negative emotions, and (ii) for the way in which it uses some of the central biological concepts, such as adaptation and, similarly, function, through which positive emotions are embedded within larger evolutionary perspective. Finally, I will try to make clear if there is any relevant lesson that could be drawn from the above difficulties that contemporary psychology encounters with positive emotions as far as a productive advancement of philosophical naturalism is concerned. In that specific regard, I will question some of the central claims expressed by moderate accounts of naturalism in the philosophy of science (see, e.g., Giere 2006), which are traditionally skeptical towards etiological strands within evolutionary psychology.

Please contact the author for references.

**Jeff Kochan**
***Subjectivity and Emotion in Scientific Research***

The history of science is full of cases of scientists appealing to their aesthetic emotions in the course of their scientific research. Examples include Werner Heisenberg's claim that the beauty of a mathematical form compels us to accept it as true; Rosalind Franklin's remark, recounted by James Watson, that the double helix model was too pretty not to be true; and Wolfgang Pauli's argument, elaborating on ideas from Kepler, that there exists a congruence between the “powerful emotional content” of the scientific unconscious, on the one hand, and the “behaviour of external objects,” on the other. This well-documented tendency brings with it a puzzle. As James McAllister has observed, philosophers of science have generally judged aesthetic evaluation to be “irremediably emotive and idiosyncratic,” and so excluded scientists' emotional dispositions from the rational reconstruction of scientific research. The underlying assumption seems to be that emotions are ineluctably subjective phenomena, and hence contribute nothing to the objectivity of science. Yet, by denying an epistemic role to scientists' affective dispositions, philosophers place themselves in the awkward position of ignoring phenomena which scientists' themselves have often insisted are of importance.

This paper suggests a possible solution to this puzzle by attempting to loosen the tie between emotions as candidates for epistemic significance, on the one hand, and the subjective experience in which emotions become manifest, on the other. We may overcome the uncritical identification of emotion with subjectivity by investigating the intersubjective contexts in which epistemically significant emotions are formed and sustained. This is a naturalistic and externalist project, calling for empirical investigation into the social processes by which the emotional dispositions of individual scientists become refined and attuned to specific objects of attention. There exists scarcely any systematic study of these processes, a circumstance lately decried by a number of philosophers, psychologists and sociologists of science who have sought to explain the constructive role played by emotion in scientific research.

The proposed naturalistic position of the paper will be developed through a critical engagement with the work of chemist and philosopher Michael Polanyi, who argued that science is an inherently emotional and social phenomenon, but also declared its emotional core to necessarily confound rational explanation. Thus, rather than illuminating the objectivity of the research process, Polanyi cloaked it in mystery. One of his key moves was to construe epistemic emotion in terms of individual scientists' unanalysable "personal coefficient," a concept he grounded in the discovery of the "personal equation" in nineteenth-century astronomy. It turns out, however, that nineteenth-century astronomers did not share Polanyi's view of the personal equation as impervious to analysis. In fact, they created a rigorous methodological regime which standarised personal acts of observation so as to ensure their objectivity within astronomical research. Attention to the history of this episode will help to dispel the mystery of Polanyi's account, and to replace it with an empirical method for articulating the rationality of the relation between emotion and science.

## *Philosophy of Physics I*

**Karim Thebault**
***Quantization as a Guide to Ontic Structure***

Ontological structural realism (OSR) is a popular viewpoint within contemporary philosophy of science, and is in part motivated by two arguments against consistency within the ontology associated with traditional realist understandings of scientific theory. The first springs from the multiplicity of different formulations of a theory (formulation underdetermination) and the second from the historical superseding of one empirically well-confirmed theory by another (pessimistic meta-induction, PMI). Under OSR the ontology of a physical theory is constituted by mathematical structures rather than objects and entities. To avoid formulation underdetermination, we designate the structures common between two formulations as our ontology; and to avoid PMI we isolate the structure common to a theory and its successor. For OSR to be viable: i) the structures must be substantial enough to constitute a genuine alternative ontology; and ii) the structures used to avoid the two arguments must be consistent.

Here I will present the outline of a program to evaluate OSR with respect to i) and ii) by considering the mathematical structure of two formulations of a given classical theory and the corresponding formulations of the quantum theory. My particular focus will be upon the test case of non-relativistic particle mechanics and considered the Lagrangian/Hamiltonian formulations of the classical theory and the path integral/Dirac-von Neumann formulations of the quantum theory. I will show that, modulo certain mathematical ambiguities, a viable and consistent candidate structural ontology can be constituted in terms of a Lie algebra morphism between algebras of observables and the relationship between the corresponding state spaces. I will then consider the prospects for extending the the program to encompass classical and quantum field theories.

**Stefan Lukits**
***The Full Employment Theorem in Probability Kinematics***

How much philosophy in the philosophy of science? I will show that for probability kinematics (the theory of how probability assignments are rationally updated), the majority view of philosophers can be undermined in favour of the majority view of statistical physicists. At issue is what I am calling the Full Employment Theorem (FET). It states that in order to reassess a probability distribution in the light of new evidence one needs a trained epistemologist to apply situation-appropriate tools from a wide range of methods in a pluralistically arranged toolbox.

FET may not be false, but I claim that the main arguments for FET fail. The converse of FET states that there are formal methods that we can successfully apply to all cases in which a probability assessment needs to be adjusted in view of new evidence, without the need for case-by-case interpretation by an epistemological expert. Advocates of FET brandish counterexamples, the pre-eminent one being van Fraassen's Judy Benjamin problem. It is alleged that this problem, under the application of the preferred formal method (MaxEnt, see below), produces counterintuitive results. Therefore, so goes the reasoning, the universality claim fails and FET stands.

If your observation comes in the form of an event, a plausible way to update your probabilities is by conditioning. If your observation comes in the form of a redistribution of probabilities over a partition of the event space, it is plausible to use Jeffrey conditioning. Observation can be even more general and come in the form of affine constraints (as in the Judy Benjamin problem). If Jeffrey conditioning cannot be applied to an affine constraint, we can use the Principle of Maximum Entropy (MaxEnt), based on the intuition that the observation should lead to an adjustment that in terms of information minimally affects the probabilities. Some, especially statistical physicists, say that MaxEnt delivers the unique solution to this problem that fulfills a set of basic rationality requirements. Advocates of FET believe that MaxEnt is only one of many different strategies to update probabilities rationally. They claim that the Judy Benjamin problem decisively undermines the generality of MaxEnt.

I will show various ways in which their arguments go awry. The results provided by MaxEnt for the Judy Benjamin problem are supported, not contradicted, by an intuitive approach that prima facie should support the advocates of FET. The independence assumptions which render the MaxEnt results counterintuitive are improperly applied by advocates of FET; in particular, it is a mistake to treat Judy Benjamin as a case for Jeffrey conditioning. The method of coarsening at random does not apply to the Judy Benjamin problem once the analogy to the Three Prisoners problem is fully appreciated.

In conclusion, philosophers have not made a persuasive case for full employment. Scientists who use the Principle of Maximum Entropy (whose applications span a variety of disciplines) can do so without worry about this instance of "philosophy in the philosophy of science."

**Johannes Thürigen**
***Theory Evaluation beyond Empirical Evidence: The Case of Research towards a Quantum Theory of Gravity***

It is widely accepted that theories in (empirical) science can be considered epistemically justified only if they predict or explain some phenomena. While this seems to be a necessary condition the overall evaluation of a theory is much more subtle. Important criteria are its systematization power (how much phenomena can be explained), its empirical content (how precisely can they be explained) and its uniformity (divisibility in as few parts as possible). Beyond these there also is a more global concept of uniformity, i.e. how well the theory fits into the overall web of theories which mainly depends on the number and strength of its intertheoretic relations.

In the light of these concepts we present an analysis of the basic structure and intertheoretic relations of some approaches to quantum gravity each starting from quite different assumptions. These are Loop quantum gravity, Spin foams, Causal dynamical triangulations, Regge calculus and Group field theory. The aim of this analysis is to critically discuss an argument of physicists working on quantum gravity, stating that there is some kind of convergence of the mentioned approaches which supports and (at least partially) justifies them. Such an argument has high relevance since neither the precise relation to the established theories (and thus the phenomena described by those) nor the derivation of original phenomena might be achievable in the foreseeable future, leaving uniformity as the only epistemological criterion in favour for them. It will turn out that, at least so far, convergence mainly takes place at the level of the conceptual framework of the theories. This work is also related to the theme of the conference: not only is this philosophical appraisal of approaches to quantum gravity in its methods quite closely adapted in particular to the physical sciences (though the structural analysis of theories and their relations can be seen as a common element to all philosophy of science as well). It also shows how important philosophical evaluation of theories becomes in science itself when theory is overtaking experiment dramatically. Regarding the closeness between science and the philosophy of science the question therefore might equally be

"How much philosophy of science in science?"

With respect to the discussed branch of physics, indeed still considered science, it seems to be more then expected: when experimentally testable consequences of theory are lacking for decades, research programs seem to start naturally addressing normative questions along the way which the discussed case is an enlightening example of.

*Philosophy of Physics II*

**Manfred Stöckler**
***How to Divide between Physics and Philosophy of Physics?***

Der Beitrag ist eine metaphilosophische Untersuchung der Rolle der Philosophie im Austausch mit speziellen Wissenschaften am Beispiel der Philosophie der Physik. Dabei geht es aber nicht um Klassifikation, sondern um Methodenreflexion und die Aufklärung einiger Missverständnisse, u. a. beim Begriff der Interpretation einer Theorie.

Was ist mit Philosophie der Physik gemeint?

Typische Probleme der Philosophie der Physik sind die Ursachen der Asymmetrie und der Richtung der Zeit, das Verhältnis von Konvention und Erfahrung in der Geometrie, das Loch-Argument im Streit zwischen substanzialistischen und relationalen Theorien der Raumzeit. Gibt es eine Lösung des Messproblems der Quantenmechanik? Über welche Sorte von Entitäten spricht die Quantenfeldtheorie? Legt die Quantentheorie die Existenz vieler Welten oder einen Strukturenrealismus nahe? In welchem Sinn ist die gegenwärtige Physik kausal? Aber auch: Welche nicht-empirischen Voraussetzungen macht Newtons Gravitationstheorie?

2. Modelle der Abgrenzung von Philosophie und Physik

Es gibt traditionell verschiedene Konzeptionen des Verhältnisses von Physik und Philosophie:

a) das „Fundierungs-Modell“: Die Philosophie legt den methodischen Rahmen und die begrifflichen Grundlagen fest, auf die die Physik dann aufbaut (z. B. Raum, Zeit, Kausalität, Substanz). Fundierungsprogramme gibt es in metaphysischen, transzendentalen oder konstruktiv-methodischen Versionen.

b) das „Allgemeinheitsmodell“: Die Grundlage ist hier ein Spektrum an physikalischen Fragen und Ergebnissen, die allgemeinsten davon bilden die philosophischen Fragen und Aussagen (vgl. auch die induktive Metaphysik).

c) das „Reinigungsmodell“: Die Philosophie beschäftigt sich nicht mit den Inhalten, sondern nur mit den Methoden der Naturwissenschaften (z.B. mit der Analyse physikalischer Begriffe).
Diesen drei Modellen liegen unterschiedliche Intuitionen über Philosophie und Naturwissenschaft (und über Nähe und Abgrenzung dieser Bereiche) zugrunde. Von

diesen Intuitionen hängt ab, wo die Trennlinie zwischen Physik und Philosophie gezogen wird.

3. Verständigung über Ziele der Philosophie und der Naturwissenschaft

Die Modelle a) und c) trennen Naturwissenschaft und Philosophie nach ihren Erkenntnisquellen. Zur Physik gehören die empirisch entscheidbaren Fragen, philosophische Probleme sind nicht experimentell zu lösen. Das Fundierungsmodell geht zu Recht davon aus, dass physikalische Theorien einen begrifflichen Rahmen voraussetzen, der nicht direkt aus der Erfahrung kommt. Dabei bleibt aber zu klären, wie über die erkenntnistheoretischen und ontologischen Voraussetzungen und über die Bewertungskriterien der empirischen Theorien argumentiert werden kann. Offenbar spielt auch hier Erfahrung eine Rolle und der einsetzende Rückkopplungsprozess mit dem Ziel eines Überlegungsgleichgewichts ist Thema der Philosophie der Physik.

Häufig trennt man Physik und Philosophie auch nach ihren Aufgaben und Zielen. Danach ist die Physik ein Instrument für Vorhersagen und für Veränderungen der Natur, während die Philosophie anstrebt, Natur insgesamt zu verstehen und unseren Platz in ihr zu bestimmen. Das Modell b) und viele Varianten des Naturalismus betonen dagegen, dass auch die Physik die Phänomene der Natur erklären und so verstehen will.

Da offenbar die Physik wie auch die Philosophie beide sowohl mit empirischem wie mit nicht empirischem Wissen arbeiten, sollte die Ausdifferenzierung der Philosophie der Physik mit besonderen Kompetenzen begründet werden, die bei speziellen Fragestellungen nötig sind. Dazu gehören professionelle Vorstellungen über Begriffsexplikationen, mögliche ontologische Modelle oder Begründungsformen von Geltungsansprüchen.

**Emre Keskin**
***Philosophy of Cosmology: Not Enough Philosophy, not Enough Cosmology.***

In conjunction with the advances in modern cosmology, philosophy of cosmology started to attract more attention. Although closely resembling philosophy of physics, the issues discussed in philosophy of cosmology have certain unique features. For this reason mentioning some cosmological ideas in passing while writing philosophy of physics cannot be sufficient to argue serious points native to cosmology. One issue regarding the treatment of ideas related to cosmology is that there are not satisfactory treatments of modern cosmology in relation to philosophy of science. Although there are relatively to the point philosophy of cosmology works, they tend to be the exception, and not the standard. I will demonstrate this problem with an examination of one of the most prominent recent philosophy of physics projects that includes cosmological claims without realizing their actual ramifications.

David Albert developed an account the main aim of which is to tackle the issue of irreversible thermodynamic phenomena. In the core of his system, in addition to the time reversal invariant laws of motion, he has the past hypothesis (PH). In addition, he maintains an account of laws of nature that is similar to Mill-Ramsey-Lewis best system analysis with the emphasis being on the balance between simplicity, informativeness and fit. One of the central claims Albert's project is that PH increases the informativeness of their system significantly with almost no increase in its complexity.

I aim to show that the way the past hypothesis is employed in Albert's project is unjustifiable, because it ignores modern cosmology. I will construct an objection that appeals to modern cosmology to show that PH cannot be introduced without going against the balance between simplicity and informativeness. Moreover, I will show that the only way that PH can be considered a law of physics is if we add it to the set of laws of modern cosmology regarding the early universe.

According to Albert, the past hypothesis is a description of the first instance of "the entirety of that sector of the universe which has any physical interaction with the systems interest to us" (Albert, p. 85). However, modern cosmology offers several proposals for the laws of the early universe to explain how this first instance came to be. We cannot consider PH independently of the laws of modern cosmology if we want PH itself to be a fundamental law of physics. Thus, including PH as a law in to any system requires including laws of the early universe to that system.

First, I claim that PH cannot be considered independently of a large package of laws about the early universe. PH has to be a member of this larger set in order to achieve what it is supposed to do in Albert's project. Hence, to use PH we have to add an entire set of laws of modern cosmology to our best system. This extra baggage diminishes the claims of simplicity.

Second, I argue that even if we concede that PH is simple, it is a state chosen due to its simplicity rather than its ability to explain the time evolution of physical systems. I argue that there were states of the universe earlier than PH that cannot be stated as simply as PH. In either case, I maintain that including PH into a system that makes use of best system analysis cannot be justified and it can only be a law if it is a part of laws of modern cosmology.

**Thorben Petersen**
***Is There Too Much Philosophy in The Rietdijk/Putnam-Argument?***

The special theory of relativity ranks among the most successful of our scientific theories. Philosophical issues concerning special relativity inter alia include (i) the questions whether, and if so why Einstein's unifying approach is preferable to Lorentz's dynamic interpretation of relativistic effects, (ii) the question whether

Lorentz invariance merely codifies or ultimately explains relativistic behaviour, (iii) the notorious conventionality of simultaneity as well as (iv) the question whether one of the theories' founding assumptions, viz. the light postulate, is in serious conflict with quantum non-locality, which is also empirically well-confirmed (most of these issues are closely interrelated).

A further, more delicate philosophical issue is the impact of special relativity on our intuitive or everyday concept of time. It has been argued, most notably by Wim Rietdijk and Hilary Putnam, that special relativity may be used to show, in Putnam's famous phrase, that "future things are already real". Effectively this is to say that our everyday understanding of time, which inter alia involves the notion of a future not existing yet, fails to link up with the real world and that our impression of time passing is at best subjective a phenomenon (in a very pejorative sense of the word 'subjective' alluding to illusion).

Now, while some object to this threat to common sense constructively (e.g. by interpreting relativistic effects in Lorentzian fashion), others, such as Yuval Dolev and Steven Savitt, simply deem the whole controversy to be ill-posed. In other words, then, these authors claim that this time it's too much philosophy in the philosophy of science. However, just because a certain scientific theory is in conflict with our common-sensical picture of the world doesn't mean or guarantee that the resulting debate is therefore meaningless. The aim of this talk is to figure out whether the Rietdijk/Putnam-Argument indeed induces too much philosophy into the philosophy of science. Besides the truth of the special theory, the argument actually does mobilize two further assumptions of a rather philosophical character, namely, roughly, that (I) frame-dependent simultaneity of events implies their (frame-dependent) co-existence and that (II) the relation thus specified holds transitively across frames. Both assumptions in turn presuppose that (III) existence be defined temporally (in terms of the present) and (IV) that inertial frames of reference are equipped with an "observer". I shall consider whether any of these assumptions may reasonably be considered too philosophical to rule out common-sense and then compare the debate at hand with some of the abovementioned philosophical issues concerning special relativity.

## *Historically oriented studies*

**Cornelis Menke**
***John Stuart Mill on the Existence of the Ether***

In his System of Logic, John Stuart Mill wrote it would not have been written without the aid derived from William Whewell's History of the Inductive Sciences

(1837) and his Philosophy of the Inductive Sciences (1840). Nevertheless, Mill was critical of Whewell's views on induction and scientific method; Whewell replied to Mill's criticism in the short treatise Of Induction (1849), to which Mill referred to in later editions of the System.

According to the usual reading, this debate was about methodology: Whewell was defending the view that a successful prediction (of a certain kind) carries more evidential weight than accommodations of known phenomena, while Mill was maintaining that whether or not a phenomenon had been predicted or accommodated is irrelevant to question of theory confirmation.

In this paper, I shall defend the thesis that this interpretation misconstrues central parts of the debate in general and of Mill's position in particular: From Mill's point of view, the debate not about methodology, but about scientific realism. Mill was particularly in- terested in the case of the ether hypothesis. The point of contention was not Whewell's claims concerning methodology, but the claim that the successful predictions of the ether hypothesis "prove" that the ether really exists.

Against this claim he argued that, firstly, the success of the predictions of the wave theory are "nothing strange" but exactly what is to be expected—and so in this case the successful predictions added nothing because the laws of the theory had already been confirmed. Secondly, he claimed that the predictions in dispute were predictions of phenomena of the same kind as the phenomena the theory was devised to explain—namely different forms of wave phenomena. Thirdly, Mill argued that the predictions confirmed only the empirical laws of the wave theory but not the existence of the ether itself.

This interpretation is strengthened by Mill's reminiscence to a point made by John F. W. Herschel in his Preliminary Discourse on the Study of Natural Philosophy, to which Mill's methodological views are known to be indebted. In the Discourse, Herschel discussed pre- dictions in two contexts: on the one hand, like Whewell he regarded the fulfillment of predictions as providing a particularly strong form of confirmation, especially if they were connected to a correction of premature generalisations; on the other hand, Herschel thought fulfilled predictions to be valuable for laymen—people lacking the ability to follow and jud- ge advanced reasoning and mathematical calculation—, allowing them to assure themselves of the correctness of scientific theories. It is exactly this point Mill is maintaining when talking of predictions as "strik[ing] the ignorant vulgar" as opposed to "scientific thinkers".

I shall argue that Mill's argument is not only of historical interest, but provides a powerful yet neglected objection against recent versions of the no-miracles argument for scientific realism, too.

**Dinçer Çevik**

***Meeting the Metaphysics of Geometry: The Legacy of Herbart, Gauss and Riemann***

In my talk, I will discuss contributions of J. F. Herbart and C.F. Gauss' philosophical reflections on space and geometry in Riemann's introduction of the concept of the manifold. I found this topic intriguing because it is a good exemplar for how and to what extent philosophy can be relevant in the clarification of the basic concepts of geometry.

The most important turning point in geometry is Riemann's lecture titled "On the Hypotheses which lie at the Bases of Geometry". In that lecture, Riemann argued that the fundamental concepts that are central to Euclidean geometry do not have to be part of every system of geometry imaginable. What Riemann meant was that the fundamental concepts of Euclidean geometry should not be though of as necessary for all possible system of geometries In order to reach these conclusions about Euclidean geometry and in order to introduce new concepts, it was necessary for Riemann to engage in the activity of conceptual clarification. The fundamental new concept he introduced was the concept of manifold. Describing this notion Riemann explicitly refers to J. F. Herbart and C.F. Gauss' philosophical reflections on space and geometry. By tracing the philosophical roots of Riemann's discussions, I will try to address the issue of how and to what extend philosophy can be relevant in the conceptual clarification of basic concepts of geometry.

According to Herbart sciences developed their central concepts with respect to their contexts, however philosophical studies of the sciences requires more; they had to form unifying concepts that transcends specific contexts (Scholz, 1982, p.424). Among others, especially this understanding seems to influence Riemann's ideas about geometry; diversity in geometric thought could be kept together by means of concept of manifold for it could admit different enrichments to show the possibilities and conceptual freedom of geometric thought (Scholz, 1992, p.4).

Treating complex numbers Gauss used of geometric language in a non-geometric context. Separating possibility of mathematics based on abstract spatial concepts from constrained approach derived from perceptions he discusses the geometry of the complex numbers (Nowak, 1989, p.27-28). Riemann's specific interest was not on complex numbers; rather he was drawing inspiration from Gauss. Creating space-like objects was opened by the Gauss and Riemann was following and citing him as an authority for the validity of such expansions of the domain of mathematics. Distinguishing mathematical language which only taken from vocabulary of sensible space and from mathematics which intrinsically dependent upon it Riemann also followed Gauss. Gauss' influence on the first part of the "On the Hypotheses which lie at the Bases of Geometry" is clearly philosophical.

**Parzhad Torfehnezhad**
***In Carnap's Defense***

In this paper I would present an interpretation of what Carnap calls a "linguistic framework" (LF). I will present LF as an unobjectionable concept directly implied by the "linguistic doctrine of logical truths". According to what a comprehensive survey of Carnap's later works suggests LF is to be construed as a hierarchical heterogeneous factual-conventional space for making assertions that, according to some rules, primarily would be constructed from purely factual statements to purely conventional statements of a calculus, and that can equally be constructed the other way around i.e. from purely conventional statements of a calculus to purely factual statements of a newly interpreted language. Based on this interpretation, I will show all of Carnap's distinctions including the analytic-synthetic distinction are not to be construed as absolute distinctions at all. Rather, they are relative distinctions decidable by our preferred choice of syntax. I would also show that there is a difference between what Carnap calls "way of speaking" and what he considers as an "artificial language". In the course of this paper I will talk about different meanings of what we regard as "obviousness of elementary logic". Consequently, I will conclude none of Quine's major objections would address to the main points of Carnap's theory.

## *Philosophy of Social Sciences (incl. Economics)*

**Andrei Nasta**
***A Justification of the Minimalist Notion of Economy***

This paper provides new justification for a methodological principle, the principle of economy (or simplicity), advanced in the linguistic Minimalist Program (MP).

This methodological economy principle is a guide for theory construction (Chomsky 1995), but is relatively neglected in the linguistics literature.

Two main arguments are common: (i) simpler theories are easier to handle, (ii) good scientific practice recommends simplicity. Both have limited force.

They do not convince the skeptics, since the cost of changing the theory is high. More importantly, if reality is complex, methodological economy may be the wrong guideline (cf. Jackendoff 1996). Independent justification is thus needed, and this is what is on offer. I first make an abstract argument for simplicity, and then discuss its compatibility with the minimalist theoretical context.

(1) We assume, that science is able to reach the truth about the empirical world with respect to a particular scientific question. We further assume the following (cf. Kelly 2007).

A world is an infinite sequence of mutually disjoint subsets of (stable) effects (propositions).

The empirical world cannot be seen all at once, but only in part (an initial segment of sequences of effects).

Each set of effects corresponds uniquely to a theoretical structure (theory).

Science should find a strategy to construct a theory that corresponds to the empirical world.

The result says that even in a complex world, the Ockham strategy (of choosing the simplest theory) is the most efficient route to the truth.

Efficiency is the least upper-bound on the cost of converging to the truth. (The cost is a function of retractions. Retractions are bad because they involve cognitive effort and errors.)

Thus, the best strategy is to cover as much as possible form the effects (experience) encountered as simpler as possible, and refrain from speculating (guessing anomalies). This is a minimax solution.

(2) Similarly, the MP recommends minimal assumptions given the empirical facts (class of initial segments of effects), which corresponds to the Ockham strategy. Indeed, the MP renounced to several theoretical structures: extra-steps in the derivations, extra symbols in representations, and the representations not needed to vindicate the basic linguistic facts (Boskovic & Lasnik 2007).This establishes a minimal compatibility between the MP strategy and the Ockham strategy.

A minimalist test case for the above strategy is how it deals with grue-like problems (Goodman). How does simplicity select between inconsistent developments of the minimalist program. Crudely put, Ockham has no such grue-problem, because it is a semantic principle.

A further interesting consequence, is that the commitment to simplicity puts severe constraints on the MP. Some minimalist principles, although supported empirically, are less justified by Ockham. The Ockham strategy recommends suspension of belief in those principles.

My tentative conclusion is that philosophy does have the resources to justify the minimalist linguistic principle of simplicity, and also to put further constraints on empirical research.

**Kristina Musholt**

***The Personal and the Subpersonal in Social Cognition***

It is a generally accepted assumption that what grounds our ability for social interaction is the mastery of a common folk psychology, where the term folk psychology is used to stand for the practice of ascribing mental states to others for the purpose of predicting and explaining their behaviour. There is considerable debate as to how the nature of folk psychology is to be understood. According to the theory-theory (TT), folk psychology is to be seen primarily as the ability to predict and explain the behaviour of others by applying a theory concerning the way the mind functions. According to the simulation theory (ST), rather than having to apply a set of principles about how the mind works, we can simply rely on our own minds to predict and explain the behaviour of others. However, both theories (with the exception of so-called low-level simulation accounts) are in broad agreement that social cognition should be characterised in terms of mental state attributions, that is to say that social cognition consists in reasoning about the mental states of others in order to predict and explain their behaviour. This, in turn, requires the possession of mental-state concepts, in particular the concepts belief and desire. Consequently, much of the empirical research in this area focuses on determining when and how children acquire mental state concepts, in particular the concept of belief.

Recently, this standard conception of social cognition as a practice of mental state attributions for the purpose of predicting and explaining behaviour has been called into question by proponents of phenomenological approaches to social cognition (e.g. Gallagher 2007, Zahavi 2005) and by those who argue that mental state attributions are so computationally demanding that it is implausible to assume that they underwrite most of our social cognitive abilities (e.g. Bermúdez 2004, Apperly & Butterfill 2009). In response, it has been argued that while these attacks on the classical view of social cognition have purchase at the personal level, they have no bite at the subpersonal level of explanation (Herschbach 2008, Spaulding 2010).

In this paper, I critically examine this response by considering in more detail the distinction between personal and subpersonal level explanations. I will argue that insofar as both TT and ST rely on the possession of mental state concepts, they cannot be seen as subpersonal level explanations, as concept possession can only be ascribed at the personal level. Moreover, the appeal to the subpersonal level cannot adequately address concerns regarding the computational complexity of mental state reasoning. Thus, this defence of the received view of social cognition fails. I conclude by sketching an alternative view of social cognition according to which there are nonconceptual forms of mentalizing that ground many of our social interactions and provide the basis for the acquisition of mental state concepts. This alternative view is also better suited to account for recent findings regarding the social-cognitive abilities of young infants.

**Simon Lohse**

***Social Emergentism Reconsidered***

In this paper I want to shed some light on the ongoing debate between social emergentists and (reductionist) methodological individualists by discussing general problems and preconditions for a plausible emergentist social theory. The basic argument of the article is that it is fruitful to analyze social emergentist theories against the background of the original intention behind the introduction of the concept of emergence as an intermediate position between dualism and reductionism in the philosophy of biology.

Social emergentists, in the general tradition of Emile Durkheim, argue for some kind of distinctiveness or autonomy of social phenomena. These phenomena are based on intentions, actions and interrelations of individuals but, nevertheless, are supposed to show properties that are not reducible to the individual level. Methodological individualists, on the other hand, believe that these phenomena can in fact be reduced to the individual level: If individuals and their relations constitute social phenomena, what else – in principle – do you need? A key issue in this debate is the ontological status of social phenomena: are certain social phenomena really autonomous or distinct in some sense or is the (prima facie) autonomy merely apparent?

To approach this issue, I proceed as follows: In the first section I will outline the relevant opposing camps within sociology and philosophy of social science into which I will insert my own approach. Then, I am going to briefly discuss two ambitious emergentist strategies and their different concepts of social emergence, namely Niklas Luhmann´s concept of communication, and Dave Elder-Vass´ concept of social structures. It is my goal to highlight some of the problems of these theories and thereby illustrate some challenges for the concept of social emergence in general. Next, I want to introduce some conditions of adequacy from general philosophy of science, which articulate a fruitful concept of emergence. Using these conditions of adequacy makes it possible to systematically analyze and evaluate the emergentist strategies and to pinpoint the roots of the problems that were highlighted in the previous section. Finally, I want to briefly touch on a promising concept of social emergence.

**Anke Bueter**

***The Agnosticism-Argument for Value-Freedom***

A common strategy in defending the ideal of value-free science is the "Agnosticism-Argument" (AA). Proponents of AA often proceed from the following reconstruction of criticisms of value-freedom: because of some form of underdetermination thesis, evidence and cognitive values would be insufficient to justify theory choice, wherefore other values came to bear. This argument is then rejected by conceding that theory choice is sometimes underdetermined by evidence and cognitive values; yet, this would not imply a legitimacy or necessity of extrascientific values. Instead, the epistemically correct behaviour were to stay agnostic until further evidence decides the question.

I will argue that AA is unsuccessful in saving the value-free ideal. The reason for this is that not all relevant decisions can be postponed until further evidence, or can be determined by this evidence. To start, hypotheses and empirical evidence do not stand in a relation of direct implication. This point does not presume any strong underdetermination thesis, only that a hypothesis does not by itself imply which empirical consequences are essential for its evaluation. The deduction of empirical consequences often proceeds on the basis of background assumptions, parts of which specify which empirical evidence is significant and which anomalies might be insignificant. This specification of significance can already be value-laden.

Second, it is a misconstruction that opponents argue social values to be necessary because the traditional standards of theory choice were insufficient. Rather, it has been argued that these standards (especially the distinction between cognitive and non-cognitive values) are themselves problematic. Cognitive values are justified by their contribution to the goal of science. Here, it is questionable which is the goal of science; whether there is one such superordinate goal at all; and how to establish the relation of certain values to this goal. I argue that the designation of cognitivity better proceeds from specific goals in concrete research contexts, because this facilitates an empirical evaluation of certain values' contribution to them. Importantly, these goals can be cognitive and political at the same time. Such a value-ladenness of scientific goals is then transferred to the values contributing to them.

A third problem stems from the debate on inductive risks. This concerns research with foreseeable, possibly severe social consequences of errors. It has been argued that value-judgments concerning these social consequences should play a role in setting the standards for evidence: Differing degrees of severity might require different amounts of evidence. Again, it is not possible to just postpone the concerning decisions – only if by waiting for more evidence, a state of complete certainty could be reached. If this is not the case, the decision on how much evidence is enough needs to be made in the light of risks of error.

I will propose that instead of defending the value-free ideal by invoking unreachable standards of certainty, it is preferable to recognize the multiplicity of points were values can become relevant and to discuss such value-influences openly, evaluating their legitimacy case by case.

**Adam Toon**
***Models, fictions, and Emma Bovary***

When scientists formulate a theoretical model they typically make assumptions that are true of no actual, physical object. And yet they also talk as if there were such objects and as if they can find out about their properties. Theoretical modelling therefore presents us with ontological puzzles: how are we to make sense of the fact that much of scientific practice seems to involve talking and learning about things that do not exist?

One way to try to solve these puzzles is to insist that, while no actual, concrete object satisfies the scientists' assumptions, there is some other object that does satisfy them. According to Ronald Giere, for example, theoretical models are abstract objects. Recently, Peter Godfrey-Smith, Roman Frigg and others, have suggested that theoretical models should instead be understood in the same way as fictional characters, like Emma Bovary. These authors endorse what I will call an 'indirect fictions view' of theoretical modeling. According to this view, scientists represent the world indirectly, via fictional characters.

In this talk I will argue that we should resist the indirect fictions view. First, I shall argue that, although comparisons between models and fictions are suggestive, scientists' modelling assumptions do not always parallel passages about fictional characters. Instead, they are better understood in the same way as historical fiction that represents actual people, places or events. Second, and more seriously, I shall show that the indirect fictions view must confront the longstanding debate over the nature of fictional characters. Realists argue that we must grant fictional characters a place in our ontology, perhaps as some form of abstract or Meinongian non-existent entities, while antirealists attempt to understand fiction without positing such entities. As a result, comparing models to fictional characters does not, in itself, appear to get us very far.

Proponents of the indirect fictions view respond to this problem in different ways. Some look to existing theories of fictional characters. For example, Frigg aims to flesh out the indirect fiction view by drawing on Kendall Walton's antirealist theory of fiction. I shall argue that this strategy faces difficulties, since an antirealist account is at odds with the indirect view of modelling. By contrast, Godfrey-Smith suggests that philosophers of science may defer questions concerning the ontology of fictional characters to those working in aesthetics or other fields. I will argue that this deferral strategy is unsuccessful, however, since the indirect fictions view makes important questions concerning the nature of scientific representation dependent upon the ontological status of fictional characters.

In place of the indirect fictions view, I will offer my own, direct account of theoretical modelling. This account also takes its inspiration from Walton's theory, and sees important parallels between models and works of fiction. But it does not require us to enter into debates over the ontology of fictional characters. According to the account I will propose, rather than representing a system indirectly in modelling, via abstract or fictional entities, scientists represent the system directly, by prescribing imaginings about it. Models, I shall conclude, are works of fiction, but not fictional characters.

**Stephan Kopsieker**
***Making Sense of the Distinction between Functional and Structural Modularity***

The concept of modularity has gained some prominence in recent evolutionary biology and developmental biology, especially in regards to an intended synthesis between the two biological disciplines. Modularity captures an important feature of complex biological systems like gene-networks, developmental systems, evolving lineages, or ecosystems. From a epistemic point of view the concept of modularity can be seen as a tool for dealing with the biological complexity of such systems. Modularity allows the scientists to apply the strategy of decomposition. The system under invastigation is decomposed into its modules, which then can be investigated separately, sometimes by further decomposition. But besides this reductionist element (i.e. explanation of a system in terms of its parts at a lower level), the concept of modularity has an holistic element too. Modules are always conceived as parts of a greater whole with a hirarchical organization.

Currently philosphers distinguish between two different notions of modularity, a functional and a structural one. Complex systems can be decomposed into modules from a structural or a functional perspective. This distinction is important because, depending on the concept of modularity which is applied, the decomposition of a system can result in very different pictures. A structural module can correspond with a functional module, but doesn't have to. Furthermore the distinction gains more weight, when the relation of modularity to the concept of plasticity is considered. The concept of plasticity accounts for another phenomenon in complex biological systems (i.e. the ability of the system to react with different responses to different conditions in the external or internal environment).

One aim of this paper is to analyse the relationships between the two concepts of modularity in more detail than sofar; another is to analyse how they go together with the concept of plasticity. The claim will be, that structural modularity and functional modularity differ in their relation to plasticity. While structural modularity allows for (and can even be seen as a condition for) plasticity, functional modularity can be at odds with plasticity, especially when plasticity itself is conceived in functional terms.

I will illustrate this claim with the example of the immune system, which will be looked at from the perspective of the clonal selection theory. I will show, that although functional modules can be identified, the functional plasticity of the immune system (i.e. the abillity to defend the organism against a broad variety of pathogens) is the result of structural modularity in the surface textures of lymphocytes.

# Symposia

## Monday, 11 March •16:45-18:45

*Philosophy of Biology*

**Emanuele Ratti [Emilio M. Sanfilippo, Federico Boem]**
***Ontology for and from Sciences. The Ontological Analysis of Biology***

In philosophy, formal ontology is usually considered to be the combination of informal methods of classic ontology and formal methods of modern symbolic logic. In this respect, ontology is formal in the sense that it can be formalized in a language of logics whose syntax and semantics are designed to capture the most general features of being regardless of any particular area of reality. Recently, a field called applied ontology has emerged in computer science, as the attempt to represent the content of information in a computer processable and understandable format. Computational ontologies constantly make use of the formal tools and theories of philosophical ontology, e.g. the theory of parts, the theory of dependence, the theory of wholes, etc. Thus in bioinformatics, a field is emerging that exploits philosophy and takes benefits from philosophical investigations for its own purposes. In this symposium we are interested in the application of formal ontological theories for the computational representation of scientific results, particularly in the field of general medicine and molecular biology.

The first talk is dedicated to the introduction of applied ontology. We briefly introduce the Basic Formal Ontology (BFO), an upperlevel domain-independent ontology which development is philosophically driven and is particularly suited for the representation of scientific results. Then, we show how some biomedical entities (e.g. disorder, disease, and organism) can be represented using the ontological and logical structure of BFO. We follow the standards proposed by the OBO Foundry, which is a scientific community that aims at developing and maintaining scientific biomedical ontologies. However, applied ontology shows several difficulties regarding the formal representation of some particular entities, which ontological status is still not very well known or ambiguous.

In the second talk we show that the current widely recognized mereologies and theories of unity cannot make sense of scientific results concerning that object that in biological research is named ‘gene’.

Next, we present how the current ontological alternatives (continuants or occurents) on defining stemness, can provide new lifeblood to the debate on what is biological individuality.

To sum up, the aim of this symposium is twofold. First, there is a practical role for philosophy within scientific research in providing new tools for concept modeling and computational knowledge representation. Theories and tools of formalphilosophical ontology play a fundamental role in representing scientific results for computation purposes and have led to world widely acknowledged improvements in computer science and engineering. Next, new trends in molecular biology can be useful for understanding and further improving such formal philosophical theories. In other words, specific research fields, as molecular biology, can really provide useful insights to the investigation of the being qua being. In particular, the way biological entities are conceived and structured by biologists can show us new ways of thinking about such formal relations as parthood, deriving from, or identity criteria. This will be shown by underlying the difficulties in applying the classical ontological theories to recent findings in biology.

*Mechanisms*

**Phyllis Illari, Stuart Glennan and Meinard Kuhlmann**
***The New Mechanical Philosophy and the Unity of Science***

This symposium examines the issues of unity and disunity with respect to mechanisms across the sciences. The past decade has seen a rapid growth of philosophical interest in the concept of mechanism and its place in understanding traditional issues in the philosophy of science, including explanation, causation, reduction, inference and discovery. While much work in "the new mechanical philosophy" has focused in the philosophy of biology, psychology and neuroscience, some philosophers have argued that there are meaningful ways to think about mechanisms across the sciences. Collectively, these developments suggest that the new mechanicism could provide resources for a more unified philosophy of science and with it some perspective on the ways in which the sciences themselves might be unified. In this session we will explore whether and to what extent these possibilities are real or illusory.

Paper 1 proposes and defends a unifying characterization of mechanisms that gives an understanding of what is common to mechanisms across the sciences. The core of this characterization consists in the identification of three elements, namely (1) responsibility for the phenomenon, (2) entities and activities and (3) organization. The paper examines these elements in some detail, using astrophysical examples in particular, and shows how they apply to various fields. Although different fields face different challenges, the proposed characterization makes it clear how common features of mechanisms contribute to a unifying project across the sciences.

While concurring with the claim that there is a unifying concept of mechanisms across the sciences, paper 2 argues that it is necessary to acknowledge that the kinds of mechanisms are very diverse. However, instead of just acknowledging and exemplifying this fact, paper 2 proposes a systematic taxonomy of mechanisms: Kinds of mechanisms can be identified by kinds of phenomena, kinds of entities, kinds of activities and interactions, and kinds of organization. Examination of this taxonomy will show that a given kind of mechanism often has exemplars in diverse sciences, thus demonstrating how the study of mechanisms can contribute to the project of unifying the sciences.

Paper 3 explores a striking kind of mechanism that occurs in completely diverse scientific contexts. Extreme events, such as financial market crashes, monster waves or hurricanes, occur surprisingly often even in the absence of any specific exterior causes. Since there are strong reasons for assuming that this is not just a matter of chance, it calls for an explanation in terms of interior interactions. Paper 3 argues that the unifying reason for the occurrence of such extreme events is the presence of certain kinds of mechanisms. However, the unexpected independence from micro details poses a challenge to the consensus characterization of mechanisms.

In total, the symposium shows that the focus on mechanisms to a large extent overcomes the apparent separation of different sciences and the respective philosophical sub disciplines. Since very similar mechanisms occur in different sciences it is most fruitful to investigate (kinds of) mechanisms across the sciences—thereby unifying science as well as philosophy of science.

*General Philosophy of Science*

**Till Gruene-Yanoff, Hanne Andersen and Mieke Boon**
***Teaching Philosophy of Science to Scientists: Challenges and Opportunities***

Many European countries have introduced legislation (e.g. Swedish “högskoleförordningen” and Finnish national graduate programs) that mandates courses in “Theory of Science” for Masters and PhD students. While such courses are often run by the students’ respective departments, in some institutions,  hilosophy departments have been coopted for this purpose.

This has posed an interesting challenge to philosophers: designing a course that addresses the abovementioned legislation, the requirements of the respective programs and the needs of its students, while also retaining a genuinely philosophical perspective. A standard philosophy of science curriculum, on the one hand, does not satisfy this purpose: it requires more philosophical background knowledge than science students commonly have, and it does not address typical methodological problems that science students face in sufficient detail. On the other hand, a typical “methodology” course within a specific discipline commonly focuses more on the technical mastery of specific methods than on the ways how scientific methods can be justified (as an extreme example, take LSE’s “Methods of Economic Investigations” course, which is a course in pure econometrics). The challenge thus is to address the methodological problems of diverse scientific disciplines with an appropriately unifying philosophical account.

In this symposium, philosophers from Twente (NL), Aarhus (DK) and Stockholm (SE) report on their experiences of designing and running such courses. Specifically, the symposium addresses two questions. First, can philosophy deliver a course that satisfy the above requirements and still remains genuinely philosophical? If yes, how does the curriculum of such a course look like? Second, what are the advantages, if any, of philosophers teaching such a course to an interdisciplinary audience, rather than subjectspecific scientists teaching methodology courses?

The symposium papers answer both questions in the affirmative and thus provide an important argument for the relevance of (parts of) philosophy of science to a wide audience.

**Learning Research - History and Philosophy of Science for the Engineering Sciences**

Mieke Boon, Department of Philosophy, University of Twente, The Netherlands

This contribution will describe a history and philosophy of science course for graduate students in the engineering sciences. The approach and content of this course has resulted from a research-project *Philosophy for the engineering sciences* funded by a grant from the Dutch National Science Foundation, and a close collaboration with research groups in the MESA+ Institute for Nanotechnology.

The general context is *scientific research in the context of technological applications.* Its general aim is developing in-depth understanding of scientific knowledge and scientific research. Our two-tiered approach, on the one hand *illustrates* by means of appealing and challenging historical examples 'how scientific theories were produced,' and on the other hand *clarifies* by means of a body of helpful philosophical ideas 'how scientific knowledge is made'.

General issues that justify the relevance of aiming at in-depth understanding of scientific knowledge and scientific research relate to 'becoming a better scientist', such as, adequate uses of scientific knowledge; adequate reading of the scientific literature; translating technological problems to scientific research; and working inter- or multidisciplinary.

In the research-project *Philosophy of the Engineering Sciences,* several of the 'common' notions discussed in the philosophy of science and commonly used in the language of scientific practices, have been reconsidered from the perspective of the engineering sciences. These notions are, for instance, phenomena, laws of nature, truth, observation, proof, explanation, scientific concepts, scientific discovery, fundamental theories, scientific models, instruments and experiments, and fundamental versus applied science. This approach has resulted in a preliminary conceptual framework that is more productive in understanding the engineering sciences as a scientific research practice. In this course, students learn to use this new conceptual framework.

Importantly different from traditional approaches in the philosophy of science is the focus on 'how scientific knowledge is constructed' (traditionally called the context of discovery), which I propose to call the *context of construction.* Similarly, one of our didactical aims is that students learn 'to think as a scientist.' Moreover, they learn that even our most fundamental theories have been constructed by studying historical texts of the great scientists (e.g., Newton, Faraday, Maxwell, Carnot and Prandtl). In brief, by learning to apply the new conceptual framework, students develop an understanding of how these scientists *constructed* their theories.

Based on the new conceptual framework, also a concrete conceptual tool is proposed for analyzing current scientific research. Based on analyses of many different scientific articles, it is suggested that most of the construction of knowledge concerns scientific modeling of phenomena, such as phenomena that are held responsible for the (dis-)functioning of technological materials, processes and devices. The conceptual tool enables analyzing these articles as models of phenomena – in turn, these models are considered as epistemic tools that enable e.g., reasoning about (e.g., intervening with) the phenomenon. It appears that these kinds of analyses enable better understanding of scientific articles, even in fields that are unfamiliar. Also, they facilitate structuring and explaining research-projects, and assists in inter- and multi-disciplinary communication.

**Philosophy of science and scientific proficiency**

Hanne Andersen, Centre for Science Studies, Dept of Physics, Aarhus University

In this talk I shall argue that philosophy, history and sociology of science as disciplines that study the sciences in their development can provide important insights on a variety of issues of importance to scientists, science educators, and science managers.

However, most previous efforts in bringing history and philosophy of science into science education has focused on K-12 education and on teacher education (Lederman 1992; Abd-El-Khalick and Lederman 2001; Lederman 2007), while only little emphasis has been on college education in science (Thoermer and Sodian 2002; Roth 1994; Halloun and Hestenes 1998) and almost none on graduate education in the sciences although occasionally scientists do ask that their graduate students be given philosophical guidance on the principles and wider role of science (Ziman 2001; Gauch 2003).

Hence, most analyses of philosophy, history and sociology of science and science teaching have focused on how it can improve scientific *literacy*. For example, it has been argued that in order to make informed decisions on socioscientific issues and in order to make sense of science in everyday life and appreciate science as a part of contemporary culture it is crucial to understand the nature of science in all its complexity (Lederman 2007), and inclusion of history and philosophy of science elements in the science curriculum have often been seen as important means to this end (Brush 1989; Matthews 1994; Solomon and Aikenhead 1994). Similarly, it has been argued that including in the science curriculum historical accounts of how scientific results have been achieved and put to use can help students overcome barriers in their encounters with science by humanizing it and making it less abstract and more engaging, by displaying connections between topics and disciplines of science, and by counteracting scientism (Matthews 1994).

In this talk, I shall focus on how philosophy, history and sociology of science can improve scientific *proficiency*. I shall report from the situation in Denmark where philosophy of science courses are obligatory in all bachelor programs, and based on my experiences from teaching a broad range of such courses (philosophy of medicine, public health, human biology, dentistry, medicinal chemistry and nanoscience) I shall describe some specific areas in which instruction in philosophy, history and sociology of science can indeed contribute in important ways to the education of scientists.

**For a Philosophically Based, Practice-Oriented Methodology of Science**

Till Grüne-Yanoff, Royal Institute of Technology (KTH), Stockholm

The philosophy department at the Royal Institute of Technology (KTH) in Stockholm teaches “Theory and Methodology of Science” (TaMoS) to about one thousand graduate students annually. Students come from very different programs, ranging from nuclear physics through vehicle engineering to media management. Most of the students are required to pass the 7.5 credits of this course.

Based on my experience from 5 teaching periods, I argue that there is a genuinely philosophical curriculum for such a course. Specifically, it focuses not on specific methods of particular disciplines, but rather identifies scientific practices that are pursued across many disciplines, like measuring, modeling, experimenting, or interpreting. Thus, while illustrating these practices with concrete examples, the emphasis is on a “middle level” between specific methods and the high level of abstraction known from standard philosophy of science curricula.

The main analytic thrust of the course is to identify the function of these practices in the light of general scientific goals – in particular prediction, explanation and control – and to develop justificatory schemata for these practices as means for these goals. Importantly, the course does not aim so much at giving justifications for specific methods, but rather teaches students how to develop and critically assess such justifications for their own purposes. Thus it teaches “methodology” as the systematic study of how to give reasons and justifications for scientific practices, adaptable to the contexts of different disciplines. That, I conclude, is a genuine philosophical topic, and best located in the hands of a philosophically trained teacher.

Furthermore, I argue that such a philosophical course has additional benefits over typical methodology courses offered by and for specific disciplines. In particular, a philosophical perspective allows to better abstract from problems with specific methods, hence offering a clearer distinction between methods and methodology. This in turn allows better identification of crucial justificatory issues through comparison and contrast of practices from different disciplines. In particular, it helps showing that there is a common approach to justification, neither determined by specific (disciplinary) domains nor by disciplinary traditions, but by a commitment to the general goals of science.

## *Philosophy of Physics*

**Michael Krämer, Michael Stoeltzner, Koray Karaca and Martina Merz**
***The Return of the Higgs Hunters: Epistemological Perspectives on the Large Hadron Collider***

More than previous experiments in elementary particle physics, the LHC (Large Hadron Collider) at CERN and the search for the Higgs particle involve important epistemological problems. They concern the presuppositions of knowledge acquisition about statistically rare entities, the identification and representation of interesting events, the theory-ladenness of a particle detector that searches for a specific particle and 'new physics' broadly conceived, the role of models and simulations in mediating between theory and experiment, the epistemic dynamics of a broad theoretical community focused at a large experiment, and the peculiarity of the Higgs mechanism within the standard model of elementary particle physics.

On July 4th, 2012, CERN announced the discovery of a new particle whose properties are consistent with the standard model of elementary particle physics (SM). Exploring the nature of this new particle has now become the key task of current and future LHC analyses and theoretical investigations. The crucial point in the argument for discovery was to exclude any other cause of the discovered signals beyond a level of 5 standard deviation units, a statistical criterion that was justified by signals of previous experiments that later turned out not to correspond to a new particle and applied to the large amount of data obtained during since the launch of the experiment. While it could be shown that the particle was a boson, other properties will only be corroborated after the present shutdown of the LHC.

This interesting situation to have obtained clear evidence, but no full experimental proof of a SM Higgs – as compared to a more complex process producing the same signal and in which the alleged Higgs boson is some kind of composite or intermediate state – provides the unique opportunity to study the above-mentioned epistemological questions in real time and present both the new physical findings and aspects of their philosophical interpretation to the first GWP meeting. The interdisciplinary symposium brings together philosophers of science, physicists, and sociologists of science who are members of a Wuppertal-based international collaboration.

Paper 1 reviews the SM Higgs mechanism and discusses theoretical constraints on the mass of the Higgs boson. Presenting the most recent results of the LHC, he evaluates their consequences for the SM and theories beyond the SM, among them supersymmetric models and models with dynamical symmetry breaking and composite Higgs particles. Paper 2 starts from a real-time map of the models in the Higgs sector and investigates its epistemic dynamics. It turns out that most Higgs or Higgs-like models entertain three types of mediating relationships: between the SM and the data, between the data and physics BSM, and in the sense that these generalizations reproduce the SM at low energies. Paper 3 analyzes how the novel and 'interesting' events are identified within an overwhelming mass of known physics in a particle detector, a procedure by which a lot of data is irretrievably lost. This yields problems for the exploration of 'new' physics. In paper 3 it is argued that suitable selection criteria can be established by demanding their robustness across the models and theories LHC is aimed to test. Paper 4 shows that in the process of distinguishing signal from background and evaluating confidence levels, visual representations play an important constitutive role, especially in the context of the prime analysis result. Other than for bubble chambers and the like, these pictures are not directly observable but arise from condensing the data of a large-scale project by employing various strategies of modeling and simulation.

*Models and Representations*

**Mathias Frisch, Rafaela Hillerbrand and Herman Russchenberg**
***Uncertainty in Climate Modeling***

There is a broad scientific consensus that human emissions of greenhouse gases have and will continue to change global and local climate. Determining the adequate response to these changes in the form of mitigation, adaption, reducing greenhouse gas emissions, or other countermeasures like geo-engineering hinge on climate projections derived from very sophisticated climate models. However is also well-known that many aspects of the climate system are not yet well-understood and climate projections contain large uncertainties.

This symposium brings together philosophers and climate scientists to examine different sources of uncertainty in climate modeling and their effects on policy making. Sources of uncertainty include uncertainties in initial conditions, uncertainty about the form of modeling equations and uncertainty in the values of parameters in the model. The aim of this symposium is to examine the epistemic status of climate models and their role in policy guidance by addressing the following questions: To what extent do these uncertainties affect policy-relevant predictions of climate models? Could these uncertainties be overcome through more reliable data or do they reflect inductive uncertainties that are an unavoidable feature of long-range predictions in complex and non-linear systems? Does the fact that climate models centrally involve computer simulations affect the epistemic status of the models' predictions or projections? How can we arrive at policy recommendations given the different predictions of different climate models? One approach to infer policy recommendations from climate models uses coupled economy-climate integrated assessment models to assess the effects of climate mitigation strategies through a cost-benefit analysis with the aim of determining the optimal strategy. What are additional sources of uncertainty faced by integrated assessment models? To what extent are integrated assessment models (and also climate models) value-laden? And are there considerations undermine the usefulness of these models to issue policy recommendations? What are alternative strategies for using climate models as policy guidance?

By addressing a question at the intersection of science, philosophy of science, decision theory, and ethics this symposium also provides and implicit answer to the overarching theme of the conference "How much philosophy in philosophy of science?” While not offering a quantitative answer, the symposium aims to show that there are philosophical questions that can fruitfully be addressed only by integrating an active engagement with a particular science, methodological tools central to general philosophy of science, and resources from other core areas of philosophy—in this case value theory.

## *Induction*

### Paul Thorn, Gerhard Schurz and Kevin Kelly

### ***Formal Approaches to the Problem of Induction***

Inductive inference is central to the formation of scientific theories, but the justification for inductive inference has proved elusive, so much so that C. D. Broad famously described induction as the "glory of science and the scandal of philosophy." This Symposium will bring together an international panel to present novel formal approaches to the problem of induction, including recent work relating to the justification of induction via meta-induction, a recent attempt to justify induction via direct inference, and an attempt to address the problem of induction via a non-circular justification of Ockham's razor.

First Presentation Title: Meta-induction a prediction and action strategy: some optimality theorems

Abstract: In this paper I continue my previous work on meta-induction as an optimal prediction strategy over finite sets of alternative prediction methods. In the first part I generalize my results to arbitrary actions (instead of predictions), whose utilities are not known and may change in time. I will show that applying the method of weighted average meta-induction to the utilities of observed actions will always improve one's utility in the long run, with possible short run losses whose worst case bounds are small and precisely calculable. In the second part I consider limitations of meta-induction when applied to countably infinite sets of action methods. I will argue that the question of the optimality of meta-induction for infinitely many action methods is related to Goodman's problem of induction, and I will explain how a variant of Kevin Kelly's version of Ockham's razor may provide a partial solution to this problem.

Second Presentation Title: Induction with or without Uniformity.

Abstract: I present an account of induction in the vein of proposals made by D. C. Williams and others, which locates the applicative aspect of inductive inference within direct inference. I then consider what it would mean for nature to be disuniform, and defend the thesis that inductive inference is reasonable absent the assumption that nature is uniform, by appeal to simple examples and commonly accepted principles of direct inference. I next defend the thesis that a limited from of inductive inference is reasonable, even when it is known that nature is disuniform.

Some may think that the preceding theses are secured only inasmuch as I disregard the problem of induction that was outlined by Goodman. To address such doubts, I outline a new response to the Goodman Problem that dovetails with the proposed defense of induction. I show that inductive inferences leading to the conclusion that all (or almost all) emeralds are grue, by appeal to a sample of green emeralds, are defeated. Such inferences are defeated, because they depend on a direct inference that is based on a reference class that incorporates insufficient information about the sample of observed emeralds.

Third Presentation Title: Ockham's Razor Explained.

Abstract: Ockham's razor---a systematic bias toward simple theories---is the most characteristic feature of scientific method. Bayesians explain Ockham's razor in terms of a prior bias toward simplicity. That is evidently circular. Frequentists explain Ockham's razor in terms of predictive accuracy. That also presupposes a prior bias toward simplicity, since simple theories are inaccurate if the truth is sufficiently complex. Is there a non-circular argument for Ockham's razor in science? We present a mathematically precise one that is not circular. The talk is aimed at a general scientific audience interested in the role of simplicity in scientific reasoning.

## *Causality*

**Michael Baumgartner, Vera Hoffmann-Kolss and Markus Eronen**

### ***Interventionism and Multi-Level Causation***

Theories of causation and causal explanation are among the central topics in contemporary philosophy of science. Currently, the interventionist theory of causation, prominently defended by Woodward and Hitchcock, enjoys wide acceptance as a pragmatic approach to analyzing the notion of causal explanation actually used by scientists working in various fields. The central idea underlying this conception of causation is that causes are difference-makers for their effects. Properties or states standing in a causal relation to each other are represented by variables taking values of a certain range, and a variable X is classified as causally relevant to a variable Y iff it is possible to carry out an intervention on X which changes the value or the probability distribution of Y (Woodward 2003; Woodward and Hitchcock 2003).

An alleged advantage of this account is that it avoids many of the metaphysical quandaries arising for traditional approaches to analysing causation, in particular, the question whether causation can only take place at the microphysical level or whether there can be genuine higher-level causation or inter-level causation. A number of authors argue that interventionism solves the problem of higher-level and downward causation (e.g. Menzies 2008; Raatikainen 2010; Woodward 2008). Yet, this claim is not uncontroversial (Baumgartner 2009), and currently, it is an open research question whether and how interventionism can suitably account for higher-level or inter-level causal relations.

In this symposium, we aim to explore this question by bringing together authors supporting opposing points of view. While the paper `Defending the Interventionist Solution to the Exclusion Problem' argues that, despite several arguments to the contrary, interventionism does indeed provide a viable approach to vindicating higher-level causation, the paper `Interventionism and the Proportionality Constraint' aims to show that higher-level causal claims raise more serious problems within the interventionist framework than is often assumed. The third paper, `Mutual Manipulability and Constitutive Relevance', discusses an application of interventionist concepts to constitutive relations occurring in mechanistic explanations, which can be considered as a particular case of inter-level relations. By combining these three approaches, we seek to gain a better understanding of the benefits and limitations of interventionism in the context of special science explanations.

Please contact the author for references.

*Philosophy of Biology*

**Christian Sachse, Ulrich Krohs and Ellen Clarke**
***Organisms and Biological Individuals – Metaphysical and Epistemological Reflections on the Current Debate***

Organisms seem to be the paradigmatic living things, both to our common sense view and within the biological sciences. However, despite the familiarity and the successful scientific use of the concept of an organism, there is an ongoing debate about its theory-dependence and about the ontological status of biological organisms. The debate was in part reframed in asking for what counts as a biological individual rather than what counts as an organism (Hull 1992). The concept of the individual is regarded as the better candidate for answering metaphysical questions about living beings mainly because every organism might be regarded as an individual – though not vice versa – and because evolutionary theory already might give the tools at hand which are required to individuate biological individuals, while no generally accepted theory of the organism is available. Here, however, we are already in the controversy (cf. Pradeu 2010).

One root of the debate is formed by cases that do not only challenge our common sense view of an organism, but also allow for diverging scientific answers, depending on which criterion for biological individuality is given priority. A well-known example concerns found fungus samples of the species Armillaria bulbosa. Morphologically, they may be regarded as separate individuals. Due to their genetic similarity and underground connections, they might be rather seen as parts of only one individual gigantic fungus that takes over a region of fifteen hectares, has a biomass of more than 10 tons and is older than 1500 years – which would make it one “among the oldest and largest organisms on earth” (Smith et al. 1992). Numerous similarly debated examples can be found in literature (cf. Wilson 2005, ch. 3; Clarke 2012).

The debate has another, more fundamental root in problems with defining meronomic or parthood conditions for biological individuals and their components. Similarity of components can neither be a sufficient nor a necessary condition for two parts to belong to the same individual, as the example of twins intuitively illustrates: even though twins do have (e.g. genetically) similar cells, twins are generally taken to be two distinct individuals. Sober therefore suggests to focus on functional interdependence: two (even different) parts of one organism/individual that causally interact in a characteristic way differ from causal interactions between parts of different organisms/individuals. However, it seems evident that “organisms differ widely in the degree of functional interdependence that unites their parts” such that the status of biological organisms and that of biological individuality cannot be a yes/no affair but comes in degrees, with no precise boundary with respect to both the synchronic and the diachronic dimension (Sober 2000, p. 154). Of course, this is not where the debate ended but where it really starts, and were it becomes evident that different roles of the concept of an organism may not so easy be united: 'Organism' is an important sortal term in biology, which singles out individuals on a certain level, but it also conceptualizes what makes something to be a living thing.

Against this background, the aim of this symposium is threefold. The first talk will discuss recent approaches from a metaphysical point of view and argue for the theoretical possibility of evolutionary theory to clearly define organisms and biological individuals. The second talk discusses the relationship between the problem of defining the organism and that of explaining major transitions in evolution, arguing that several apparent problems with the notion of transition arise from conforming to a falsely synchronic and categorical organism concept. The third talk proposes viewing the concept of an organism as an epistemic tool rather than as a natural kind term and thus questions that its use has the strong metaphysical implications which are discussed in the first two talks.

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| **10:30<br>-<br>11:00** | | Coffee Break |
| **11:00<br>-<br>13:00** | | Symposium (R 013):<br>*The New Mechanical Philosophy*<br><br>Sessions:<br>(i) Philosophy of Chemistry & Technology (R 116)<br>(ii) Induction (R 415)<br>(iii) Cognition and Concepts (R 703)<br>(iv) Models and Representations (R 009) |
| **13:00<br>-<br>15:00** | Registration | Lunch Break |
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