SPSP (Society for the Philosophy of Science in Practice), and;
CEPTES (Center for Philosophy of Technology and Engineering Science)

University of Twente
P.O.Box 217
7500 AE Enschede
The Netherlands
tel. +31 53 489 80 31

www.ceptes.nl
www.gw.utwente.nl/spsp/

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CONTENT

PROGRAMME COMMITTEE

Mieke Boon
University of Twente
M.Boon@gw.utwente.nl

Hasok Chang
University College London
h.chang@ucl.ac.uk

Marcel Boumans
University of Amsterdam
M.J.Boumans@uva.nl

Rachel Ankeny
University of Sydney
rankeny@science.usyd.edu.au

Henk de Regt
Vrije Universiteit Amsterdam
h.w.de.regt@ph.vu.nl

PLENARY PRESENTATIONS

1. EVIDENCE FOR USE

Nancy Cartwright
London School of Economics and Political Science

Everywhere we hear the call for evidence-based policy. But what is evidence for evidence-based policy? Theories of evidence in philosophy of science have two failings when it comes to answering this question. First, most pure scientific disciplines have fairly well understood methodologies that dictate what counts as acceptable evidence and most of our philosophical accounts of evidence are based on studies of these methods within the sciences. But policy, like other areas outside 'pure' science, raises problems that generally do not fit neatly with any one or another of these methodologies and our philosophical theories of evidence are concomitantly ill equipped to discuss evidence here. Second, many of our theories are based on knowledge of probabilistic relations involving evidence and hypothesis (like likelihoods or relevance relations). But this puts the cart before the horse. Policy needs an account of what constitutes evidence to use as a guide for how to set or estimate these probabilities in the first place. Alternative accounts based on explanatory connections are also often of little concrete help because they don't offer practicable advice about how to tell if there is an explanatory connection. This paper will discuss these problems and make a plea for a theory of evidence that is at once philosophically principled and practicable.

2. Two Traditions in Scientific Practice

Mieke Boon
University of Twente

Current laboratory sciences have emerged from two scientific traditions. Kuhn (1976, 1977) describes mathematical and experimental traditions which have developed distinctly from one another. The mathematical approach arose in the Hellenistic world in the fifth century B.C. Classical mathematics was dominated by geometry, and was conceived as the science of real physical quantities, especially spatial. The experimental tradition arose in the seventeenth century, with fields such as electricity and magnetism, heat, and chemistry. In this tradition—often referred to as the Baconian tradition, after Francis Bacon—experiments were performed in order to see how nature would behave under previously unobserved, or previously nonexistent, circumstances.

The two traditions have different metaphysical presuppositions. The metaphysical presuppositions of the experimental tradition are corpuscular-mechanical ideas. The discovery of mathematical regularities in nature has licensed a metaphysical interpretation as well, which is that laws of nature are the formal causes, the terminus of the scientific causal chain.

The two traditions are still familiar. Most of us were introduced to physics and chemistry along these lines. Chemistry teaches us the corpuscular-mechanical ontology, with mathematics as a mere tool. Physics teaches us the laws of nature, with causal explanation as a heuristic tool.

I claim that the most successful scientific practices have succeeded in integrating the two traditions. However, philosophical problems arise for a realist interpretation of science, in which they appear to be inconsistent. A more coherent and productive picture of actual scientific practice arises from an anti-realist picture in which the two approaches are interpreted as different scientific ways of structuring and interpreting the world. Accordingly, the mathematical approach conceptually structures the world in terms of mathematical patterns and aims at interpreting these patterns in terms of a small number of mathematical axioms or principles. The experimental approach conceptually structures the world as physical phenomena and aims at explaining these physical phenomena in corpuscular-mechanical terms.

I count several branches in the engineering sciences as particularly successful in this respect. They have succeeded in integrating these two basic ways of structuring and interpreting the world. However, we cannot find out about this integrated approach by the mere study of textbooks, since one of the two approaches usually still dominates the topic. The problem with textbooks is that they purport to be literally true about the world, whereas they usually are only literally true about a phenomenon that is co-constructed in the scientific explanation. As a result, textbooks present theories as both true and explanatory; they typically do not reveal that the consequences derived from these theories are usually not true about the empirical facts. This is what we find out in actual scientific research practices. Successful scientific practices integrate the two approaches in order to construct scientific knowledge that actually is both explanatory and true about the world.

3. The Abstract Individual in Nature and Society

John Dupre
University of Exeter

Thinking of the biological world as composed of discrete and autonomous individuals seems so natural to us as hardly to demand justification, and this is even more so for the social world. Yet in fact there is no unique and obvious way of dividing the biological world into individual organisms. While it might be hard to imagine any alternative to the division of the social world into familiar human individuals, the assumption that these individuals, rather than more inclusive social entities, are invariably the correct location for agency, choice or value is certainly questionable. It seems likely that this assumption is reinforced by the belief that the human individual is just an instance of a much more general and natural division of the biological world into similarly differentiated individuals. I shall suggest that the concept of the individual is, therefore, one of the crucial points at which partially ideologically shaped assumptions flow between biology and social theory.

4. Articulating the World: Experimental Systems and Conceptual Understanding

Joseph Rouse
Wesleyan University

Recent philosophy of science has become increasingly disconnected from philosophical work in other fields: many other philosophers often rely upon implicit conceptions of science disconnected from actual work in the philosophy of science. The philosophy of scientific practice can potentially rebuild this connection by showing how attention to practice can contribute to a broader philosophical understanding. I illustrate this theme by showing how work on scientific practice contributes to a classic philosophical issue, concerning the relation between conceptual understanding and empirical engagement with the world. Standard formulations ask how conceptual spontaneity is constrained at its boundaries by experience (or causal interaction). I argue instead that experimental interaction with the world is integral to conceptual articulation in scientific practice, giving special attention to how new domains of scientific understanding are opened to conceptual understanding. Recognizing how experimental work and conceptual articulation are intertwined has significant implications for philosophy of language and mind more generally.

5. The Philosophical Grammar of Scientific Practice

Hasok Chang
University College London

Even those scholars who are deeply concerned about scientific practice have tended to use rather haphazard or limited frameworks for description and analysis. I seek to provide a more considered, systematic and comprehensive template — a philosophical grammar of scientific practice, "grammar" as meant by the late Wittgenstein. For this purpose I take and freely adapt insights from the traditions of pragmatism, operationalism and phenomenology, and also from some contemporary authors such as Ian Hacking, David Gooding and Marjorie Grene. I begin with the recognition that all scientific work, including pure theorizing, consists of actions — of the physical, mental, and "paper-and-pencil" varieties. When we set the basic focus of study on action, to see what it is that we actually do in scientific work, a set of questions naturally emerge: who is doing what, why, and how? More specifically, we must arrive at some coherent philosophical accounts of the following elements of scientific practice: (1) the agent — free, embodied, and constantly in second-person interactions with other agents; (2) the purposes and proximate aims of the agent; (3) types of activities that the agent engages in, everything from calculating to smelling, from glassblowing to computer simulation, from synthesizing specific pharmaceuticals to explaining the structure of the universe; (4) ontological principles necessarily presumed for the performance of particular activities; (5) instruments and other resources that the agent pulls together for the performance of each activity. Looking ahead to further development and application of the framework developed here, I will finish with some illustrative contrasts between the more traditional descriptions of scientific practice and the kind of descriptions enabled by the proposed framework.

SYMPOSIA

SYMPOSIUM 1: DOWN TO EARTH: PHILOSOPHY OF THE GEOSCIENCES

Maarten G. Kleinhans
Utrecht University

Philosophical analyses of the geosciences have been quite scarce, on the one hand because philosophers of science deemed it a relatively uninteresting discipline, on the other hand because most practitioners of the geosciences have eschewed philosophy. This session aims to contribute to the development of a philosophy of the geosciences that is based on analysis of actual geoscientific practice. The focus of this session is on the specific nature of the methods employed in geoscientific research. In particular, we investigate the function and status of modelling strategies (esp. physico-mathematical and numerical modelling), which play a crucial role in geoscientific explanation. In addition, we analyse the nature of geoscientific experimentation and compare it with experimentation in other sciences.

1. Computer Models and Inference to the Best Explanation in Geosciences

Maarten G. Kleinhans
Utrecht University

Henk W. de Regt
VU University

Geosciences purport to describe and explain the (history of) inanimate processes on earth. Theories of geoscience are typically hypotheses about unobservable (past) events or generalized - but not universally valid - descriptions of contingent processes. Geosciences combine various forms of narrative explanation with causal explanation, and typically rely on Inference to the Best Explanation (IBE). The narrative part of the IBE describes the actual sequence of events in the past ("how the phenomenon came about"), while the causal part refers to a common cause of several pieces of evidence. Models are essential in geoscientific IBE: they are invoked to test whether the common cause is not in conflict with the laws of physics and chemistry.
In the present paper we investigate the role of physico-mathematical computer models in geoscientific explanations by focusing on models predicting behavior of sand bars near the coastline. During storms (large waves) these bars migrate offshore and during calm weather (small waves) the bars migrate onshore. The migration rate differs orders of magnitude between different beaches. Models are developed in order to answer two fundamental questions: Why do the bars exist? Why and how fast do they migrate in different directions in calm weather and storms?
We analyze two papers that attempt to explain this phenomenon. The first paper (Hoefel & Elgar, Science, 21 March 2003) invokes a hitherto ignored and ununderstood physical mechanism to explain the migration of these bars. The second paper (Ruessink et al. forthcoming in J. Geophysical Research) shows how observed trends in data of three sites from different continents can be modelled by three basic and well-understood physical mechanisms (implemented in a numerical computer model). The trends are sensitive to

choices of model parameters (which are empirical parts of physical laws such as friction and drag coefficients) and to the specified boundary conditions. Furthermore, the precise values of the model parameters are site-specific and cannot be known in advance, and the boundary conditions, such as a time series of offshore wave height and period, have measurement uncertainties. Hence, the model can be optimized in various ways to reproduce the data by adapting the poorly constrained model parameters or boundary conditions, or by adding or removing a physical mechanism from the model. This contradicts the claims by the first paper where the uncertainty of the model parameters and boundary conditions have been ignored.

The predicted phenomena are underdetermined by measurement uncertainties of model parameters and boundary conditions, which leaves room for different, partly incompatible physical explanations of the phenomena. The modeling demonstrates that the phenomena can in general be explained by a suite of physical mechanisms, but the precise contribution of each mechanism remains unconstrained. The mechanism proposed by Hoefel & Elgar might be important but Ruessink et al show that they have not demonstrated this unambiguously. In sum, the explanation of the observed phenomena - which physical mechanisms underly the dynamics of sand bars – is hampered by problems of underdetermination.

2. Historical Contingency, Underdetermination, and Numerical Modeling

Derek Turner
Connecticut College

Philosophers and logicians usually think of necessity and contingency in broadly logical terms. According to the standard view, a necessary truth is a statement that is true in all possible worlds. A necessary falsehood is a statement that is true in no possible world. And a contingent truth is a statement that is neither necessarily true nor necessarily false. Yemima Ben-Menahem (1997) has argued that this standard way of characterizing necessity and contingency is not very helpful to historical researchers. In the context of historical research, she thinks it is more helpful to conceive of necessity as insensitivity to initial conditions, and contingency as sensitivity to initial conditions.

To borrow an example from Elliott Sober (1988), imagine a person holding a ball standing on the rim of a giant bowl. When the person releases the ball, it rolls down and eventually comes to a rest at the center of the bowl. The final position of the ball is relatively insensitive to the initial conditions. That is, it makes no difference where along the rim the ball is released; it will always come to rest in the same place. The ball's final resting place is historically necessary, in Ben-Menahem's sense of 'necessary'. This is also closely related to the issue of underdetermination. Hypotheses about the point of release are underdetermined by the observation that the ball is resting at the center of the bowl.

In this paper, I begin by presenting and defending Ben-Menahem's conception of historical necessity and contingency. I then argue that this distinction can help us to understand the practice of numerical modeling in paleobiology and geology. Although numerical modeling plays an important role in historical science, it cannot help researchers perform direct tests of claims about the past. Modelers have to assume that the simulation represents past processes in the relevant ways. Yet numerical experiments can be used to test ideas about the (relative) necessity/contingency of historical events and processes. By conducting multiple trials while varying initial conditions, earth scientists can use numerical models to determine the degree of sensitivity of subsequent developments to those initial conditions. For example, they might do several runs of an ice sheet model in order to see whether (and if so, to what degree) the extent of glaciation depends on initial levels of carbon dioxide in the atmosphere. This sort of experimentation cannot tell scientists how much carbon dioxide was in the atmosphere at any given time in the past; nor can it reveal the extent of glaciation at any time in the past. Instead, it is (among other things) a way of studying historical contingency and necessity.

Since (as I will try to show) historical necessity and epistemic underdetermination go hand-in-hand, numerical experiments may also help researchers to ascertain the severity of underdetermination problems. For example, if a model shows that subsequent developments (e.g., the extent of glaciation) are insensitive to initial conditions (e.g., carbon dioxide levels), that means that we cannot infer the initial conditions from the later conditions.

3. Experiments in the Geosciences: Caging the Phenomenon?

Robert Inkpen
University of Portsmouth

Comparing the nature of experiments in the 'hard' sciences and in the geosciences a number of important differences become clear. Hacking (1983) notes that an important role for experimentation is creation of phenomena. He then argues that the phenomena created, such as the Hall effect, does not exist outside of the confines of certain kinds of apparatus (Hacking, 1983, p,226). Phenomena tended to be viewed as isolated instants of reality. Isolation of a single phenomenon, in the sense used in 'hard' sciences, is not necessarily a major goal in the geosciences. Geosciences are more concerned with trying to understanding effects rather than identification of general phenomena. Using an abductive approach, geoscientists are concerned with identifying plausible explanations for observed effects. Within the geosciences there is a general assumption that experimentation can aid in understanding the complexity of reality by constraining that reality. Phenomena and reality are not so much seen as being created more as being caged. Researchers tend to believe that they are simplifying reality, but not beyond the point where it does not produce the effect of interest. Reality performs to the specifications of the experimenter in the experiment, but still has an existence independent of the experiment. Changing the constraints, altering the cages for the performance, is viewed as providing new insights into how reality operates rather than creating something wholly new. Within the study of salt weathering, for example, a continual refinement of experimentation can be observed from identification of the potential impact of salt upon rock (e.g. Goudie, 1974; Sperling and Cooke, 1985,), to more complex assessments of the sensitivity of salt weathering processes to environmental conditions (e.g. Rodriguez-Navarro and Doehne, 1999). Exploring such experimental work illustrates that reality is manufactured in such experiments. In addition, changing the cages, does alter the type of reality manufactured making the assumption of a common frame of reference for experimental effects more difficult to sustain.

Cooke, R.U. and Sperling, C.H.B. 1969. Laboratory simulation of rock weathering by salt crystallization and hydration processes in hot arid environments. Earth Surface Processes and Landforms, 10; 541-555.
Goudie, A.S. 1974. Further experimental investigation of rock weathering by salt crystallization and other mechanical processes. Zeitschrift fur Geomorphologie, Suppl. 21; 1-12.
Hacking, I. 1983. Representing and intervening. Cambridge University Press. Cambridge.
Rodriguez-Navarro, C. and Doehne, E. 1999. Salt weathering: Influence of evaporation rate, supersaturation and crystallization pattern. Earth Surface Processes and Landforms, 24; 191-209.

SYMPOSIUM 2: CHALLENGING THE HIERARCHY OF EVIDENCE IN EVIDENCE-BASED MEDICINE

Robyn Bluhm
University of Western Ontario

Overview: The evidence-based medicine (EBM) movement of the past decade and a half advanced the radical suggestion that the practice of medicine should shift from a reliance on authority, intuition and clinical experience to a basis in research evidence. EBM is widely regarded as "the conscientious, explicit and judicious use of current best evidence in making decisions about the care of individual patients" (1) and has evolved into a vigorous movement spanning all areas of healthcare. At its methodological and epistemic core is the 'hierarchy of evidence,' a pre-graded ranking of clinical methodologies that places the evidence produced by randomized controlled trials on top, while maligning non-randomized and uncontrolled methods. (2) In this session, the participants draw on work in philosophy of science to explore the methodological, theoretical and social limitations of the evidence hierarchy of EBM and consider the implications of these limitations for clinical practice.

1. Sackett, DL, Rosenburg, WM, Gray JA, Haynes RB, Richardson, WS. "Evidence-based Medicine: What it is and what it isn't." British Medical Journal (1996) 312: 71-72. For earlier statements, see: Evidence-based Medicine (EBM) Working Group, "Evidence-based Medicine: A new approach to teaching the practice of medicine," JAMA (1992) 268 17: 2420-5, and Guyatt GH, Sackett DL, Cook DJ [Evidence-based Medicine Working Group] "Users' guides to medical literature," JAMA (1993) 271: 56-63.
2. According to standard versions of the hierarchy for medical treatments, meta-analyses of large-scale, double-blind randomized controlled trials (RCTs) produce the highest quality of evidence, while non-randomized trials and observational studies produce mediocre evidence and case series, case studies and anecdotal evidence produce the lowest quality evidence. Oxford Centre for Evidence-based Medicine: http://www.cebm.net/levels_of_evidence.asp (Accessed Nov. 19, 2006).

1. The Hierarchy of Evidence and Biomedical Research

Robyn Bluhm
University of Western Ontario

Although in principle EBM is supposed to operate with a broad definition of evidence, on which "any empirical observation about the apparent relation between events constitutes potential evidence," (1) in practice the evidence considered by EBM comes from its "hierarchy of evidence." This hierarchy places clinical trials, which borrow from the methods of epidemiology, above studies examining physiological mechanisms, and favours randomized over non-randomized clinical trials. The hierarchy also "implies a clear course of action for physicians addressing patient problems: they should look for the highest available evidence from the hierarchy." (2) In this paper, I argue that, as it stands, the hierarchy of evidence provides a view of medical research that will ultimately limit the progress that can be made in biomedical research (and thus in medical practice). I further suggest an alternative characterization of the relationship between different types of biomedical research that (1) replaces the focus on the randomized/nonrandomized dichotomy in clinical research with a more nuanced understanding of methodological choices and (2) draws more closely on epidemiology in integrating population-based research with research into causal mechanisms underlying disease.

1. Evidence-Based Medicine Working Group. Users' Guide to the Medical Literature,
2. G. Guyatt and D. Rennie (Eds.) AMA Press, 2002., p. 6
Ibid., p. 8

2. The Value(s) of Objectivity in Evidence-based Medicine

Kirstin Borgerson
University of Toronto

This paper investigates a central assumption underlying the hierarchy of evidence offered by EBM, namely that evidence derived from research methodologies ranked higher on the hierarchy is more objective than evidence below. Objectivity is regularly used to signify "everything from empirical reliability to procedural correctness to emotional detachment."(1) Heather Douglas has recently identified eight distinct senses of objectivity in common use. (2) I draw upon her careful catalogue in order to characterize the sense(s) of objectivity implicitly and explicitly assumed by proponents of EBM in the design of the evidence hierarchy. I raise some concerns about the potential dangers of relying exclusively on these mechanisms for producing objectivity, particularly in terms of their inability to address the influence of unidentified social values on science. I argue that an over-reliance on procedural objectivity has led proponents of EBM to the false belief that methodology alone (narrowly construed) can secure objectivity, and also to the related and even more problematic belief that guidelines produced on the basis of the evidence hierarchy provide an objective basis for medical decisions. Finally, I draw upon philosopher of science Helen Longino's comprehensive account of contextual objectivity and some of the early writings from EBM proponents in order to suggest ways in which EBM might be improved.

1. Daston, L., Galison, P., "The Image of Objectivity," Representations 40 Special Issue: Seeing Science (Autumn 1992): 82.
2. Douglas, H., "The Irreducible Complexity of Objectivity," Synthese 138 (2004): 453.

3. Iconoclast or Creed? Objectivism, Pragmatism and Evidence-Based Medicine's Hierarchy of Evidence

Maya Goldenberg
University of Toronto

Because "evidence" is at issue in EBM, the movement has been largely critiqued on postpositivist grounds, where the critics have drawn from the work of Quine, (1) Kuhn, (2) or Popper (3) to demonstrate the untenability of the objectivist account of evidence underscoring the evidence- based approach. While these post-positivist critiques seem largely correct to me, I propose that the critics miss important and desirable pragmatic features of the evidence-based decision making technology. I redirect critical attention toward EBM's rigid hierarchy of evidence as the culprit of its objectionable epistemic practices. Reframing the EBM discourse in light of a distinction between objectivist and pragmatic epistemology will allow for a more nuanced analysis of EBM than previously offered: one that is not "either/or" in its evaluation of the decisionmaking technology as either iconoclastic or creedal.

1. Djulbegovic, B. "Evidence and Decision Making: Commentary on M. R. Tonelli (2006)." Journal of Evaluation in Clinical Practice 12 (2006): 248-256.
2. Harari, E. "Whose Evidence? Lessons from the Philosophy of Science and the Epistemology of Medicine." Australian and New Zealand Journal of Psychiatry 35 (2001): 724-730.
3. Shahar, E. "A Popperian Perspective of the Term "Evidence-Based Medicine." Journal of Evaluation in Clinical Practice 3 (1997): 109-116.

SYMPOSIUM 3: THE ROLE AND IMPACT OF THE 'PHILOSOPHY OF SCIENCE IN PRACTICE' ON DESIGNING APPROACHES TO HIGHER SCIENCE EDUCATION

Ismo T. Koponen
University of Helsinki

Agustín Adúriz-Bravo
Universidad de Buenos Aires

In higher science education (post-compulsary secondary, tertiary, college, university... involving students 16+ years old), including especially pre-service science teacher education, we detect a need to find ways to address the topics of knowledge production and justification in science –topics that may eloquently be called the 'nature of science' (NOS). Recent findings in educational research concerning higher science education have convincingly pointed to the need to address NOS in an explicit and carefully designed way. This becomes of the utmost importance in the case of science teacher education, since it has been shown that teachers' deficient and incomplete NOS views reflect unfavourably in science education at school in all the educational levels.

Current literature in didactics of science (i.e., science education) and in higher education has, however, been narrow and limited in scope when it comes to the philosophical underpinnings of knowledge production and justification that are suggested for teaching purposes. Research in these fields, we think, would enormously benefit from a philosophical orientation firmly rooted in an examination of the practices of science; we are talking, then, of a naturalised approach to NOS. One key thing that needs to be discussed in higher education are the 'as authentic as possible' views on the nature of scientific knowledge, methods, validity and evolution. Towards this direction, the philosophies of science that pay attention to how science actually functions give an excellent starting point and bears promises in our field of work.

In this symposium, different aspects related to connecting science, didactics of science and the philosophy of science for a more authentic science education are discussed. As we have said, the main focus is on didactical research with the aim of laying epistemological foundation that permit to improve higher science education (with strong references to science teacher education). In the contributions, the theme of modelling is in focus, since, in recent developments in science education, theoretical models have been acknowledged to play a central role. This shift towards models in policy (science curricula), practice (science teaching) and reflection (didactics of science) of course mirrors the momentum that model-based views are gaining in recent philosophy of science.

The five contributions to this symposium are connected by two conceptual and discursive threads: 1. all of us authors are engaged in teaching the philosophy of science to non-philosophers, since we deal with the introduction of NOS in our educational practices having as audience students and teachers; 2. we all recognise the need to turn to recent and contemporary philosophies of science for our proposals, being especially interested in the cognitive turn, the semantic view and model-based approaches.

1. Using Recent Philosophical Views on Science Practice to Design More ‘Authentic’ Science Education: The Model-Based View

Ismo T. Koponen
University of Helsinki

Agustín Adúriz-Bravo
Universidad de Buenos Aires

Background. In science education, the idea of resorting to the history and philosophy of science (HPS) in supporting the construction of ‘solutions’ to didactical problems is a traditionally acknowledged and appreciated stance, already put forward by eminent scientists/philosophers such as Ernst Mach and Pierre Duhem. Today, ‘HPS’ is a fast-growing research area within didactics of science dealing with the contributions of the meta-sciences to science education in its different aspects: didactical research, curriculum- and unit-design, production of materials and teaching strategies, teacher education (McComas, 1998).

Focus. HPS has drawn attention to the need of developing a more ‘authentic’ science education, i.e. a school scientific activity that, on the epistemological level, would share as much as possible with science practices in the academia. The nature of science, especially the ‘praxiological’ aspect of natural sciences, may illuminate science teaching practices. Therefore, HPS should be a component of pre- and in-service science teacher education. However, it should be noted that, in designing suitable solutions for science education, science practices cannot be transferred as such for teaching purposes; instead, didactical transpositions are needed (Izquierdo & Adúriz-Bravo, 2003).

Working hypotheses. 1. The nature of science (NOS) is not only a worthwhile source of contents to be taught in science education; its study also provides ‘hints’ on how to teach science and enact more authentic practices at all educational levels. In particular, HPS helps in finding didactical approaches to make the ‘modelling’ aspect of physics more meaningful for students. 2. Didactics of science has extensively resorted to the new philosophy of science of the 1950s, 60s and 70s for NOS insights along the line of what constitutes the scientific method to be transferred to educational practices. More recent philosophical approaches (namely, the cognitive turn, the semantic view, the model-based approach) can provide very powerful intellectual tools for a more authentic science education. In particular, we are interested in authors such as Ronald Giere (1988), Javier Echeverría (1995), Ian Hacking (1983) and others for their contribution to the understanding of different key aspects (semantic, praxiological, rhethorical, axiological...) of modelling processes in science.

Theoretical proposal. In this presentation, we will develop some key aspects of what has begun to be known as the ‘cognitive model of school science’ (Izquierdo & Adúriz-Bravo, 2003), especially focussing on experimental intervention and on the construction of evidence in science practices, which we consider to be two of the main epistemological features of modelling.

Practical proposals. Our theoretical approach has led us to designing several ‘interventions’, especially at the level of pre-service science teacher education, which will be briefly mentioned here and shall be object of other specific communications.

Echeverría, J. (1995). Filosofía de la ciencia. Madrid: Akal.
Giere, R. (1988). Explaining science. A cognitive approach. Chicago: University of Chicago Press.
Hacking, I. (1983). Representing and intervening. Introductory topics in the philosophy of natural science.

Cambridge: Cambridge University Press.
Izquierdo, M. & Adúriz-Bravo, A. (2003). Epistemological foundations of school science. Science & Education, 12(1), 27-43.
McComas, W. (ed.) (1998). The nature of science in science education. Rationales and strategies. Dordrecht: Kluwer.

2. Models and Modelling in Physics and Physics Education: Philosophical Underpinnings and Suggestions for Revisions

Ismo T. Koponen
University of Helsinki

Agustín Adúriz-Bravo
Universidad de Buenos Aires

Science education research, which sets as one of its main goals the elucidation of an 'authentic' picture of science in order to transpose it for educational purposes, has gained much insight from model-based views on science, where models and modelling are considered to play the central role in the formation, justification, systematisation and communication of knowledge. Within this view, a question of the utmost importance for science education purposes is how models make a connection with the real world. Such question has been previously discussed under the philosophical lense of scientific realism, a choice that is often preferred on the basis of the opinion that most practising scientist tend to adopt a realist stance (Grandy, 2003).

On the other hand, the emerging model-based view of science education, which strives for 'authenticity' in science teaching practices, is currently seeking support from philosophical positions related to the Semantic View of Theories (SVT). These recent advances are promising steps towards establishing a robust and coherent philosophical framework, and are promising candidates for a philosophical background for science education. However, we think that, at least in the case of physics education, a too direct realist conception of models, as well as the SVT itself, need some revision for the following reasons (Koponen, in press): firstly, the SVT is still too limited to acknowledge the required 'semi-autonomy' of models from theories; and secondly, the SVT does not give a completely adequate picture of how the relation between models and the experimentally accessible phenomena to be modelled is bi-directional: phenomena are not only abstracted (modelled) but also 'fitted' into models, and this is done through designing laboratory experiments, which isolate the phenomena of interest. Design of experiments and isolation of phenomena are heavily theory-guided and involve elaborate modelling tools.

It is suggested here that, in physics education, attention needs to be drawn to the notion of the empirical reliability of models and modelling, and to the methodological question of how empirical reliability is established in the process of making a match between theory and experiment. The suggested picture –intended for the purposes of physics teacher education– replaces the current, more limited philosophical frameworks used in science education with one of a wider scope. Moreover, this 'revised' philosophical background gives a more 'authentic' picture of the working of physics as a science, and of how modelling activities are conducted, than other current stances in science education. Such framework may also bring nearer the views of instrumentalism and moderate realism, which perhaps would both be needed to capture philosophical nuances in the practice of science education.

Grandy, R.E. (2003). What Are Models and Why Do We Need Them? Science & Education, 12, 773–777.
Koponen, I.T. (in press). Models and Modelling in Physics Education: A Critical Re-analysis of Philosophical Underpinnings and Suggestions for Revisions. Science & Education.

3. Findings on practicing physicists' use of analogy will help science educators train students to think scientifically

Ciara A. Muldoon
University of Bath

As Nancy Nersessian remarks: "we will be more successful at training students to think scientifically if they are taught, explicitly, how to engage in the modelling practices of those with expertise in physics." This paper presents empirical data on practicing physicists' use of analogy to conceptualise and communicate physics. It focuses on the different forms of analogy used by physicists in different contexts. The data stems from on an on-line questionnaire on visualisation, analogy and computer simulations in physics; follow-up e-mails with a select sample of respondents; observation of public lectures, and interviews with two prize-winning physicists.

The findings show that analogical reasoning is an important model-based reasoning technique used by many physicists to explore new ideas and to bridge conceptual divides between, experts and novices, scientists from different disciplines, and specialists within the physics community. Formal, mathematical analogies are frequently employed in theory building and when communicating with experts, as the analogies hold for a hierarchy of relations. Playful analogies with physical and/or pictorial features are often used by physicists to informally communicate novel ideas to colleagues, funding bodies, students or the public.

The pedagogic value of analogical reasoning allows physicists to explain the unfamiliar in terms of the familiar. Unfortunately, this familiarization is both a strength and weakness, since misunderstandings caused by inappropriate use of analogy are often difficult to oust. Use of analogy should not be outlawed, but physics teachers, like practicing physicists, need to maintain a deliberate, structured approach to the use of analogies, to minimise naive interpretations. I will present an example of a playful but well thought out analogy which has features which may make it very useful in an educational context. For example, it is amusing, follows an easily visualised narrative, contains little literal similarity but holds for a whole series of relations between the source and target domains.

I believe that an editable, searchable, online database containing these kinds of playful but well-structured analogies (and also clearly identifies the limitations of the analogies) would be a useful educational resource. Compiling such a database would ideally involve collaboration among creative practicing physicists, science teachers, cognitive scientists, historians and philosophers of science, and science studies researchers. Analogy might be a double-edged sword, but when wielded in a balanced, coordinated way, it can open up exciting new vistas.

Nersessian, N.J. (1995), "Should physicists preach what they practice? Constructive modelling in doing and learning physics." Science & Education 4:203-226, pp. 204 – 205.

4. Values and Science: Enhancing Science Education Through a Philosophical Stance

Agustín Adúriz-Bravo
Universidad de Buenos Aires

Ana C. Couló
Universidad de Buenos Aires

There is growing consensus among science teachers, teacher trainers, researchers, curriculum designers and educational policy makers on the significant role of the philosophy of science (together with the history and sociology of science) in improving the teaching of science both to students that will go on further science studies, and to students for whom school science is their only access to scientific literacy. The reasons of such significance may lie on different dimensions: the philosophy of science serves intrinsic aims (it provides conceptual elements leading to a better appraisal of the contribution of science to human culture and to a critical assessment of its nature), cultural aims (it constitutes a valuable intellectual creation and contributes to the formation of educated citizens), and instrumental aims (it aids science teaching and learning, for instance, by identifying obstacles to the development of scientific knowledge both in individuals and in society) (Adúriz-Bravo et al., 2003).

Once the relevance of the philosophy of science to science teaching is admitted, we can discuss what issues of the nature of science (NOS) are useful for the aims above. Among such issues, the role of values and judgement in the work of scientists has been long debated. For instance, we can discuss whether the acceptance or rejection of scientific constructions is bound by logical, rule-driven inferences or whether it resembles value judgements. Also, whether epistemic values should be considered first or only in such judgements, or whether, on the contrary, non-epistemic values play a considerable role in scientific evaluation of theories.

Some philosophers, usually from a normative standpoint, claim that epistemic values – truth, coherence, simplicity, predictive fertility…– should be deemed more relevant than, or even displace, other values –moral, religious, cultural, aesthetic, not to mention personal (Laudan, 1984). From a different viewpoint, non-epistemic values may be seen as an inevitable and not necessarily 'illegitimate' component when producing and evaluating scientific theories (Echeverría, 1998; Longino, 1990).

On the other hand, non-epistemic questions should gain a place in science classrooms. The so called 'externalist' perspective emphasises the ethical, social and political responsibilities of science and scientists, and deals with issues that are sometimes raised by students (questions about genetics, greenhouse effect, nuclear energy, biological and nuclear warfare, and so on) and that are now being the object of epistemological and axiological reflection in philosophical debates.We have taken into consideration some of the abovementioned questions for designing classroom and distance-learning activities in science teaching. We will present and analyse some of such activities that are being implemented in science teaching and science teacher education in Buenos Aires, Argentina.

Adúriz-Bravo, A., Couló, A., Kriner, A., Meinardi, E., Revel Chion, A. and Valli, R. (2003). Three aspects when teaching the philosophy of science to science teachers. Programme & Abstracts of the 2003 ESERA Conference, 73. Noordwijkerhout: ESERA.
Echeverría, J. (1998). Filosofía de la ciencia. Madrid: Akal.
Laudan, L. (1984). Science and values. Berkeley: University of California Press.
Longino, H. (1990). Science as social knowledge. Values and objectivity in scientific inquiry. Princeton: Princeton University Press.

5. Ethics in Chemistry Education for Future Scientists

Veli-Matti Vesterinen
University of Helsinki

Maija Aksela
University of Helsinki

Over the past few years, there has been a growing interest in practical ethics, particularly in the societal and environmental risks of nano- and biotechnology. However, as the main focus of the public conversation has been on nano- and bioethics, there is a risk that the important ethical issues of other areas of scientific research haven't had enough attention. The ethical issues concerning the development of novel technologies (e.g. nanotechnology) are not necessary as novel as usually thought or limited to only certain recent areas of research (MacDonald 2004). Chemistry is a science that is produces applications and has potential to change the world around us. Therefore the societal and environmental issues related to creating novel technologies are of utmost ethical concern for research in chemistry.
Recent research in the ethics of chemistry has shown that there is a need to address the professional ethics of chemistry in university education (Coppola 2000, Kovac 2000). Professional ethics refers to the formal and informal codes of conduct adhered to by the members of the particular profession. As members of a profession, chemists have responsibilities to both their peers and to society as large. As chemistry is closely related to many societal and environmental issues, there is often a need for chemists to be able to discuss the ethical issues related to their work, not only with their peers, but also with the general public.
This study discusses the Finnish chemists' competency to address the ethical issues related to their work. Based on the results, suggestions are made on how to integrate ethics into Finnish university chemistry curricula. In the focus of this study are graduate students, who are just beginning their professional careers as chemists. The graduate students of chemistry and applied chemistry in five Finnish universities are asked to describe the societal impact and importance that their research might have, and the ethical concerns they have had to take into consideration in their research. The views presented in the answers are organized into categories and subcategories, and related to the ideas presented in the research of the ethics of chemistry. On the basis of comparison between the views expressed in answers and the views presented in the research, possible implications for Finnish university education are discussed.

The views are organized into internal and external responsibilities. Areas of concern on internal responsibilities include problems with authorship and problems connected with the design, execution and reporting of the experiment. Areas of concern on external responsibilities include divided loyalties resulting from non-public funding and anticipating the environmental and societal consequences of the research. As a conclusion of the study it is suggested, that there is a need for graduate courses where both the external and internal responsibilities are discussed explicitly.

MacDonald, C.: 2004, Nanotech is Novel; the Ethical Issues Are Not. The Scientist, 18(3), 8.
Coppola, B. P.:2000, Targeting Entry Points for Ethics in Chemistry Teaching and Learning. Journal of Chemical Education 77, 1506–1511.
Kovac, J.:2000, Professionalism and Ethics in Chemistry. Foundations of Chemistry, 2, 207–209.

Symposium 4: How Do Practices Move Knowledge Around? Insights from 'Facts' Travelling across Disciplinary Domains

Rachel Ankeny
University of Adelaide

Martina Merz
Universität Luzurn

Studies of the technical, material, tacit and collaborative aspects of doing science have enriched our understanding of scientific practice. Reconstructing knowledge production procedures and capturing them in vivid descriptions, however, leaves often aside the constraining or restricting role of disciplinarity that affects the ways in which knowledge can be translated, replicated or understood. In this session, our main focus is to explore the ways in which practices move, transmit and circulate knowledge and to elaborate an approach which acknowledges the epistemological challenge of integrating knowledge across domains of research. Instead of emphasising disciplinary integration and differentiation, boundary construction and transgression, we ask what kind of methods, tools and forms of interaction facilitate or prevent the movements of factual knowledge across domains of research.

More precisely, in this session, we focus on the ways in which scientific practices grounded in the material and instrumental settings help to assimilate new 'facts' that are produced within a different domain. Through the analysis of cases from psychology, model organism research in molecular biology and epidemiological modelling, we argue that in order to understand the transmission of factual knowledge across domains, we need to identify the ways in which experimental and technical standards control and limit this process. Furthermore we consider the implications of these limitations for the integration of knowledge.

1. Psychologies of Crowding: Experimental Practice and Environmental Design

Edmund Ramsden
London School of Economics

Building upon studies of density and behaviour in ecology, the problem of stress from crowding has proven a popular subject of discussion and analysis in the social and medical sciences and the design professions. Most notable were a series of experiments on rats and mice carried out by the comparative psychologist John B. Calhoun, employed at the National Institute of Mental Health (NIMH). In 1962, Calhoun identified a series of "social pathologies" that resulted from increased population density, such as violence, autism and sexual deviance. The aim of this paper is establish how, and how well, facts of crowding pathology, generated in the rodent laboratories of NIMH, travelled to an alternative experimental setting, the cities and institutions of the social and environmental psychologist.

In so doing, the paper will assess the role of experimental tools, standards and practices in determining the transfer of knowledge and degree of collaboration between disciplines.

Indeed, in seeking to test Calhoun's findings on human beings, social psychologists were faced with obvious ethical and practical restrictions. They therefore sought alternative approaches, either through short-term experiments with individuals in crowded situations, such elevators, waiting rooms, or shopping trips, or through analysing the effects of social density in restricted institutional settings, such as the prison. Just as in Calhoun's rodent studies, experimental practice and design determined results. For those concerned with the development of a new field of environmental psychology, focused upon the broader questions of social interaction in the urban environment, evidence of social pathology was carefully contextualised and restricted, and thus, comparisons between animals and man clearly delineated. For many concerned with institutional reform, however, evidence of the deleterious consequences of treating human beings as "caged animals", were analogous to Calhoun's pathological rodents.

2. Evidence, Facts and the Database Revolution in Biology

Sabina Leonelli
London School of Economics

Contemporary experimental biology is characterised by an overproduction of data about virtually every aspect of the most popular model organisms. The model plant Arabidopsis thaliana alone attracts the attention of over 16.000 researchers around the globe, working on fields as different as physiology, genetics, ecology and cell biology. The data thus accumulated are brought together, organised and circulated with the help of digital databases, which are becoming crucial tools towards integrating knowledge produced by different branches of biology. This paper explores the travels of data (1) from the laboratory in which it is acquired to the databases where it is stored and (2) from the databases to their users, i.e. biologists who need that information for their own research. My interest lies in how the treatment of data within these settings affects their epistemic value as evidence for specific factual claims about the biology of organisms.

I examine the processes through which data originally produced in one laboratory is selected, manipulated and visualised to fit standards and ways of understanding characterising other research contexts. The acceptance of factual claims across different branches of biology depends on the way in which evidence for those claims is transformed to fit the standards adopted in each field. Database curators need to make data accessible to biologists employing practices and goals that differ considerably from the practices and goals characterising the setting where the data was originally produced. Databases enable data generated as evidence for a specific claim to become available to biologists with varying research interests, hence allowing researchers to assess the relevance of those same data to supporting other facts. Indeed, the transformation of data to fit database standards often results in an increase of the facts about organisms for which data are taken to provide evidential support. This arguably points to the emerging role of databases not only as tools for dissemination of data, but also as means to increase the evidential import of given sets of data, thus potentially creating new biological knowledge.

3. The Brokers, the Conformists and the Stubborn: the "Travellers" Crossing Disciplinary Divides in Modelled Environments

Erika Mattila
London School of Economics

Production of new, interdisciplinary knowledge, especially in technically demanding modelling environment, requires adoption of knowledge from different, collaborating fields. To achieve this kind of cross-fertilisation is not, however, a simple or straight-forward process. This study explores the interdependence of technical artefacts, visual representations, computational algorithms and research questions embedded in modelling practice and related to the networks of expertise and collaboration. This interdependence allows us to examine in detail the ways in which research groups with different disciplinary backgrounds actually adopt, produce and apply factual knowledge. The perspective taken in this study is that of a 'fact'. More precisely, the focus is on the ways in which 'facts', which were produced in a long-term, interdisciplinary modelling practice and resulted in a set of infectious disease models for public health purposes, become accepted, acknowledged, or, perhaps, ignored by collaborative partners or in broader disciplinary contexts.

Once we observe the ways in which 'facts' travel across the different domains in the cross-fertilisation processes, we may sharpen our focus onto the processes of integration and disintegration of knowledge. Why some 'facts' become unquestionably part of the 'canon', the specific way in which questions concerning disease transmission or data augmentation are presented? Could we characterise the various roles 'facts' are given during building and application of models? To address these questions, this study traces the research problems, techniques, data, and computational algorithms that enhance or prevent the 'spread of facts' across heterogeneous modelling practices within research groups based in Finland and the UK. This study is based on three types of data: long-term ethnographic research on infectious disease modelling, analysis of published documents and articles and interviews with members of collaborative network engaged in epidemiological or statistical modelling.

The key findings suggest that some 'facts' reported, for example, in a statistical context may become accepted as epidemiological 'facts' that actually link the documented model with the broader disciplinary tradition. Hence, these 'facts' may be seen as 'conformists' that became acclimatised in the new domain or as 'brokers' that try to facilitate the cross-fertilisation process between the domains. Moreover, 'simulated facts' (i.e. those produced by a simulation model) may gain credibility when are used in other models by anchoring the stories told by these models into disciplinary contexts. Interestingly though some 'facts' seem to behave "stubbornly" and require auxiliary concepts in order to become domesticated in the models. A typical example of a 'stubborn fact' is actually 'disease transmission', which needs to be addressed through the simplified transmission mechanisms that allow it to be expressed in mathematical algorithms. These findings, hence, give us a new insight into the epistemologically challenging level of interdisciplinarity by tracing the possible patterns of integration or disintegration of knowledge and by showing how the relation of disciplines is shaped during this process.

SYMPOSIUM 5: HOOKING UP MODELS TO THE WORLD

Uskali Mäki
Academy of Finland

Aki Lehtinen
University of Helsinki

The session seeks to contribute to the growing body of literature on scientific models by examining and developing contemporary accounts of economic models. The four papers intended for the session all deal with the broad and complex issue of how theoretical models in economics are connected with real world economies by practicing economists. These links have various kinds of ontological, semantic, epistemological, methodological, and pragmatic aspects. Taken together, the four papers will cover all these aspects. What the papers share is that they are motivated and inspired by Robert Sugden's account of theoretical models as credible worlds as well as by the idea that models isolate important aspects of their targets, such as capacities and causal mechanisms. Among the key issues and concepts, the papers analyse and discuss the notions of representation, isolation, unrealistic assumptions, credibility, robustness, and learning.

1. Economic models as representations, isolations, and credible worlds

Uskali Mäki
Academy of Finland

In his celebrated "Credible worlds" Robert Sugden contrasts his account of theoretical models in economics with those of Dan Hausman and myself. Since the first appearance of Sugden's paper at a conference in 1997, I have failed to see the contrast between his and my account as stark, or even as existing at all. The paper will outline my own current understanding of models and show how it accommodates many of Sugden's most valuable insights.

I take models to be representations with two aspects: the representative and resemblance aspects. As representatives, models serve as substitute or surrogate systems the properties of which are directly examined (in order to indirectly acquire information about the target systems in the real world). I suggest credible worlds a la Sugden are representatives in this sense. Theoretical models (as representatives) are also isolations employing controls implemented by idealising assumptions. This does not imply any incompatibility between the notions of models as credible worlds and models as isolations, Sugden's apparent suspicions notwithstanding. Regarding the resemblance aspect of models as representations, the credibility of models as credible worlds will be analysed in terms of similarity along ontological and epistemological dimensions. Finally, the identity of models (regarding their abstractness and materiality) will be discussed. Throughout, the examples discussed by Sugden will be used to illustrate.

2. Learning from Economic Models

Till Grüne-Yanoff
Royal Institute of Technology

Theoretical economic models portray extremely simplified, counterfactual worlds. This paper discusses how we can learn from such models about the real world. As a starting point, I take Sugden's account of models as credible worlds. He argues that by judging a model to be credible, the causal factors described in the model are judged to be the same as the ones active in the real world. This, Sugden claims, allows an inductive inference from the model to the real world. Learning from models thus crucially depends on how credible one finds them. I argue that Sugden's notion of credibility prevents accounting for the most important kinds of learning from models. Judging models to be credible without reference to context or purpose only allows us to conclude from the credible model that a certain event is possible. We thus learn from the model only if we were confident in an impossibility hypothesis contradicting this conclusion. In contrast to this narrow account, I argue that we also learn from models by making credible inferences from them – inferences that are credible for a particular situation and a particular purpose. An inference from model to situation is credible if there are no reasons indicating that details neglected by the model matter for the situation. For this credibility judgment, evidence from the particular situation is required. However, we learn about the world from the model – the evidence is circumstantial in the sense that it only supports the inference. Thus, we can learn from a model about a broad spectrum of claims about a particular situation, if we consider the models in the light of this specific situation. This implies that model builders interested in what their models can teach us should focus more on the application of their constructions to at least some particular situation – a practice unfortunately rare in current mainstream economics.

3. Unrealistic models -- credible inferences? Economic modelling in theory and practice

Tarja Knuuttila
University of Helsinki

The most persistent philosophical problem of economics has concerned the realisticness of economic theories and their basic assumptions such as utility maximization, perfect information and the givenness of tastes. The issue has been whether such assumptions are too unrealistic or whether that should matter at all. Various standpoints toward the issue have been taken throughout the history of economics ranging from essentialist realism to fictionalism and instrumentalism. Recently, this issue has been addressed through studying how economic models give us knowledge about the economic phenomena. It has been suggested that economic models should be best conceived as surrogate systems,

through which we can get knowledge if they nevertheless succeed to isolate some causal mechanisms correctly (Mäki 2005) or license inductive inferences concerning their target phenomena (Sudgen 2002).

While treating economic models as surrogate systems describing tentative causal mechanisms goes some way in solving the problem of their unrealisticness, it still seems to me that many theoretical models in economics are far too removed from the economic reality to be taken as realistic representations of their target systems in any relevant sense. What is more, there are several examples of developments in which more realistic models have been cast aside in favour of more unrealistic ones. Thus the question is how to account for this phenomenon. I will suggest that theoretical modelling in economics should be conceived as a specific practice guided by a certain metaphysical understanding of its objects (cf. Mäki 2005) and making use of available computational templates, modelling methods and representational means in a rather opportunistic fashion. From this point of view what counts in modelling are the results produced—which results can explain some observed data but models might also be built to justify the modellers' theoretical preferences. I will also present some examples from economic modelling to illustrate the productive approach to models put forth.

4. Incredible worlds and credible results

Jaakko Kuorikoski
University of Helsinki

Aki Lehtinen
University of Helsinki

Robert Sugden offers his account of theoretical economic models as credible worlds as an answer to the question of how to justify the inductive leap from the world of models to economic reality. He also considers robustness considerations as the key justification for this inductive leap, but rejects this possibility since, according to Sugden, robustness considerations can only be about comparisons between models and thus cannot facilitate the inductive leap from models to reality. We argue that robustness considerations are inductively relevant in the sense that they are about model-world relationships; robustness guards against inevitable errors in model-based reasoning concerning systems with underlying causal heterogeneity.

All economic models involve abstractions and idealisations and there is no way to eradicate all false assumptions from a model. Moreover, in contrast to physics for example, economic theory itself does not tell which idealizations are truly fatal or harmful for the result and which are not. This is why much of what is seen as theoretical contribution in economics is constituted by deriving familiar results from different modelling assumptions. If a modelling result is robust with respect to particular modelling assumptions, the empirical falsity of these particular assumptions does not provide grounds for criticizing the result. Thus the credibility of a result, whether it can be expected to correspond to a genuine economic phenomenon, can be assessed by comparing even incredible worlds. We demonstrate how analytic or derivational robustness analysis does carry epistemic weight, and answer criticism concerning its allegedly non-empirical nature and the problematic form of the required independence of the ways of derivation.

SYMPOSIUM 6: STABILIZING EVIDENCE

Marcel Boumans
University of Amsterdam

The proposed symposium discusses how scientists build evidence. It will be shown that observations are not enough. To distill evidence from observations, these have first to be ‘stabilized’. Stabilization strategies take different forms: it can mean stabilizing an environment (e.g. laboratory), stabilizing the ‘observer’ (e.g. instrument, observatory), stabilizing the observations (e.g. standardization, protocol), stabilizing an audience (e.g. peer review), or stabilizing interpretation (e.g. pattern, theory). The three cases studies presented at this symposium take a specific kind of observation (microscope inspection, armchair observation, and statistics subsequently) as staring point and lay bare its road to evidence.

1. Protocol, pattern and paper. Interactive stabilization of immunohistochemical knowledge.

Hubertus Nederbragt
Utrecht University

This paper analyzes a small research project, performed to investigate the distribution of the extracellular matrix-protein Tenascin-C in mammary tissues of dogs. The method used for this investigation was immunohistochemistry of tissue slides which constitutes the application of an antibody to tenascin-C that specifically binds to the protein and can then be labelled and made visible for microscopic inspection. The first phase of the project is the making of the immunohistochemical protocol, the second phase is the deduction of a pattern of tenascin-C distribution in the tumours from the microscopical observations and the third phase is the writing of a manuscript for publication in a journal. Each of the three phases is analyzed separately, using the concept of resistance and accommodation, described by Pickering in his The Mangle of Practice (Princeton University Press, 1995). My purpose is to show that in each phase of the process of producing knowledge the scientist meets several resistances which force her to accommodate by changing her conceptual, technical and methodological approaches and that in the end of each phase a situation of stability of knowledge is reached.
In the protocol phase the main interaction takes place between the scientist and the antibody in combination with the tissue slides. In the pattern deduction phase the scientist meets resistance in the pathological diagnosis of the tumours and the expectations and hypothesis with which she had entered the project, in the criteria to be used for assigning each of the slides to one of a limited number of tenascin-C patterns, and in the response of colleagues and supervisor who have to be convinced of the proposed pattern(s) as stable knowledge. In the paper writing phase the interaction is between the scientist and the scientific community who should accept the knowledge of the research project as having implications for the knowledge of the community.

The three phases are connected to each other in two different ways. First, when stabilization of knowledge is obtained in a certain phase the agents of resistance turn into accomplices, giving support to accommodating to the resistances in the later phase. Second, the stabilization of knowledge of the protocol is further enhanced when stabilization of the pattern is obtained, whereas the latter is more definite when it has become stabilized knowledge in the scientific community.

2. The (shifting) nature of evidence in political economy

Harro Maas
University of Amsterdam

This paper is concerned with the shifting nature of evidence in political economy before econometrics took hold. As a case study I will look at John Elliot Cairnes' The Slave Power of 1862 and its antipode, Ulrich Bonnell Phillips American Negro Slavery of 1918.
Following John Stuart Mill's seminal essay on the definition and method of political economy Victorian political economists claimed a separate mode of observation in economics that was nevertheless as certain as what was generically referred to as "experiment and observation" in the natural sciences. This particular mode of observation distinguished itself from the controlled observation in the laboratory and from the field observations of natural philosophers. By mid nineteenth century, the Irish political economist John Elliot Cairnes was the most explicit in defending the economists' privileged route to truth to both sides, experiments and natural philosophy. In his methodological writings Cairnes claimed that the "business of the political economist" was done once he had traced a phenomenon back to a mental principle. Elsewhere, he compared this method with that of a "comparative anatomist" who (like Cuvier) deduced the shape of an extinct animal from "a fragment of a tooth or bone". For Cairnes observation consisted in the mediation between a phenomenon and a principle of mind. But if this mediation was not established by means of experiments, nor by means of field observations, the question is 'How was it?'. That is: what "bones" (or evidence) led to the motives? Put this way, the Millian method of observation found its nineteenth century literary equivalent in the works of Edgar Allan Poe and Arthur Conan Doyle. The first purpose of this paper is to investigate the fruitfulness of this literary connection in relation to Cairnes' Slave Power of 1862.
In his days, Cairnes' book was generally heralded as a fine piece of inductive reasoning (even though he considered it a piece of deductive reasoning himself), and became the standard account of the American slave economy until 1918, that is until the appearance of Ulrich Bonnell Phillips American Negro Slavery. Phillips forcefully argued that Cairnes might have got to a very different image even on the basis of the evidence then available. The blistering attack of the historian Phillips on Cairnes' (mis-)use of statistical evidence definitively damaged Cairnes' reputation of having provided an accurate account of the American Slave South, but it raises the question what counts as credible

evidence for a theory, and what makes criteria for evidence change over time. The illumination of these questions is the second purpose of my paper.

3. The problem of finding evidence outside the laboratory

Marcel Boumans
University of Amsterdam

Trygve Haavelmo's methodological manifesto The Probability Approach in Econometrics not only laid down the paradigm for modern econometrics, but also sets out the strategy for measurement in the 'wild'. His conceptualization of 'passive observations' and the framing of the problems that go with them, are still very useful for understanding current 'experiments outside the laboratory'.
Haavelmo's classic is very rich: it provided the framework for introducing probabilistic methods in econometrics and a profound discussion on invariance ('autonomy'). This latter subject is well treated by various philosophers, like N. Cartwright, K.D. Hoover, and J. Woodward. This does not, however, apply to the 'problem of passive observation'. It is only mentioned, if it is mentioned at all, in relation to the discussion of autonomy, but that is it.
The problem of passive observation is, however, a separate problem, namely the problem of finding a (complete) list of all relevant causal factors outside the laboratory; that is, trying to pick them out when they are all working simultaneously and we cannot isolate them. All kinds of alternative empirical methods were suggested to find evidence outside the laboratory. Ragnar Frisch, one of the leading econometricians of those days, suggested a method to measure the strength of the causal influences and Jan Tinbergen, the other leading econometrician, applied this method in his paradigm-setting methodology of macro-econometric modeling. Haavelmo, however, showed that this empirical approach could lead to spurious explanations. Therefore he distinguishes between factual and potential influences. Factual influences are influences we observe. These are the causal factors that have potential influence and which vary enough to reveal their influence. But this does not mean that the influences we do not observe have no potential influence. To know which factors are causal we, as passive observers, are dependent on when Nature's willingness to show them, and this might take a long time.
Already in Haalvelmo's manifesto, but more explicit Koopmans program for modern econometrics, it gradually became accepted that this problem could not be solved by empirical research alone, and that theory had a decisive role in selecting the relevant causal factors.
This paper will give a reconstruction of his discussion of 'factual' and 'potential' influences, which provided Haavelmo the framework to discuss 'Nature's experiments' and will allow us to discuss more generally measurement outside the laboratory. Moreover, it shows how Herbert Simon could develop this framework into his own account of causal ordering.

Symposium 7: Social and Political Aspects of the Study of Disease and Cure

Julian Reiss
Complutense University

David Teira
UNED

Which biomedical problems ought to be studied? Once we know which questions to ask, how do we go about addressing them? The aim of this session is to shed light on these normative and methodological issues. The core idea underlying two of our papers (by Ankeny and Kerridge and by Reiss) is that biomedical research should serve the common good. That is, it should be practised such that the values and goals of the global population are addressed, including the poor, minorities and other disadvantaged groups. These two papers investigate specific cases demonstrating that biomedical research, in its currently form, is far from this ideal. One paper addresses the WHO system of classifying diseases, the other, the so-called neglected-disease problem.

The remaining two papers (by Dehue and Teira) look at the social epistemology of randomised clinical trials (RCTs). RCTs are now the "gold standard" for proving clinical efficacy in evidence-based medicine and, in fact, it is today virtually impossible to introduce a new drug into the market without having it tested in a large RCT. But this has not always been the case. Dehue's paper examines the background of social values that had to prevail in order to make RCTs socially acceptable. Teira's paper investigates an episode in the UK medical history in which methodological, social and political arguments competed about whether or not to introduce RCTs and shows that all three types of concern can be unified in a single economic model.

1. Classifying Malignancies: The Effects of Practice on What Counts as Disease

Rachel Ankeny
University of Adelaide

Ian Kerridge
University of Sydney

Over the past four decades there have been enormous advances in the understanding of the haematological and lymphoid malignancies. Identification of various types of differences between particular disease conditions has enabled vastly greater specificity in diagnosis and more accurate prognostication. In recent years, discovery of the molecular basis of these conditions has led to significant changes in the way they are diagnosed, classified, and most importantly, treated. This paper focuses on the most recent and widely adopted classification system for the lymphoid malignancies as proposed by the World Health Organization (WHO), against the backdrop of a brief history of previous classification systems. The development of the WHO system has been stated to be in accordance with the "longstanding WHO principle that international agreement on criteria for the definition and classification of cancer types and a standardised nomenclature are prerequisites for progress in clinical oncology, multicentre trials and comparative studies in different countries" (Jaffe et al., 2001). However while the WHO system appears to establish more clearly delineated disease categories in terms of characteristic epidemiology, aetiology, clinical features, and oftentimes distinctive therapeutic responses, it requires diagnostic information from techniques that are not widely available within the developed world and are rarely available in developing countries. Hence we argue that although the concepts of disease that have been created through this system are in some sense more fundamental or real than previous ones, they may fail to allow the development of diagnostic principles that are globally relevant and also impede comparative research and progress in the field.

2. The Emergence of the 'Control Group'

Trudy Dehue
University of Groningen

Handbook histories present scattered examples of treatment comparison through the ages. One classic example is that of the eighteenth-century doctor James Lind who embarked on a ship to compare six scurvy treatments by giving each of them to two diseased sailors. Another one is that of doctor Ignaz Semmelweis who fought child-bed fever by comparing two maternity clinics in mid-nineteenth century Vienna. This paper, however, argues that only in Whig histories can such examples can be described as immature precursors to our present-day experiment comparing 'true' experimental and control groups.

Moreover, experimental comparison of groups was not an option in pre-twentiethcentury scholarly debates on research with human beings. John Stuart Mill's System of Logic (1843) extensively discusses the "method of difference", that is comparing cases in which an effect does and does not occur. Yet, Mill considered this method inappropriate in research with human beings. And 1865, the illustrious French physiologist Claude Bernard published a book with the deliberately provocative title of Introduction à L'étude de la Médecine Expérimentale. For the sake of valid knowledge, Bernard maintained, "comparative experiments have to be made at the same time and on as comparable patients as possible". Nevertheless, one searches Bernard's Introduction in vain for comparison of experimental to control groups. As ardently as he defended experimentation, he rejected group comparison.

This paper presents an explanation of why eminent nineteenth-century scholars did not adopt the use of control groups as a methodological condition. It argues that the methodological significance of such groups was inconceivable before considerable changes occurred in society at large. Analysing the nineteenth-century values that excluded comparison of artificially composed groups, helps to recognise the twentiethcentury values endorsing it.

3. Neglected Diseases and Well-Ordered Science

Julian Reiss
Complutense University

The practice of the sciences is well-ordered (in Philip Kitcher's sense) only if inquiries are directed in ways that promote the common good, conceived as aiming at the goals that would be endorsed in a democratic deliberation among well-informed participants committed to engagement with the needs and aspirations of others. Whether or not this particular elaboration of the idea of the common good is adopted, a necessary condition for well-ordered science in biomedical research is that research addressed to alleviating the burden of suffering due to disease should accord with the "fair-share" principle: at least insofar as disease problems are seen as comparably tractable, the proportions of resources assigned to different diseases should agree with the ratios of human suffering associated with those diseases. Thus if the disease burden associated with a form of respiratory infection is twice that of a specific type of cancer, and if there are approaches to both diseases that are roughly equally promising, then the funds assigned to the respiratory infection should be approximately twice those given to the cancer.

It would be difficult to maintain that contemporary biomedical research is well-ordered in this sense. The global disease burden is distributed very unequally across the globe (for instance, between first- and third-world countries, between men and women, between richer and poorer strata within the same societies), and these inequalities are reflected in medical research and development investment: for instance, cardiovascular diseases receive ten times the funding of malaria per DALY lost, diabetes ten times that of tuberculosis.

This paper addresses research in the context of a range of tropical diseases, remedies for which are extremely scarce because no markets for alleviating drugs exist: the so-called "neglected diseases". It discusses a number of solutions to the neglected-disease problem that have been proposed, including an income-transfer solution, a public-goods solution and the Biomedical Research and Development Treaty and argues that each proposal has a number of important shortcomings. The proposal made here focuses on the patenting system that prevails throughout the Western world and suggests that this system needs to be redrawn if biomedical research is to serve the common good.

4. What Caused the Adoption of Randomised Clinical Trials in Britain?

David Teira
UNED

My paper analyses the adoption of randomisation in the design of clinical trials in the United Kingdom at the time when the first test of the efficacy of streptomycin was conducted. I try to reassess this episode from the standpoint of an economic model of scientific behaviour. Historical accounts of the process adopt three different approaches.

In accordance with the testimony of the medical statisticians who promoted randomisation, some argue that it was adopted just for the sake of its methodological strength. There are those who argue that the adoption of RCTs was driven by broader social concerns, independently of their statistical cogency, mostly to justify the allocation of streptomycin to patients at a time of scarcity. It has been also argued that the introduction of RCTs was just a by-product of the British pharmaceutical policy in the inter-war period, aiming at an impartial regulation of the pharmaceutical market. Historians usually grant that there is a bit of truth in each of these alternatives, but their accounts emphasise one or another depending on their personal intuitions as to the motivations of the agents involved in the adoption of RCTs. Those who portray them as mostly epistemic agents prefer the first of the three accounts; those who view them as self-interested policy-makers opt for the second one and, finally, those who focus on the normative concerns they expressed prefer the third explanation. The underlying philosophical dilemma is whether we can somehow unify all these threads into a single model of scientific behaviour.

My claim is that this is feasible if we construct a social epistemology of RCTs that articulates all these concerns. The relevant political features of randomisation, namely impartiality, can be captured by decision theory, if we assume that the clinicians act in their own interest. This is the very plausible assumption that the supporters of the second account favour, but it is not incompatible in principle with the motivations preferred in the first and the third accounts. I argue that if we adopt an economic model of scientific decision-making we can render these three accounts (to a certain extent) compatible.

SYMPOSIUM 8: SCIENCE, SOCIAL CONSTRUCTIVISM AND PSYCHOANALYSIS: EXPLORING THE OPTIONS

Filip Buekens
Tilburg University

Recent debates in France and elsewhere have (once again) raised questions concerning (i) the status of psychoanalysis as a science and (ii) the epistemic status of psychoanalytic interpretations. (Meyer et.al., 2005, Buekens 2006). A careful examination of arguments of both Freud critics and defenders of psychoanalysis (both as science and as therapy) shows that both parties appeal to versions of social constructivism as a theory of science science and an account of scientific and therapeutic practices to make their case. The following options can be discerned:

An account of the way genuine social facts are construed can explain how psychoanalytic theory has evolved and how psychoanalytic interpretations of patients and cultural artefacts are generated. A defence of this position is compatible with a rejection of global social constructivism as an account of science in general and an account of psychoanalysis as a circular hermeneutics.
A social constructivist account of mental disorders explains the way psychoanalysis functions, thus making its potentially positive effects in therapy consistent with its rejection as a genuine scientific theory.
Social constructivism qua theory of science is an indefensible account of science, but often appealed to by psychoanalysts as an immunisation strategy against their critics.

The aim of the proposed symposium is to clarify the fascinating and often confusing dialectical role of issues concerning science and social constructivism in a highly debated area.

1. Searle, Institutional Facts and Psychoanalysis

Filip Buekens
Tilburg University

Among the many variants of social constructivist claims about science, three claims stand out as central. First, there is the idea that knowledge is ‘created’, rather than found. Theories create their own facts – facts are social constructions (pace Boghossian 2006). Secondly, there is no sharp distinction between justification and acceptance. Justifications are social constructions and truth is what is ‘accepted by our peers’ (Rorty). Thirdly, when we say that a certain description is ‘accurate’ or ‘true’, we are not judging it according to how well it depicts the world. Rather, we are saying that the words have come to function as ‘truth telling’ within the rules of a particular language game – or more generally, according to certain conventions of certain groups (Gergen 1999).

I will present an account of the construction of social facts that does not support global social constructivism in science. Genuine social facts come into existence, are created by rules of the form 'X counts as Y', they have a context C in which find a justification for their existence and they must be accepted by a community of agents to continue their existence (Searle 1995, Hindriks 2004). Searle does not intend his theory of social facts as a theory about truth, scientific theories or how scientific facts come into existence (on the contrary!). However, the features discerned by Searle explain remarkably well the way a psychoanalytic claim functions. I show how constitutive rules govern the introduction, invention or construction of 'psychoanalytic facts' (the Oedipus complex, death drive, the unconscious, …). These rules have two functions: (i) They introduce an implicitly define psychoanalytic concepts (in the Y-position), and since they are declaratives that have both mind/world and world/mind direction of fit, they create the facts with which they correspond. (ii) The constitutive rules must be accepted by a community (there are no psychoanalytic facts independently from the acceptance of its constitutive rules by the psychoanalytic community) and (iii) the created institutional facts exist relative to a context in which its appliers find ample evidence for their 'truth'. This constructivist account of psychoanalysis sheds less light on why it is a pseudoscience, but illuminates how it functions in practice,under a description many psychoanalysts tend to reject.

2. Placebo and psychoanalytic practice: a moderate social constructivist defence of psychoanalysis

Andreas De Block
Radboud University Nijmegen & Catholic University Leuven

According to social constructivists, social constructions arise when the continuous flow of contingencies is stabilized by the use of generalizations and concepts. Such concepts are often reified: people start to think that these abstract concepts have real and tangible existence. Social constructivism further analyses how and why the concepts and their transformations influence the thinking and behaviour of individuals. Hacking's analyses of fugue (Hacking 1999) and multiple personality disorder (Hacking 1995) are based on social constructivist theory. He contends that mental disorders are not indifferent to psychiatric theories and diagnostic tools. According to Hacking, mental disorders are not natural (or indifferent) kinds, but what he calls 'interactive kinds'. Hence, what was known about people suffering from a mental disorder may become false because people of that kind have changed in virtue of how they have been classified, or because of how they have been treated as so classified. I will argue that the socially constructed character of many - if not most - mental disorders has interesting consequences for psychoanalytic theory:
Because mental disorders are to a large extent 'culture bound syndromes' or social constructions, psychoanalysis could only flourish as long as it was widely accepted (Borch-Jacobsen 1989).

Psychoanalysis does not differ substantially from other psychiatric theories. All of the (human) behavioural and social sciences create – at least partially – what they study. Obviously, in science not all is invention and nothing discovery (Bunge 1996), but all sciences studying interactive kinds both invent and discover.
The effects of psychoanalytic therapy are primarily placebo (Shapiro & Morris 1978). But this is equally true for other psychotherapies (Frank 1961, Dongier 2001). A 'talking cure' can only be effective if the therapist and the patient have more or less the same beliefs about (a) the basic psychiatric taxa, (b) the aetiology of mental disorders, and (c) the aim of the therapy (Torrey 1972). Hence, part of the today's failure of psychoanalytic therapy can be brought back to the public criticism of psychoanalysis, rather than to the justified content of that criticism.
If psychoanalysis is nowadays considered to be a bad theory, it really is a bad theory. The social construction of a failure (or a success) does not make the failure (or success) any less real (Hacking 1999).

3. Psychoanalysis and social constructivism: the epistemic status of a pseudoscience

Maarten Boudry
Ghent University

Social constructivists assert, among other things, that there are no objective facts which by themselves are able to change a scientific paradigm. Golinski (2005) writes that "given sufficient creativity and resourcefulness on behalf of its defenders, the existing paradigm could be maintained indefinitely." Many philosophers and scientists regard this as a dubious claim if applied to science, because it takes the occasional narrowness or irrationality of scientists as intrinsic to the whole discipline (Holt, 2002). I will argue that social constructivism unintentionally offers a useful description of psychoanalysis, but not necessarily of every pseudoscience. The characterisation of a theory as a pseudoscience does not require the properties that constructivism describes. A theory is suitable for a constructivist description if its development is not determined by some objective physical reality, but is contingent upon the social and cultural predispositions of the theorist or the research community.
The epistemic structure of psychoanalytic theory allows for a constructivist description of its functioning. Several epistemic conditions jointly make psychoanalytic interpretations ultimately arbitrary, and therefore susceptible to cultural and social factors for its continuing appeal:
the postulate of an imperceptible realm of the mind which is called the psychodynamic "Unconscious"
the attribution of certain properties to this Unconscious which reduce the chances of encountering falsifying material, and which concomitantly extend the means of drawing inferences between the source material to reach a certain conclusion or interpretation.
the assertion that the Unconscious is illogical, irrational, and it tries to deceive us in the most far-fetched ways: this makes any apparent implausibility in psychoanalytic interpretations again consistent within the theory.

The deeper meaning assigned to empirical material (dreams, slips of the tongue, works of art, human behaviour in general) is in practice unfalsifiable, Because of these epistemic properties, the deeper meaning assigned to the empirical material (…) can in practice always be 'confirmed', and therefore psychoanalytic interpretations function as arbitrary decrees, which can be described accordingly (Buekens, 2005). The epistemic core of psychoanalysis consists in a purely formal recipe for generating confirmations in theorising about the human psyche. Psychoanalysis is in that sense a hollow or "empty" theory (Borch-Jacobsen 2005). As the constructivist would say, disputes between rivalising psychoanalytic schools or "paradigms" can never be decided on an objective basis, because the theory is "flexible" (Golinski, 2005) enough to cope with any anomalies (Crews, 1998). In my contribution I will defend the following claims:
The proliferation of social constructivist ideas in psychoanalysis seems to be a consequence of the constructivist internal dynamics of psychoanalysis itself, and the naïve extrapolation of these properties to science in general. The fact that quite a lot of 'post-modern' psychoanalysts tend to think that the scientific theories are 'socially constructed', maybe witnesses to the fact that this is just the kind of theory dynamics with which psychoanalysts are most closely acquainted.
Although social constructivism is a suitable theory to describe the internal dynamics of psychoanalysis, this does not mean that it can be used legitimately as an argument to defend psychoanalysis against scientific and philosophical critiques. In that case the constructivist argument is an immunization strategy which is implicitly relativistic and epistemically too strong.

Buekens, F. (2005), Fear and Loathing in Lacania, in http://www.butterfliesandwheels.com/articleprint.php?num=159
Buekens, F. (2005), Pourquoi Lacan est-il si obscure? In C. Meyer e.a. (eds), Le livre noir de la psychanalyse, Paris: les arènes.
Buekens, F. (2006) Freuds Vergissing Leuven: Van Haelewyck
De Block, A. & S. Dewitte (2007) Mating games. Cultural evolution and sexual selection. Biology and Philosophy 22.
De Block, A., (2006) Applied Darwinism: Lessons from the history of applied psychoanalysis. Culture and Organization 12, 293-305.
De Block, A en P. Adriaens (2006), The Evolution of a Social Construction. The case of Male Homosexuality, Perspectives in Biology and Medicine 49, 570-585.
De Block, A. (2006) Freud as an evolutionary psychiatrist. The foundations of a Freudian philosophy, Philosophy, Psychiatry and Psychology 12, 315-324.
De Block, (2005) A. Doomed by Nature, Philosophy, Psychiatry & Psychology 12, 343-348.

PAPER ABSTRACTS

DEMARCATING MISCONDUCT FROM MISINTERPRETATIONS AND MISTAKES

Hanne Andersen
University of Aarhus

Within recent years, scientific misconduct has become an increasingly important topic, not only in the scientific community, but in the general public as well. Spectacular cases have been extensively covered in the news media, such as the cases of the Korean stem cell researcher Hwang, the German nanoscientist Schön, or the Norwegian cancer researcher Sudbø. In Science's latest annual "breakthrough of the year" report from December 2006, the descriptions of the year's hottest breakthroughs were accompanied by a similar description of "the breakdown of the year: scientific fraud".

Official guidelines for dealing with scientific misconduct were introduced in the 1990s. At this time, research agencies, universities and other research institutions around the world developed guidelines for good scientific practice and formed committees to handle cases of scientific misconduct. In this process it was widely debated how to define scientific misconduct. Most definitions centered on falsification, fabrication, and plagiarism (the so-called FFP definition), but suggestions were also made for definitions that were broader and more open-ended, such as the 1995 suggestion from the US Commission of Research Integrity to replace FFP with misappropriation, interference and misrepresentation (the so-called MIM definition). The MIM definition was not adopted in the US, but MIM-like definitions have been adopted in several other countries.

In this paper, I shall describe these MIM-related definitions of scientific misconduct and analyze the arguments that have been advanced in their favor. I shall discuss some of the difficulties inherent in the MIM-related definitions, such as the distinction between misrepresentation and mistake, and the demarcation of misrepresentation in areas characterized by uncertainty or by diverging research paradigms.

I shall illustrate the problems inherent in the MIM-definition through a particular case: the ruling of the Danish Committee on Scientific Dishonesty (DCSD) about Bjørn Lomborg's best-selling book The Skeptical Environmentalist in which he argued that contrary to what was claimed in the “litany” of the environmentalists, the state of the environment is getting better rather than worse. Lomborg was reported to the DCSD by several environmental scientists, and this controversial case from 2003 ended with a verdict that characterized Lomborg’s conclusions as misrepresentations, but acquitted Lomborg of misconduct due to his ignorance. I shall analyze this verdict and the problems it reveals with respect to the MIM-related definitions of misconduct, and I shall briefly describe the aftermath of case, including the way in which misconduct allegations have since become popular in the Danish public debate on politically controversial research, such as intelligence research, climate research, or the history of the Cold War. Finally, I shall conclude the paper by returning to some of the considerations that led to the MIM-related definitions and discuss how to achieve their aims.

DO TECHNOSCIENTIFIC EXPERIMENTS HAVE THEIR OWN LIFE? FOUR SCHEMES FOR THE EXPERIMENT-THEORY RELATIONSHIP

Juan Bautista Bengoetxea
Spain

In order to talk about naturalism in the realm of philosophy of science, philosophers sometimes allude to different discourses about scientific practices. It is true that this kind of practices may be considered an appropriate way to detect modal expressions, especially that of necessity in its natural form. That is to say, these practices may show that the concept of natural necessity has a normative authority on what we say or make. What is the reference we talk about when we develop this kind of practices is not that clear, however.
Scientists use to emphasize the role that experimental intervention plays in the realms of scientific and technological knowledge. They try to know about nature and that is why construct conceptualizations that help us do that work. But it is not less true that scientists and engineers must intervene in nature manipulating, changing, and forcing its limits in order to get not just economic benefits, but also gains in knowledge. And the most typical interventionist form is no doubt experimentation.
Since the 1960s, philosophers of science have almost unanimously accepted that theory and experiment cannot live separately. Only a few exceptions, sometimes close to the new experimentalism, have tried to underline the virtues of experimentation, even by claiming that this has a life of its own –that is, a life independent from theory. However, those who supply the thesis of the theory-laden of observation –and experimentation— have criticized the 'experimentalist' by asserting that it is not just inopportune, but also retrograde. This claim, of course, is not necessarily so.
The philosophers who sustain the view of the independent nature of experiments do not try to completely unlink experiments from theory, but rather to place the former under a wider view in which experimentation is not absolutely dominated by a theoretical view. This point, hence, underlines that the way how the philosophy of science has focused the issue is a biased way that did not give enough importance to experiments and material practices.

From my point of view, to say that experimentation has its own life means the following four things:

(1) First, that experiments are not mere means for observing and getting data, but also sophisticate complexes that incorporate designed actions, skills, abilities, and conceptual understanding, all of which have a scientific sense by itself.
(2) Furthermore, it means that experiments do not work only in order to assess, evaluate, or interpret theories, as the logical empiricists and some historicist philosophers used to claim. In fact, the aim of experimentation is also both to account for intended goals inner to wider frames of experimental practices and to adequate to open options and possible constraints.
(3) In addition, it is to be emphasized the fact that experiments and their outcomes are not determined by the theoretical interpretation we make about them.

(4) Finally, I consider that experimentation generates and works with new and artificial phenomena whose implementation is not only due to the instantiation of laws.

This proposal tries to develop four points, even though its main aim is to answer to the question about the own life of experimentation. Neither the theory-laden thesis nor the strict experimentalism is absolutely correct. A better answer can be shaped by what I think it is a four-side scheme of possible relationships between experiments and theory. In order to develop this scheme, previously I present two steps. In the first (Section 1), I show three characteristic cases of the thesis of the own life of experiments (the 'science as technology' account, the instrumental experimentalism, and the exploratory experimentalism). In the second step (Section 2) I exhibit some replies to the previous cases. The proposal of the four-side scheme (independence, mere relation, dependence, and determination) of the 'experiment-theory' relationship is the topic of the Section 3, which I complete with a plea for a non-polarization of the items 'theory' and 'experiment' as if they were understandable in a clear and distinct way.

PRACTICES OF STABILIZING THE UNDERSTANDING OF SOFTWARE CODE

Viktor Binzberger
Technical University of Budapest

I'd like to extend Joseph Rouse's approach of analyzing experimental situations and scientific knowledge in terms of the dynamics of power relations, epistemic alignments, and situated discursive practices into a domain different from that of constructivist philosophy of science: the production of software code. Following Rouse's perspective, the discursive practices of software code production share many features of the production of scientific knowledge. While scientists aim at stabilizing and normalizing experimental practices in order to be able to extend their controlled laboratory microworlds and their practices into society, software developers try to stabilize and normalize their code-producing practices for similar reasons. Doing so, they get entangled in dynamical power relations mediated through technological artifacts, and they struggle to put up resistance against the established power patterns of these socio-technical networks, just as scientists do. Analogously to Rouse's arguments against the reification of „knowledge", based on its inherent situatedness within experimental practices and cultural context, a similar argument can be construed against the reification of „information", based on the open-endedness of the possible future interpretations of software code within the lifeworlds of its users and producers.
I'm illustrating the contemporary relevance of this philosophical thesis with two case studies. One is focused on shared practices of debugging and source code interpretation within a case of closed-source software development, and the other contrasts it with practices prevalent in one of the most successful projects in the Free/Libre Open Source world, the Mozilla Project. I'm going to look at the various strategies practitioners are engaged in to establish normalized code-producing practices, and I'm going to assess the breakdowns and resistances that are working antagonistically, diverting these strategies into locally situated adaptations, stemming from the partially diverging interpretations of the situation. I will touch on the role played by the architecture – or in Lawrence Lessig's terms, the "code" - of certain technical artifacts in these processes (programming language compilers, bug tracking systems).
By drawing this analogy, I don't want to downplay the differences between doing natural science and software development. Most significantly, software developers move in a world that is obviously socially constructed, whereas scientists strive to orient themselves toward a world that transcends our situatedness. Nevertheless, the relative stability of the social world in which programmers are engaged in permits us to draw the analogy, and the fact that the concepts of constructivist philosophy of science can be adapted to be used outside their originally intended domain underlines their relevance in understanding our contemporary technological lifeworld.

"ACCEPTANCE" AS A NOTION FOR UNDERSTANDING THE PRACTICE OF SCIENCE

Frans A.J. Birrer
Leiden University

There is a growing attention for the notion of "acceptance" in connection to issues of knowledge, belief, truth, etc. (starting with Stalnaker, followed up by Cohen, Engel, Tuomela, and many others, and also more specifically in the philosophy of science, as prominently represented by Van Fraassen). The conceptions of 'acceptance' proposed vary considerably.

I want to suggest a specific conception of the notion of "acceptance", that can be used for the understanding of scientific practice. Formulated very briefly, the basic idea is that an individual accepts or does not accept an assumption by identifying and evaluating potential consequences of the assumption later turning out to be valid or not.

In order to get a realistic picture of scientific practice (and in contradistinction to Van Fraassen), 'consequences' are to be taken as considerably broader than merely politically correct criteria in science like 'arriving at the truth' or 'empirical success'. They also have to include more mundane incentives like endowed honors and admiration, and (in as far as science is applied to the real world) implications in society. That is, acceptance takes into account what is directly observed in the laboratory, but may also take into account the rewards and costs administered in the social community of scientists, and potential consequences of application.

The notion of acceptance proposed here includes the possibility of deception and self-deception. It can be extended to unconscious processes (i.e., when the acceptance is not a conscious decision) or even unconscious or implicit assumptions, in which case the understanding may take the form of a (formal) reconstruction. And of course, what an individual does or does not accept is highly influenced by social interactions.

An advantage of such a broad (in terms of criteria) and yet specific (tied to consequences and their evaluation) conception of 'acceptance' is that it creates room for sociological understanding without falling into an abyss of unbridled relativism. Differences of opinion can occur, but may be understood as differences in consequences taken into account, and as differences in the evaluation of consequences.

One possibility for such a consequential framework would be some kind of Bayesian decision model. Often, however, more deductive models of reasoning are observed. This need not surprise us, given the weaknesses of such models in practice (steeply raising complexity of calculations; arbitrariness of priori's when prior information is lacking). In the consequential framework proposed, deductive models can be understood as if each premise is separately evaluated as accepted or not accepted given the problem at hand (context of consequences), which then allows combination of singular assumptions into a (quasi-)deductive form of reasoning.

ENGINEERING MODELS: IS THERE A DIFFERENCE?

C C Bissell
Open University (UK)

Engineeering, like science, uses many mathematical descriptions of the world. Because engineering models use many of the same mathematical techniques as scientific models (differential equations, Fourier and Laplace transformations, vectors, tensors, for example) it is easy to assume that they are one and the same in essence. Yet in the case of engineering (and technology in general) such models are likely to be used more for design than for understanding the natural world. This means that historically there has been just as great – if not greater – emphasis on rules-of-thumb, charts, and empirical models as there has been on analytical models (although the latter have also been vitally important in areas such as electronics, mechanics, chemical and civil engineering, and so on).

This paper will examine some of the characteristics of technological/engineering models that are likely to be unfamiliar to those who are interested primarily the history and philosophy of science, and which differentiate technological models from scientific ones. Themes that will be highlighted include:

- the role of language: the models developed for engineering design have resulted in new ways of talking about technological systems
- communities of practice: related to the previous point, particular engineering communities have particular ways of sharing and developing knowledge
- graphical (re)presentation: engineers have developed many ways of reducing quite complex mathematical models to more simple representations (examples will be given from information engineeering)
- reification: highly abstract mathematical models are turned into ‘objects’ that can be manipulated almost like components of a physical system
- machines: not only the currently ubiquitous digital computer, but also older analogue devices – slide rules, physical models, wind tunnels and other small-scale simulators, as well as mechanical, electrical and electronic analogue computers

Engineering models of the type discussed in the paper are not always highly valued in formal engineering education at university level, which often takes an “applied science” approach close to that of the natural sciences (something that can result in disaffection on the part of students). Yet in an informal context, such as laboratories, industrial placements, and so on, a very different situation obtains. The paper will also consider such epistemological aspects, as well as the status of different types of models within the engineering education community.

SCIENTIFIC CREATIVITY AS STRUCTURED IMAGINATION

Helen De Cruz
Vrije Universiteit Brussel

What mechanisms underlie scientific creativity; what enables scientists to make significant contributions to their disciplines? The quest by philosophers of science for some rationale behind scientific discovery and creativity has been recently joined by cognitive scientists. They examine what guides the scientific process and in what ways it resembles or differs from ordinary, everyday thought. In this paper I offer an analysis of the nature of scientific creativity based on theoretical models and experimental results of the cognitive sciences. The core idea is that scientific creativity—like other forms of creativity—is structured and constrained by prior ontological expectations. Inductive inferences and causal reasoning processes in laypeople and even young children are based on intuitive ontological expectations, suggesting that this may be a stable and universal feature of human cognition (De Cruz & De Smedt, in press). For example, when subjects are asked to invent extraterrestrial beings, they do not produce a limitless variety of creatures, but rather draw upon their knowledge of terrestrial life forms, such as that animals possess sense organs and exhibit bilateral symmetry (Ward, 1994). Likewise, religious ideas across cultures do not display an unlimited variability, but are constrained by ontological expectations, e.g., gods, like other agents, are invariably conceptualized as having desires, emotions and intentions (Boyer, 2001). Like other people, scientists are guided in their research by implicit and explicit ontological assumptions, by which they a priori gauge the outcome of an experimental setup or assess the plausibility of a result. Such ontological expectations play a major role in scientific understanding, an account that accords well with previous findings from philosophy of science (e.g., De Regt & Dieks, 2005). While ontological expectations can explain the constraints on scientific creativity, they cannot account for major shifts in scientific understanding. Applying results from creativity research in everyday thought on scientific practice (e.g., Estes & Ward, 2002), I suggest that scientific creativity arises when scientists apply the ontological structure of one domain to a different target domain. I illustrate this model of scientific creativity with examples from the history of science, such as Harvey, who imported concepts from physics and mathematics to elucidate the human circulatory system, an understanding that would have been impossible if solely based on the ontology underlying biological explanations of that time.

Boyer, P. (2001). Religion explained. The evolutionary origins of religious thought. New York: Basic Books.
De Cruz, H., & De Smedt, J. (in press). The role of intuitive ontologies in scientific understanding—The case of human evolution. Biology and Philosophy.
De Regt, H.W., & Dieks, D. (2005). A contextual approach to scientific understanding. Synthese, 144, 137–170.
Estes, Z. & Ward, T.B. (2002). The emergence of novel attributes in concept modification. Creativity Research Journal, 14, 149–156.
Ward, T.B. (1994). Structured imagination: The role of category structure in exemplar generation. Cognitive Psychology, 27, 1–40.

PRACTICES OF MATHEMATIZATION

Fokko Jan Dijksterhuis
University of Twente

Mathematization – both as a historical and a epistemic phenomenon – is commonly understood as the application of mathematics to natural, technical, societal objects. Such an understanding raises a number of questions: what mathematics is applied, what does application involve, what does this application of mathematics produce? Is the mathematics some kind of ready-made that can be pasted upon non-mathematical objects producing specific interpretations of mathematical structures without essentially altering them? Or does the process of application involves transformations of mathematical conceptions resulting in new modes of mathematical reasoning? In other words: what does it mean to develop mathematics in natural and other domains and how is this brought about?

In this paper I will approach this subject-matter from a historical perspective, in particular that of early-modern history of science. From this perspective the above questions become even more pressing because our modern conceptions of mathematics, application, etc. did not exist. The distinction of pure and applied mathematics is a nineteenth-century invention. Prior to the rationalizing strategies of Lagrange, Cauchy and the like, mathematics had an emperical basis that is best exemplified by the early-modern concept of ‘mixed mathematics’. In such diverse fields as mechanics, optics, navigation, surveying mathematics was pursued rather than imported from some external domain of pure mathematics. Mathematics was a broad domain of heterogenuous mathematical pursuits, the stratification of which still needs historical clarification. In the meantime, the sixteenth, seventeenth and eighteenth centuries witnessed a tremendous spread of mathematical practices. Mathematical reasoning entered countless new domains of inquiry and invention: heavenly and terrestrial motions, streams of airs and fluids, the nature of light, chance, ships, and so on, and so on. It is not without reason that mathematization has been regarded as a defining characteristic of the Scientific Revolution. The question is how this proces of mathematization took place. I will confront this question by looking at a few historical instances of mathematization.

For example Christiaan Huygens’ wave theory of light (1677), in which he developed a mathematical structure for the motions of ethereal particles that account for light. I will argue that, in effect, he managed to introduce mathematical reasoning in the natural philosophical domains of light physics. I will further argue that this mathematization consisted of the extension of his prior pursuit of geometrical optics – the analyses of light rays refracted in lenses – towards questions of the physical nature of light. Mathematization, in other words, consisted of the transfer of a mathematical practice to a new domain of natural inquiry. Likewise, Charles-Augustion Coulomb succesfull determination of electrostatic and magnetic forces (1787) consisted of a transfer of mathematical practices to new domains. Coulomb built upon his experiences as engineer and instrument designer when confronting the experimental philosophical question of the measure of electricity and magnetism. In so doing he went beyond the Newtonian mode of elementary analysis of forces by developing a material model of his analysis, the torsion balance.

Such transfers of practices also entail transfers of knowledge claims. Early modern mathematics, natural philosophy, engineering, each had their own conceptions of the truth, range, foundation and purpose of knowledge and a transfer may also imply the introduction of foreign conceptions. Huygens developed quite a novel conception of natural philosophical truth in his wave theory of light, privileging comprehensibility rather than certainty. Exploring circulation of practices between knowledge traditions (in a Kuhnian sense) I will try to develop a historically informed understanding of mathematization in inquiry and invention.

Explanation, Prediction, and Weight of Evidence

Heather Douglas
University of Tennessee

In evaluating evidence for use in policy-making, the standard approach used to be focused on one particularly well done toxicological study (e.g. with a well characterized strain of animal, a large study population, appropriate dose levels, and a properly timed dosing regime), which was then utilized for setting acceptable exposure levels for humans. However, concerns over the applicability of animal studies for humans have risen, and some epidemiological studies have suggested effects (or a lack of effects) where animal studies have suggested the opposite outcome. At the same time, studies of the biochemistry at the cellular level have increased understandings of mechanisms of action. With increased attention to these additional bodies of evidence, scientists are increasingly called on to "weigh" a body of evidence as a whole, rather than utilize a more narrow subset of it, when making claims about the likely risks posed by substances of concern.

In traditional philosophy of science contexts, "weight of evidence" refers to the strength of support a piece of evidence e lends to a hypothesis H. (see, e.g., Good 1982, 1987) Such formal confirmation considerations offer little guidance to scientists in the policy context, where weight of evidence refers instead to what to make of a complex body of evidence from multiple disciplines. The question is not how much support does evidence give to the hypothesis but rather what does the evidence as a whole, potentially contradictory as it often seems, indicate. In other words, it is combining all of the evidence into a coherent e that is needed.

In order to approach this problem, this paper will argue that thinking about the relationship between explanation and prediction provides useful guidance. What the scientist in the policy context seeks are reliable predictions above all else. Explanations that don't serve this end are of little use. But it is in constructing explanatory accounts of the complex body of evidence that the weighing occurs. Indeed, in the most recent EPA cancer risk assessment guidelines, "weight-of-evidence narratives" play a key role. So how should scientists construct explanatory accounts of bodies of evidence in ways that will enhance their predictive reliability?

The paper will argue for an approach that attempts to guard against ad hocery in the dismissal of evidence that does not fit a preferred explanatory framework. Prevention of early dismissal of problematic evidence and the generation of rival explanatory accounts are central principles of the approach. However, there is additionally the need to push the best explanatory accounts available. That is, scientists should be keen to develop additional readily testable predictions from the accounts with the most explanatory power, in order to fulfill the explanatory promise of the accounts and to examine their reliability before widespread acceptance. This means that explanatory power per se should not be a reason to accept a weight of evidence account. It is the utilization of explanatory power to generate new, testable predictions that is important. The success of such predictions should then bolster our confidence in our weighing of the evidence.

PARTICIPATION OR DEMARCATION? ANIMAL SCIENCE AND ANIMAL ETHICS IN ACTION

Clemens Driessen
Wageningen University

Pressing ethical issues have emerged in livestock farming over the last decades. Whereas animal production in the second half of the twentieth century developed almost solely in terms of increasing production volumes, downsides of this development into intensive farming have also become apparent. Apart from production surpluses, these most notably are environmental degradation and problems of animal welfare. The aims of animal scientists have changed accordingly. Having previously been instrumental in creating high yielding breeds and farming systems, now their work is understood in terms of fostering a more complex set of parameters related to sustainability and animal welfare.
In this paper the case of animal welfare is taken up to study approaches of animal scientists in dealing with ethical issues. In animal welfare studies science and ethics, doing research and making value judgments, are found to be blended activities. Defining and standardising farm animal welfare criteria can for instance be done in terms of behavioural and veterinarian indicators, and in terms of qualities of animals, their housing and their management. Ethical theories defending the importance of animal welfare and aimed at helping to decide on difficult issues diverge as well.
A number of animal scientists working for a large publicly funded research institute in the Netherlands were studied. The animal scientists take part in various research and development projects, some of which involve farmers, NGOs and agribusiness corporations. These projects aim in various ways at improving animal welfare by developing new farming systems and practices.
Considerable differences were found to exist in the ways animal scientists dealt with ethical issues. These are related to ways of approaching power structures and dispersed decision-making, the type of governmental regulations envisioned, perceptions of scientific uncertainties and distributions of knowledge, and the need for farm level implementation.
Different modes of dealing with these issues are discerned. First, an ethical approach aimed at problem solving and dealing with uncertainties and ambiguities by means of participatory schemes. Second, an approach aimed at criticising and controlling livestock farming by means of demarcating science and ethics and sticking to strict scientific norms. And third, an approach that is aimed at creating innovation networks and enabling farmers themselves to deal with ethical issues.
The implications of these approaches for blends of scientific research and ethical considerations are discussed. It is argued that in a field like animal welfare studies, the aim of science should not necessarily be understood as representing the animal scientifically but as ethically motivated representing of the interests of animals in the design of production systems. In that way the choices and priorities that are part of animal science in practice are made more explicit, enabling scrutiny of both science and ethics.

STATING THE "HARD PROBLEM" OF SCIENTIFIC UNDERSTANDING

Steffen Ducheyne
Universiteit van Gent

According to De Regt and Dieks's CIT–criterion, it suffices that a scientist recognizes the "qualitatively characteristic consequences of T without performing exact calculations" to obtain scientific understanding. On this account, Newton's recognition that the Moon constantly falls toward the earth, for instance, would be enough to provide scientific understanding of that phenomenon. However, I claim that it is only by making the hypothesis that the Moon is attracted by the earth empirically significant that proper scientific understanding is provided. Mind that the contrast I draw between potential understanding and proper understanding does not correspond to the difference between true or false theories (or true and false explanation) (this is De Regt's criticism on Trout, see De Regt, 2003, pp. 107-108; cf. Trout 2004, pp. 203-205). Whether a hypothesis (which is accepted by a scientist in a certain context) is objectively true or not is an entirely different debate, which has already been the focus of several volumes. The distinction I make here refers to fact that, in the case of proper understanding, a scientist is able to ascertain what the concrete, testable consequences of a theory are. This says nothing about whether these consequences are further confirmed of justified. One may therefore distinguish a third kind of understanding: justified understanding, which occurs when the empirical consequences of a properly understood theory turn out to be confirmed. It is only by demonstrating that the value of terrestrial gravitation, predicted by the assumption that the Moon is drawn by an inverse-square centripetal force, agrees to measurements of terrestrial gravity that justified scientific understanding is established. In other words, the problem for De Regt and Dieks is that such qualitative characteristic consequences need to be translated into empirically significant consequences in order to establish proper scientific understanding and need to be tested by phenomena in order to establish justified scientific understanding. Needless to say that potential understanding can be very promising from a heuristic point of view. Such qualitative intuitions or working hypotheses are the necessary point of departure of every scientist. However, I claim, that to provide proper scientific understanding means rendering qualitative intuitions empirically significant. In other words, proper scientific understanding amounts to seeing how a theory generates its empirical consequences. Abstract theory is interpreted empirically; a working hypothesis is thought through.
The "hard problem" of scientific understanding consists in providing insight into the ways in which scientists manipulate abstract theories into empirically testable interpretations of concrete natural phenomena. In the end, I have arrived at a novel way stating the "hard problem" of scientific understanding.

De Regt H.W. (2004), Discussion Note: Making Sense of Understanding, Philosophy of Science 71, 98-109.
De Regt H. W. & Dieks, D. (2005), A Contextual Approach To Scientific Understanding, Synthese 144, 137-170.
Trout, J.D. (2002), Scientific Understanding and the Sense of Understanding, Philosophy of Science 69,212-233.
Trout, J.D. (2005), Paying the Price for a Theory of Explanation: De Regt's Discussion of Trout, Philosophy of Science 72, pp. 198-208.

FOUND SCIENCE: FOUNDING 'RACE' IN BIOMEDICAL SCIENCE

Sophia Efstathiou
UC San Diego

Two associations are drawn in recent genetics literature. (1) The structure of human genetic variation corresponds to continental geographical regions (Rosenberg et al. 2002, 2005). (2) The structure of human genetic variation in the US falls under self-identified race/ethnicity categories (Tang et al. 2005). These results suggest that common 'race' categories can well approximate human genetic variation (Tang et al. 2005).

I call this a case of "found science". By analogy to found art objects, a ready-made object foreign –and even counter- to the domain of science can become found and founded as science –and thereupon function as science. I argue that the installation of the common category "race" in the context of genetics is a case of found science. A ready-made notion of 'race' is found in science by being installed in the US census and used to stratify medical data and it is founded as science by being installed in the context of interests (epistemic and pragmatic ones) and the spatial contexts (physical and discursive ones) wherein biomedical science is practiced.

Found science is an account of how common entities can enter the practice of science. It is also an account for how found entities come to differ from ordinary entities. Founding tools are used to install the common entities in scientific contexts of use and interests. These tools must find these entities in the contexts of science before they can found them. This means that a found scientific entity is a re-articulation of an ordinary entity in the terms particular to the scientific context of use and interest in question. The found scientific entity should not be mistaken for the same (one) ordinary entity. I use this frame to argue that 'found race' notions are not 'race'. And I demonstrate the frame is salient by examining how found race notions differ across the biomedical domains of epidemiology and genetics.

Falush Daniel, Matthew Stephens and Jonathan K. Pritchard (2003), "Inference of Population Structure Using Multilocus Genotype Data: Linked Loci and Correlated Allele Frequencies", Genetics 164: 1567-1587 (August 2003)
Pritchard JK, Matthew Stephens and Peter Donnely (2000), "Inference of Population Structure Using Multilocus Genotype Data", Genetics 155, 945-959 (June 2000)
Root Michael (2003), "The Use of Race is Medicine as a Proxy for Genetic Differences", Philosophy of Science, (2003) 70: 1173-1183
Rosenberg et al. (2002), "Genetic Structure of Human Populations", Science (2002) 298: 2381- 2385
-------- (2005) "Clines, Clusters, and the Effect of Study Design on the Inference of Human Population Structure", PLoS Genetics, www.plosgenetics.com, Dec 2005, Vol 1, Issue 6 e70 pp 0660-0671
Tang Hua, Neil Risch et al. (2005), "Genetic structure, Self-Identified Race/Ethnicity, and confounding in case-control association studies" Am. J. Hum. Genet. (2005) 76:268–275

The role of analogies for scientific understanding: An example from cognitive science

Kai Eigner
Vrije Universiteit Amsterdam

In their endeavor to understand reality, scientists formulate models that represent phenomena in the world. (1)In order to justify that these models are good representation of aspects of reality it is not sufficient to mention the similarities between models and reality. Similarities and differences can always be found, but these are not always relevant. For a model to be a good representation it is required firstly that scientists are able to explicitly indicate the relevant aspects of it, and secondly that they can indicate the parallels between these aspects and aspects of reality. In other words, a model is a good representation of reality if scientists are able to indicate the relations of analogy between the model and reality.

Philosophy of science literature lacks a substantial analysis of this ability to indicate the relations of analogy. Prominent philosophers of science who have written about models and analogies, such as Mary Hesse, have not made clear what this ability consists in. For instance when she discusses the relations of analogy between sound and light, Hesse argues that our knowledge that the amplitude of sound waves determines the volume of sound “immediately makes it reasonable” to suppose that the amplitude of light waves determines the brightness of light.(2) Although Hesse suggests that we can find these analogies in our language, she does not clarify how this works and it remains unclear why it is immediately reasonable to formulate these relations of analogy.

In my presentation I will discuss how scientists formulate these relations in practice. I will investigate how they assess the relevance of different aspects of the models and how they determine whether these aspects have similarities with reality. In this investigation I will make use of an example from psychology.

A scientific discipline in which the development of models is highly valued is cognitive psychology. In the 1950’s, a driving force in the development of this discipline came from the analysis of humans as information processors. Psychologists incorporated terms from information theory and used the computer as metaphor for human behavior. They argued that humans have limited capacity for the intake and storage of information (notated in bits). Humans receive information from the environment, and this information is encoded, manipulated, stored, and eventually retrieved. One of the pioneers of cognitive psychology was Donald Broadbent (1926 – 1993). He introduced the information processing paradigm in his research of attention, decision, and memory. Broadbent was inspired by the methods of engineers from information technology. He was for instance the first psychologist in modern times to use flow charts in the development of models of human information processing. Broadbent related his information theoretical models to aspects of human behavior by pointing out certain relations of analogy. (3) In my presentation I will investigate why Broadbent considered these relationships to be warranted. Subsequently I will present more general conclusions regarding the use of relations of analogy in science.

1. Giere, R.N., "How Models Are Used to Represent Reality", Philosophy of Science 71 (2004) 742-752.
2. Hesse, M.B., Models and Analogies in Science (Sheed and Ward, London and New York: 1963) p. 34.
3. Broadbent, D.E., "A Mechanical Model for Human Attention and Immediate Memory", Psychological Review 64, 3 (1957) 205-215.

RE-READING HANSON: OBSERVATIONS AND THEIR OBJECTS

Uljana Feest
TU Berlin

In the philosophy of science the topic of observation is usually discussed in relation to the question of how scientific observations can provide evidence for a theory or hypothesis. As is well known, the very notion that observations can play this role was called into question by arguments about theoryladenness and underdetermination. These arguments, in turn, have prompted other philosophers to argue (often by reference to examples from scientific practice) that these threats do not in fact exist, or – to the extent that they exist – that they can be dealt with by the methods of scientific inquiry. While sympathetic to these arguments, it seems to me that the defenders of objectivity and rationality have let themselves be pushed somewhat into the defensive. As a result they have focused on arguing against the supposed threats of theory-ladenness, rather than inquiring into the positive contributions that scientific observations (and, if you will, their theory-ladenness) make to the generation of scientific knowledge. My own take in this paper will be slightly different, namely to raise the question of what is the context in, and purpose for which, empirical observations are made in scientific research, and how an analysis of this question might affect our very understanding of "observation".

In the existing literature, there seems to be an implicit presupposition to the effect that the purpose of making scientific observations or of gathering empirical data is that of justifying or confirming theories. This is interesting, given that Norwood Russell Hanson (1958), when he famously put the theory-ladenness of observation on the philosophical agenda, did so in the context of talking about scientific discovery, i.e., about the construction of theories. It is this latter topic that I would like to address. In particular, I am interested in the fact that in the discovery process, observations often take place in the context of relative ignorance. In other words, scientists are not sure what it is that they are observing. In fact (and I am being deliberately ambiguous here), they do empirical work precisely in order to find out what it is they are observing. This raises philosophical questions about the activity of scientific observation in the discovery process. I will argue that the very concept of observation, just like that of discovery, conceptually requires the idea that there is an object of observation. When we read Hanson's work on theory-ladenness in conjunction with his work on scientific discovery, it emerges that we do not need to attribute to him some of the more radically relativist views sometimes associated with the notion of theory-ladenness. The question he was asking was how empirical scientists come to an understanding of what is their object of observation. In my paper, I will endorse and develop this question on two fronts: First, I will provide a brief overview of the ways in which philosophical discourse about observation has shifted throughout the 20th century even amongst the more "conservative" philosophers of science, making room for a renewed reading of Hanson's objectives. Second, I will discuss some options of giving a philosophical account of how to think about the objects of observation in the process of scientific discovery.

UNDERSTANDING FORMAL METHODS AS MODELS – THE CASE OF CAUSAL INFERENCE

Damien Fennell
LSE

There are many different formal methods of causal inference. To name but a few, there is structural equation models in econometrics, potential response models and the recent development of Bayesian networks. The purpose of such methods is to present a mathematical and/or statistical tool to conceptualise and find out about causes. These methods have been widely adopted by scientists and some have even been used to present justifications of widely-used, experimental methods such as randomized controlled trials. The formal methods are particularly influential in sciences where experimentation is difficult, where the appeal to a formal method can be used to make explicit and justify assumptions on which causal claims rest.

These methods of causal inference, when formalised in a complete way, that is, where a concept of cause is defined and a deductive analysis presented of how the underlying causes can be identified, and strengths measured from observation, provide a 'model of causal inference'. This paper argues that the term 'model' is a crucial. It is a necessary response to the view that some particular formal method of causal inference has (or can have) the 'right' or 'universal' concept of cause. It argues that different – and often irreconcilable – concepts of cause have their uses in different contexts. Each method of causal inference presents one idealised way to make inferences to what is represented by their particular concept of a cause. Yet, typically, there are different ways to find out about that kind of cause. So, these methods present one concept of cause among many and one method among many for finding out about that kind of cause. The methods are idealisations, since many complexities, conceptual and epistemic, are assumed away. In this way, these formalisations of causal inference are 'models'. They present toy-like simplifications that can, under certain circumstances, be used to understand, engage with and sometimes effectively control the world around us.

Drawing on the recent literature on models in philosophy of science, the paper argues that revising our view of methods as models, opens up the opportunity for more constructive methodological analysis. It argues that the arid, abstract philosophical arguments over which concept of causation is best universally, can be transformed into more insightful discussions of the strengths and limits of various methods when recast as arguments over the appropriateness of models of causes and their inference for the context to which they are applied.

TOTAL EVIDENCE AND EMPIRICAL INDISPENSABILITY: WHY ALL (OR EVEN MORE) EVIDENCE ISN'T ALWAYS BETTER IN SCIENTIFIC PRACTICE

Grant Fisher
University College London

The total evidence requirement is a normative rule that underpins the justification of belief by evidence. It is instantiated broadly in epistemology and in a variety of accounts of confirmation particularly. In its simplest terms, the requirement stipulates that belief in a hypothesis should be justified by the all evidence at our disposal. Belief in a hypothesis on the grounds that it is supported by some evidence when there is also disconfirming evidence is no justification for belief at all. In confirmation theory, Carnap employs the total evidence requirement as a methodological rule for the application of inductive logic. It also underpins van Fraassen's concept of empirical adequacy. However, the total evidence is objectionable on the grounds that it is aimed at ideal rather than actual agents. The collection, interpretation and assessment of all evidence, even if limited to the domain of "relevant evidence", is unrealisable in practice. Goldman argues that the requirement fails to accommodate the cognitive limits of actual agents and conflicts with the "ought implies can" maxim normally adopted in ethics. While I agree that the total evidence requirement is practically unrealisable and is therefore not a normatively binding rule for actual agents, an epistemic position must be articulated which explains what beliefs we can have in empirically inadequate theories and models and what warrant we have for those beliefs.

My aim in this paper is to undermine the intuition that predictive success with respect to all (or even "more") available evidence is nominally the basis for the justification of beliefs in scientific practice. The problem is not merely that the total evidence requirement is aimed at ideal rather than actual agents; the requirement obfuscates the epistemic status of empirically inadequate hypotheses and models that nevertheless serve an essential practical function in science. Even empirically inadequate hypothesis or models can have some predictive successes. But partial predictive success does not, by itself, ameliorate our understanding of the epistemic justification of theories or models that fail to predict all evidence in their domains. I argue that in some crucial cases, empirically inadequate hypotheses and models perform a critical function in scientific practice to such an extent that some predictions cannot even be attempted without them. For example, the nuclear shell model is notable for a number of predictive failures in its domain. While other models of the nucleus are relatively empirically successful, the successful quantitative prediction of some nuclear properties cannot be done without the assumption of independent particle motions. The shell model has to be used as a matter of practical necessity. In cases such as this, although partial predictive success and practical necessity does not warrant belief in empirically adequacy, it does warrant the belief that a hypothesis or model is what I shall call "empirically indispensable". Empirical indispensability is a crucial and hitherto unarticulated belief that applies to actual agents engaged in scientific practice.

DATA AND PHENOMENA

Stuart Gluck
University of Twente

Bogen and Woodward (1988) have proposed a distinction between data and phenomena which they think is crucial for understanding scientific practice. Loosely speaking, data are the observations reported by experimental scientists, while phenomena are objective, stable features of the world to which scientists infer based on reliable data. Theories predict and explain facts about phenomena, not data, and so claims about phenomena serve as the evidence in support of a theory. Their proposal is important—it was among the first work by analytic philosophers that was attentive to the intricacies of experimental scientific practice—and interesting—it promises a fundamental framework for conceiving of scientific methodology—but it has, surprisingly, generated only a moderate amount of direct discussion in the philosophy of science literature. For example, James R. Brown (1994) endorses it with minor modifications, James McAllister (1997) agrees that it is an important distinction but argues that phenomena must be investigator relative rather than objective features of the world as Bogen and Woodward claim, while Bruce Glymour (2000) argues that the distinction is at best superfluous and at worst misleading. This is a careful consideration of Bogen and Woodward's proposal, including an evaluation of the criticisms of McAllister and Glymour.

Bogen and Woodward suggest that phenomena do not explain data. This is crucial, since otherwise theories would explain phenomena and phenomena in turn data, effectively making phenomena low-level theories. Instead they characterize the data-phenomena relationship by discussing a number of considerations one can use to justify claims about phenomena from data, which revolve around assessing the reliability of the data. These considerations are justificatory, leaving open the question of how scientists discern or discover phenomena in the first place. The suggestion is that phenomena manifest themselves as patterns in data sets. McAllister argues that there are always infinitely many patterns in any data set, and so the choice of one as being a phenomenon is simply stipulated by the investigator. Glymour suggests that the fact that statistical inferences always move from a claim about sample statistics to the inferred proposition (a sort of double inference) already captures in a formal way anything worthwhile in the data-phenomena distinction. But it seems that treating phenomena as patterns in data sets misses a crucial point Hanson made about the relationship between hypotheses and observations, and which would appear just as relevant for Bogen and Woodward's account of data and phenomena: phenomena are not merely summaries of the data. If phenomena don't explain data, then at least there is something more to them than just patterns, summaries, or statistical features. What could this be? Bogen and Woodward suggest they want to devalue a consideration common in philosophy of science which they think is an artifact of British Empiricism, namely, that perception and sense experience have an epistemologically privileged status regarding the justification of beliefs about the natural world. Whether Bogen and Woodward's distinction can thus be made more cogent in a less empiricist sort of framework will be explored.

EMBEDDING OF VALUES IN SCIENCE: THE CASE OF ADHD RESEARCH

Susan Hawthorne
University of Minnesota

The convergence of interests of scientists who study attention-deficit/hyperactivity disorder (ADHD) with those of educators, parents, clinicians, ADHD-diagnosable individuals, and others has resulted in an alliance (in Bruno Latour's sense) that shapes the scientific and cultural understanding of ADHD. Elsewhere I establish that (1) cultural disvaluation of ADHD-associated traits is embedded in the diagnostic criteria, and (2) that social and scientific practices, as well as scientific results, tend to reinforce legitimacy of the ADHD category in positive feedback loops. In this paper, I argue that the allies' goals and values are embedded as well in the content of the science that defines and investigates ADHD.

Embedding occurs in at least two ways. Broadly, funding patterns that back the interests of science's allies embed those interests in the science by influencing the direction of research. Publication rates show that ADHD research in the US is primarily directed to physiological and psychological basic research, and to clinical research that includes drug treatment. In contrast, significantly less research targets (for example) ADHD epidemiology, or strategies for teaching ADHD-diagnosable children. As a result, despite the plurality of sciences involved in investigating ADHD, the ADHD literature is focused on particular ADHD-associated behaviors or traits and certain forms of management, possibly to the detriment of understanding others.

Deeper, however, is the embedding of goals and values in the terminology, variables, and data interpretation that are integral to basic and clinical research. The embedding occurs because the research is planned, performed, analyzed, and described in the complex scientific/cultural context of associations with ADHD. It might seem that research concerning particular aspects of ADHD—e.g., its genetics, neurophysiology, or neuroanatomy—could be isolated from this context; similarly for specific variables of interest, such as the DRD4 7-repeat allele, performance on Go/No Go tasks, or caudate volume. Indeed, one goal of most studies is to find a (value neutral) difference between "ADHD" and "control" groups with respect to a given variable. However, the difference between groups with respect to the variable is linked by empirical evidence or hypothesis to the between-groups difference with respect to ADHD status. Possession of the allele, "poor" performance on the Go/No Go task, or smaller caudate volume become associated with disvalued ADHD status. Thus, in describing results, language of "difference" often shifts to language of "deficiency" or another value-valenced term. Usually, no argument is supplied for this transition, because the "obviousness" of the valence in the context of ADHD hides the need for a rationale. Thus, the variables, and the terminology used to describe them, take on value-valenced meanings via association with ADHD, and most interpretations of the data assume this value valence in analyzing the results. (As Putnam puts it, description and prescription become inextricably entangled.) The further step in which scientists recommend clinical, educational, or policy approaches introduces an opportunity for embedding of goals or values in the recommendations. Finally, reiteration of value valence in thousands of scientific papers, and in translations to popular media, reinforces the pre-existing disvaluation of ADHD.

EXPLANATION, UNDERSTANDING AND OPACITY

Cyrille Imbert
PanthÃ©on-Sorbonne

Dominant accounts of scientific explanation agree on the fact that explanations provide understanding. It does so by showing how something was to be expected (in Hempel's model) or how things work (in Salmon's causal model), by shedding light on some portion of an ideal explanatory text (according to Railton), or by using patterns that unify our beliefs (according to Kitcher).

My first point is that all these accounts are so far badly suited to show how explanation does help develop actual understanding in scientific practice. For an explanation to bring understanding, one must be able to survey the explanation and this takes cognitive or computational resources. Since none of these accounts make mention of any kind of such resources, the concept of understanding that they rely on is at best woefully underdescribed. This is the understanding that an archangelic creature endowed with potentially infinite cognitive resources could get and this is typical of what Humphreys (2004) has labelled "science in principle". I further argue that a better understanding of a system is usually associated with a decrease in the required computational resources. I illustrate this with examples taken from fluid dynamics. Overall, this shows that the question of resources should at least be discussed when debating about scientific explanation.

The second part of this presentation is devoted to clear the way for a notion of explanation that enables one to describe how one more or less easily understands the explained facts. My aim is to start filling the gap between the rich literature about explanation and complexity theory, which precisely studies needs in computational resources. To that effect, I sketch a notion, opacity, which is defined in terms of the amount of computational resources that one needs in order to grasp an explanation and use it. I do not rely on any peculiar theory of explanation for that. Instead, I list constraints that must be fulfilled for the notion of opacity to work. I show that for the same system, opacity is relative to the properties that one wants to account for and that, for the same system and property, opacity depends on the facts from which one starts the building of the explanation. I argue that that does not imply that opacity is a totally epistemic notion, but that it does, at least partly, characterize systems themselves (relatively to some properties). For example, for a system that is just before a fully-developed turbulent regime, opacity is high during turbulence bursts and low during laminar phases, and this seems to be an intrinsic feature.

I conclude that 1. it will however be difficult for opacity to be a totally intrinsic feature, since applying complexity theory implies some relativity to a format. 2. a concept like opacity should help us refine how actual fundamental and applied science works, since it relies more and more on computers. 3. a theory of explanation that would help show how to use the notion properly would really make a point.

Humphreys, Paul, Extending Ourselves, OUP, 2004.

CAUSAL UNDERSTANDING IN BIOLOGY AND ECONOMICS

Michael Joffe

The motivating impulse for this paper is that biology has become a highly successful science in the past century, and that biological examples can be used to illuminate certain aspects of economics, despite the large differences between them. Thus the careful use of analogy, that explores contrasts as well as parallels between these two disciplines, can help to explore causal relations in economics, which are widely believed to be inadequately characterised in economics as currently practiced, perhaps especially in mainstream economics.

If we want to understand causal processes, the best topics for study are areas of science where the explanations are well established. One area within biology is physiology (including biochemistry, cell biology etc): the study of the workings of the body. Whilst many aspects remain to be elucidated, physiology has developed a core knowledge base that commands universal agreement among scientists, and fulfils the other criteria of a mature, established science.

Using physiological examples that require no specialist biological knowledge, I explore what biologists mean by understanding causality. The fundamental concept is mechanism, understood either as underlying chemical/physical processes, or as a set of hypothetical properties, e.g. imperfect replication in an informative environment in the case of evolution. For example, it has long been established that propagation of a nerve impulse across a synapse is mediated chemically rather than electrically, and the chemicals involved and the processes for their synthesis and control are well understood. That one can use the apparently teleological concept of "control" indicates an important property of physiological systems, that they are relatively easy to understand because their elements are repeatable: the same components recur reliably, so that one can talk generically of "a synapse". This is because biological systems are a set of self-perpetuating and self-replicating systems, which are brought into being by evolution.

In economics, one can similarly represent an economic system, e.g. that of capitalism, as a series of self-perpetuating and self-replicating systems. Evolutionary economics seeks an analogy with evolutionary biology, although its value is somewhat limited as the underlying processes (mechanisms) are quite different in the two cases. However, there are reasons to believe that one aspect of the analogy holds, that an economic system is also composed of repeatable elements such as firms, albeit with less uniformity and predictability than in the physiological case. It is further argued that the abstract characteristics of a self-perpetuating system can illuminate the properties of the economy, e.g. the aspects of capitalism that give it a uniquely dynamic (and volatile) character.

In addition to the important differences between biological and economic "evolution", the other striking difference is the centrality of agency in economics (albeit one that is ignored in mainstream microeconomic theory). Here too biological analogy, e.g. ant behaviour, has been used with some limited success to illuminate economics. A satisfactory account of economic phenomena is likely to require the fusion of physiology-like concepts of the economic system with agency-based insights such as those that are accumulating in the relatively new sub-discipline of behavioural economics.

THE CHOICE BETWEEN EXPLANATORY AND DESCRIPTIVE THEORIES IN PHOTOELECTRICITY AND PIEZOELECTRICITY

Shaul Katzir
Bar-Ilan University

Often scientists can chose between explanatory and descriptive (or phenomenological) theories. The latter describe the phenomena under consideration with a minimal number of laws without explaining them by a deeper effect or process and without determining the nature of the basic entities involved. The former often involve unproved hypotheses about underline processes and/or entities. Although they are of different kind, hardly ever one's choice of either kind of theories is based on a principle rejection of the other kind (the mature Pierre Duhem is an exception that proves the rule). General philosophical or methodological tendencies of the scientists have some influence, but the choice is primarily shaped by the advantages and disadvantages of the theories in their specific scientific context.

In this talk I will employ my previous studies of piezoelectricity and photoelectricity to examine the different fates of the descriptive accounts for both phenomena. Discovered in the 1880s, both were 'boundary' phenomena between subdisciplines of physics. In both cases, early explanatory theories failed to account for further result, and alternative explanation were suggested. However, in piezoelectricity complicated molecular models were not adopted, while a descriptive phenomenological theory of Voigt was widely accepted. This was not the case of Richardson's descriptive theory of photoelectricity, even though in this field "classical" and semi-classical mechanisms contradicted observations, and Einstein's quantum hypothesis about the nature of light, was rejected as too radical.

I suggest the following main factors for the acceptance of the descriptive theory in one case and not the other:

Voigt's theory had the advantage of being based on a small number of assumptions, which led to mathematical elegance and diminishes its arbitrariness, two appreciated characteristics. This was not the case with Richardson's theory which in addition involved questionable assumptions, and thus could not enjoy the non speculative secure basis, which is central advantage of phenomenological theories.

The explanatory theories of piezoelectricity required complicated speculative models and unlikely structure of crystals, which were not supported by other scientific field. This is different from other cases. For example in the abnormal Zeeman effect, the motion of electrons in the atom, which was supported by many external observations, was used in (failed) attempts to explain the effect, and so here a phenomenological theory of Voigt received cold reception.

Explanatory theories in piezoelectricity suggested no new testable relations (i.e. those that were not predicted already by the phenomenological theory). In photoelectricity, the descriptive theory could only regain empirical relations predicted by the Einstein's hypothetical theory. Although logically the temporal order has no relevance to the acceptance of the theories in practice it had a clear influence . One reason for that is the feeling that later theories were adjusted to the desired results and is therefore too

arbitrary, without sufficient internal necessity. This can be expressed in a Popperian terms as a criteria for the falsifiability of the theory.
Lastly of major significance was the process which the descriptive theories bypassed. When it was considered unknown due to missing details (as in piezoelectricity) a phenomenological theory was accepted. When physicists doubts that laws are missing or inadequate, they were not satisfied with such a solution. This was the case when no complicated structure could explain the observations, and when accumulation of evidence from different phenomena suggested that the problem should be tackled rather than circumvented.

COMPUTER SIMULATION AS EXPERIMENTAL METHOD

Jeff Kochan
University of Alberta

In this paper, I will consider the methodological status of computer simulation in the physical sciences. Specifically, I will focus on the question of whether or not computer simulation can be justifiably categorized as a method of experimentation.

It is unclear whether computer simulation, when considered against received philosophies of experimentation, qualifies as an experimental method. Ian Hacking, for example, has argued that a key feature of scientific experiments is their intervention in the physical world. This criterion would seem to exclude simulation from being counted as an experimental method. However, Hacking's criterion is perhaps too narrow. It excludes a number of scientific methods which are conventionally described as experiments, but which do not obviously intervene in nature. I propose to compare computer simulation to one such very well-established form of experimentation: namely, thought experiments. Thought experiments have played an extremely important role in the development of modern science. By drawing on the philosophy of thought experiments, I aim to sketch-out the basis for a philosophical account of computer simulation as an emerging experimental method.

There has been a debate in recent years over whether or not thought experiments, as acts of imagination rather than of intervention, can yield new facts. Do thought experiments provide non-empirical access to the world? John Norton says no, arguing that thought experiments are nothing but ordinary, empirically grounded arguments in rhetorical disguise. James Robert Brown counters with a Platonic account of thought experiments as providing a priori access to the world. Richard Arthur cuts a middle path between these two positions. He argues that thought experiments do articulate new facts, but only by explicating tacitly held, empirically grounded presuppositions and exposing contradictions implicit in received theory. In this, Arthur's position is similar to that of Kuhn, who adds the stronger claim that theory and world are jointly implicated in the contradictions uncovered by thought experiments. Theory can house contradiction because the world can be experienced in contradictory ways. By exposing these contradictions, thought experiments bring consistency to our theories and coherence to our world. A similar claim might also be made for simulation experiments. Like thought experiments, simulation experiments can yield new facts by resolving contradictions implicit in the way we experience and theorise the world. In a certain sense, simulation experiments might be described as computer-aided thought experiments. To develop this claim, I will draw on the philosophy of technology, where it is commonly argued that technology is the enhancement and extension of human capacities. A hammer enhances and extends physical strength; a telephone, the ability to communicate. Insofar as thought experiments are essentially acts of imagination, I suggest that computer simulation experiments can be described as enhancing and extending the power of human imagination. On this basis, one might reasonably speculate that simulation experiments could one day play as central a role in scientific research as the historic role played by thought experiments.

THE INFLUENCE OF MEASURING TECHNOLOGY ON MODEL BUILDING IN MOLECULAR BIOLOGY

Ulrich Krohs
University of Hamburg

Until the late 1990ies, models of molecular processes on the cellular level were based on kinetic data of a few components of a biochemical pathway. The set of parameters included in such models remained limited for two reasons. Firstly, the processes were usually modeled as dynamic systems, and it was neither practical nor desirable to make these systems too complicated. Otherwise they would have lost their predictive power. Secondly, the experimental methods used for data collection allowed for determination of only a limited set of parameters in acceptable time. The data were often very precise but the measurements required purification of the molecular components of a pathway. So there was a well-established correlation of experimental and modelling techniques. Both concentrated on isolated pathways within the metabolic network of a cell.

The recent availability of high throughput analytical methods, primarily developed in the fields of genomics and proteomics, gave rise to a dramatic change of the situation. Meanwhile, it is easy to produce a flood of data not only about the structure, but also about the dynamics of genetic and metabolic systems of whole cells. Subsets of such data could be used in several cases to refine models of the "classical" type, for example the models of the circadian clock and of signalling pathways. However, the new experimental methods also gave rise to completely new modelling strategies that allow to utilize whole data sets of "-omic" dimension. The new models describe no longer isolated pathways but the dynamics of networks as wholes.

I will discuss the change in model building as mediated by the availability of the new measuring technology. Interestingly, the data gathered by the new techniques are much less precise than those obtainable with the older methods. The new large-scale models nevertheless allow for new insights into and better explanation of cellular processes. The focus of explanation, however, has changed. I will furthermore discuss this change of the explanatory focus that has occurred with the change of the experimental methods.

THE RE-CONFIGURATION OF BAYESIANISM IN RECENT STATISTICAL PRACTICE

Johannes Lenhard
Bielefeld University

The development of statistical theory and practice has been structured by applications nearly all of the time since their origins. At the beginning of the 20th century, the influential contributions of R.A. Fisher, J. Neyman, E. Pearson and others established frequentism as the leading philosophy in the statistical practice of the natural sciences and in mathematical statistics itself. Bayesianism played only a minor role there, notwithstanding its more prominent standing in philosophy of science. Recently, however, it seems to perform a significant upswing – think, for instance, of the so-called "empirical Bayes" approach now widely practised in disciplines that formerly adhered to a frequentist stance. Hence one may ask: Is scientific practise changing its philosophy? If so, what are the reasons and driving forces?

It will be argued that essential parts of Bayesian philosophy have been given up by current approaches, although they still use the label "Bayes". The main claim will be that one can observe a convergence or unification of frequentist and Bayesian elements that formerly presented themselves as incompatible or mutually opposing each other. This unification, however, is not a theoretical but rather a pragmatic one driven by instrumentation. This claim will be argued for by analyzing two examples of recent approaches, namely "empirical Bayes" and "Bayesian calibration".

Both are part of a general development: New technologies, high-throughput devices, satellite data, microarrays, etc. produce huge amounts of data while at the same time increasingly complex models and simulations are applied in a growing number of scientific fields. Classical approaches encounter difficulties in dealing with this situation. By taking advantage of new instrumentation (computer technology, including simulation and visualisation) recent statistical practice is going to change fundamental assumptions in statistical philosophy.

Empirical distributions can be dealt with directly – without modelling and formalizing a prior distribution. First, the necessary amount of data that describe the distribution is often available and, secondly, computer methods can deal with them directly. Thus radically simplifying models of data may become dispensable – changing the very rationale of modelling. So-called calibration techniques that are employed in complex models of, for instance, climate science will also be discussed. They treat prior distributions not as representing knowledge that has meaning and significance independently from the observed data and underlying models, but rather as parameters that are varied systematically to achieve a good fit to the data. Again, this approach is rendered possible only by computing techniques while, at the same time, simulation models also call for this kind of approach. This is because the internal dynamics of these models often remains partly opaque so that it may be impossible to attribute meaning to them independently from the model's performance. The paper will discuss how mathematical/ statistical instrumentation and practice mutually influence one another.

PROMOTING RESPONSIBLE SCIENCE FOR ADVISING PUBLIC POLICY: ON THE PROSPECTS AND PITFALLS OF ETHICAL CODES FOR SCIENTIFIC POLICY ADVICE

Justus Lentsch
Bielefeld University

In this paper, I will explore the prospects and pitfalls of ethical or professional codes of conduct in scientific policy advising. Science is still the major institution for producing knowledge pertaining to political decision making and regulation. As such, it remains indispensible for approaching the urgent environmental, societal and political problems our society currently faces. However, the role of science in providing advice to political decision makers and to the public for managing public policy problems such as the BSE crisis, nuclear power, genetically engineered food or animal diseases has given reason for widespread disappointment with regard to the quality and integrity of the advice given. One of the most challenging problems is the role of academic scientists having financial or partisan interests in the assessment of drugs, toxic chemicals etc. (cf. Krimsky 2003). Hence, developing procedures and guidelines for responsible and scientifically based policy advice – such as the British Chief Scientific Adviser's Guidelines – is now a major concern to scientific experts and policy makers alike (cf. Lentsch / Weingart 2006).

However, the task of formulating ethical or professional codes is not an easy one. This is, at least partly, because scientific experts have to serve two principals when advising policy: Firstly, they have the responsibility to provide expertise in a way that promotes autonomous decision-making on the part of their clients (cf. Elliott 2006). Secondly, they have to consider the needs and the concerns of the society that funds the scientific enterprise. In classical professions, an ethical code of conduct is what makes a profession deserve the trust on which society's support should base (cf. Davis 1994). However, responsible conduct in science is more than simply a matter of following everyday ethical imperatives (Kitcher 2004). Moreover, the production of scientific expertise differs essentially from basic science – with regards to objects and methods as well as to its aims and core values (cf. Jasanoff 1990). Hence, we will have to ask how to account for the different rationales and responsibilities in formulating such codes for scientific expert advising.

On the theoretical level, I will draw on recent work in social epistemology (cf. Kitcher 2001; Kourany 2007; Longino 2002), science studies (e.g. Jasanoff 2005, 2006) and the ethics of science (cf. Resnik 1998). Whereas the issues of what role values play in science (cf. e.g. Douglas 2000; Longino 2002) as well as questions about the values of science (Kitcher 2001; for critical comments on Kitcher cf. e.g. Dupré 2004) receive increasing attention, questions of the impact of institutional arrangements have rarely been addressed. Looking at the rationale and the guiding principles for formulating ethical or professional codes for the production of scientific expertise for public policy, this paper takes a first step to close this lacuna. On the level of science policy, I will discuss experiences from an ongoing project of the Berlin-Brandenburg Academy of Sciences and Humanities. The Academy undertakes to devise such guidelines, thereby addressing primarily the role of scientific expert advisers, instead of making prescriptions about the appropriate utilisation of scientific expertise by policy makers, as in most of the other guidelines. Building on this example, I will further discuss the role academies of science should play in devising guidelines to promote the responsible conduct of scientific expert advising. I will conclude with some considerations on the prospects and chances as well as on the pitfalls and limits of such ethical or professional codes as a valuable instrument not only for policing scientific expert advisers in cases of fraud, scientific misconduct and conflicts of interests, but also for encouraging scientific experts to reflect upon how to provide policy advice in an ethically and politically responsible way.

WHAT IMPLICATIONS DOES THEORY-LADEN OBSERVATION HAVE FOR SCIENTIFIC PRACTICE?

Matthew D. Lund
Rowan University

The thesis that observation inevitably contains some non-testable elements, i.e. that observation is theory-laden, is ordinarily associated with the critique of the logical positivist model of science. However, most post-positivist treatments of theory-laden observation were much more concerned with addressing the apparent subjectivity brought into science by theory-laden observation than with its consequences for scientific practice. Thus, the post-positivist discussions of theory-laden observation were primarily devoted to logical problems of traditional epistemology which just happen to arise in scientific contexts, but which could come up in other contexts as well. I argue that this preoccupation with issues that abstract away from the actual practice of science not only robs us of interesting and crucial insights into the workings of our most successful epistemic enterprise, but fundamentally misrepresents the motivations of the philosopher who first put the term "theory-laden" into the philosopher of science's vocabulary, N.R. Hanson.

Hanson's discussion of theory-laden observation was the first step in his project of developing a philosophy of science to account not merely for science's logical structure, but for its historical, practical, and conceptual aspects as well. Despite its historical importance, Hanson's account of theory-laden observation has presented some challenges in exegesis. I argue that Hanson's account of observation owes a great deal to Pierce and that if we pay attention to the pragmaticist elements in his account, we can extract an account of theory-laden observation appropriate for understanding scientific practice. In recent years, a number of philosophers have made considerable progress toward achieving a notion of theory-laden observation capable of making sense of scientific practice (Heidelberger, Radder, and Norris). Heidelberger, in particular, has presented a new typology of theory-laden observation (drawing on Kuhn and Duhem as well as Hanson) that pushes Hanson's original intuitions toward greater precision and usefulness.

I develop the following account of theory-laden observation based on the views of Hanson, Peirce, and Heidelberger. I start with Peirce's idea that entertainment of a concept entails the truth of a set of conditional statements about experience and practice, i.e. commitment to a theory has some in principle testable implications. However, if this were what concepts actually amount to, there would be no difficulties with theory-laden observation since we could just test all of a concept's implications. However, there are a number of factors that block this inclusive conditional understanding of concepts from usefully applying to certain cases; the most important impediments are psychological, historical, and practical. Thus, one may be prevented from examining certain testable implications of a theory because one's cognitive limitations do not allow those implications to be grasped. I show that historical cases of dispute over observation between scientists were resolved by appeal to testable implications, though it was often a task of some difficulty to produce a list of practical test implications. Blondlot's observations of N-Rays were theory-laden and he avoided, for psychological reasons, examining certain test implications he knew to follow from the theory of N-Rays. Nineteenth century observational astronomy was fraught with worries about observation and though the observations of individuals differed, the differences were eventually found not to be due to revisable theoretical commitments; Bessel's development of the notion of the "personal equation" was the result of serious consideration of the testable implications that would result from certain theoretical explanations of error.

ULTIMATE AND PROXIMATE EXPLANATIONS OF COOPERATIVE BEHAVIOR: PLURALITY OR INTEGRATION?

Caterina Marchionni
Erasmus University Rotterdam

Jack Vromen
Erasmus University Rotterdam

The behavioral sciences are characterized by a multiplicity of forms of explanation. Mayr's famous distinction between ultimate and proximate explanations has been generally invoked to make sense of at least part of this plurality. Whereas evolutionary theorizing explains human behavior by appeal to evolutionary forces (such as, notably, natural selection) working in the past, proximate explanations explain by appeal to current cognitive and psychological mechanisms. Each is held to be a legitimate form of explanation, and to be indispensable for a full understanding of behavior. At the same time the belief in a single unified theory of human behavior has revived in recent years (e.g. Glimcher and Rustichini 2004; Gintis 2006), suggesting that evolutionary and proximate explanations can, and perhaps should, be integrated. In this paper we enter the debate by scrutinizing the relation between proximate explanations and ultimate explanations of human cooperative behavior. We show that there are different ways in which ultimate and proximate explanations complement each other, not all of which equally support plurality of explanations. First, in some cases ultimate explanations are thought to directly account for behavior (for example, Ken Binmore's game-theoretical account of reciprocity). Second, ultimate explanations are held to explain behavior only indirectly, that is, by explaining proximate mechanisms (for example, Robert Frank's account of emotions as commitment devices and evolutionary psychology). Finally, in still other cases, there is only one explanation that appeals to both proximate mechanisms and evolutionary forces to explain current proximate mechanisms, and thereby behavior (for example, dual inheritance co-evolution theory). These three kinds of complementarity correspond to distinct ways in which different approaches (both within and across disciplines) can relate to each other in providing understanding of cooperative behavior: in particular, the approaches tend be increasingly integrated as the kind of complementarity gets stronger. If ultimate and proximate explanations both account for behavior but do so differently, then evolutionary and 'proximate' approaches can proceed without taking much notice of each other as long as their explanations are compatible. If ultimate explanations explain proximate causes, then ultimate explanation helps in identifying proximate causes ("from function to form") and, conversely, the detection of proximate causes provide an empirical test for ultimate causes ("from form to function"). In the third case, different approaches and fields are integrated to produce more complete explanations. The pursuit of more complete explanations is here driven by the recognition that ultimate explanations that completely disregard proximate causes, for example, might seriously distort the actual causal history. The current trend in the behavioral sciences is to opt for the second or third position, and hence seems to be a trend towards greater integration rather than plurality. The general lesson to be drawn is that finer-grain analyses of complementarities between actual scientific explanations are needed to illuminate kinds of inter-theoretical and inter-fields relationships, and the degree to which they support plurality vis-à-vis unity.

WHAT IS COGNITION? A VIEW FROM SCIENTIFIC PRACTICES

Sergio F. Martínez
UNAM

How do scientific practices embed and account for cognition? Two families of answers are found in the literature; we call these Cartesianism and Interactionism. Cartesianism is the classic account: cognition is the individual-level processing of representations. Scientific practices are communal processes that are aggregations of these individual-level processes. Philip Kitcher understands scientific practices in this way. Furthermore, cognition is seen to involve natural kinds of processes that belong to clear and distinct individuals. A theory of cognition as individual-level "problem-solving," as developed by Herbert Simon and recently by Gerd Gigerenzer, exemplifies this atomistic manner of understanding cognition.

On the other hand, Interactionism, which we defend, claims that scientific practices structure a variety of norms and strategies of inquiry. Joseph Rouse analyzes scientific practices along these lines. In addition, under our view, cognition is understood as including both individual and social processes. Individual-level processes of cognition we argue, contra Simon, cannot be individualized as natural kinds because the identification of such processes co-varies with different scientific practices. Furthermore, we claim that the irreducibly social processes of cognition are coordinated, and (to an important extent) constituted, by scientific practices. Such practices evolve as part of distinct historical research communities. Thus, cognition is distributed.

The operative contrasts between Cartesianism and Interactionism, then, include: (1) a focus on (a) cognition as the processing of representations occurring within individuals vs. (b) distributed cognition as an interactive activity taking place at multiple levels (e.g., agents as individuals, research groups, etc.), (2) the classification of the processes of cognition as (a) natural kinds vs. (b) kinds dependent both on social context and on the purposes for which such processes are identified for a particular investigation, (3) understanding cognition as (a) a purely "internal" process vs. (b) a complex of processes situated both "within" and "among" agents embedded in a variety of environments co-conformed by the processes themselves.

The Interactionism we wish to defend regarding cognition emerges from a concern with the development of a truly social epistemology, and is strongly inspired by interactionist views proposed by researchers as different as Rodney Brooks in AI/robotics, Susan Oyama in developmental psychology and developmental biology, and John Dewey in philosophy.

ETHICAL VALUES, SCIENTIFIC PRACTICE AND VIRTUE EPISTEMOLOGY

Isabelle Peschard
University of Twente.

Discussions of the role value judgements play in scientific activity generally start by assuming a categorization of values in distinct kinds: epistemic or cognitive and others, political, social, ethical. Second, non-epistemic values are meant to influence, at most, the choice between theories or research programs. Third, this influence would not affect the epistemic/representational content of scientific knowledge (Lacey), but would entail however, fourthly, the inability of epistemology to account for the conditions of scientific knowledge (Laudan).

I will call these four points into question. I will consider ethical value judgements, in relation to judgments of responsibility, significance and negligibility, as judgments stating what is important to us, and argue that even though ethical they can have an epistemic function. I will take up two specific cases of development of models, both addressing a 'knowledge-phenomenon': in cognitive science I will contrast models pertaining to representational and to embodied theories of cognition (Varela). In social science I will contrast deficit and participation models of public understanding of science (Wynne, Jasanoff). In these two cases, I want to show that ethical values can first influence the conception of the phenomena to be explained, what has to be accounted for, and then, through judgments of significance, influence the identification of what has to be taken into account, what kind of data count as relevant. These judgments condition the kind of possible models for the phenomenon under study. Furthermore, through judgements of negligibility ethical values can influence the assessment of models of a given kind. But if ethical values contribute to the epistemic/representational content of these models, does that imply an inability of epistemology to apply to scientific knowledge?

I will contend that it only shows a deficiency of traditional epistemology, oblivious to the conditions of formation of knowledge, and stresses the need for philosophy of science to enlarge its vision of epistemology and to benefit from recent developments in this domain. The shift in philosophy of science towards the conditions of scientific practice and formation of scientific knowledge, that is associated with the perception of the epistemic function of 'non-epistemic' values, was mirrored in epistemology by a shift towards the conditions of acquisition and enunciation of knowledge claims and beliefs. Contextual (DeRose) and virtue epistemologies (Code, Zagzebski), as developed in the two last decades, show the epistemological relevance of considerations relative to the epistemic context in the evaluation of knowledge claims and to the intellectual virtues of the epistemic agents in the formation of beliefs. The epistemological legitimacy of such considerations provides philosophy of science with a promising epistemological framework for conceiving the epistemic relevance of ethical value judgements in the production of scientific knowledge.

SEEKING REPRESENTATIONS OF PHENOMENA: PHENOMENOLOGICAL MODELS

Demetris P. Portides
University of Cyprus

Scientific models come in various guises that over time philosophers have analyzed and distinguished into different categories. One kind of model that, in my view, has not been adequately scrutinized by philosophers of science (notable exceptions are Cartwright 1983, Hartmann 1999, Morrison 1998, 1999) and which deserves more weight than that attributed to it in the literature, is phenomenological models. Phenomenological models are one species of mathematical models constructed by physicists to apply theory to phenomena. Being a species of mathematical models, their representational function, their relation to theory and to phenomena, their function as sources of knowledge, their methods of construction and more generally their importance in physics, have been overshadowed by another species of mathematical models, namely theory-driven models. The focus on theory-driven models could be attributed to the philosophical demand that questions pertaining to the uses of scientific models and foundational questions about theory structure should be addressed within a framework that assumes the unity of theory and models. This approach is manifested both by the Received View and by the Semantic View of scientific theories, both of which, it could be argued, fail to adequately address the theory/experiment relation and the representational function of scientific models. Attention to phenomenological modeling, however, could enhance our understanding on both of these matters.

I argue that phenomenological modeling can be best understood as a process that abides by theoretical constraints which are in constant interplay with experimentally determined results and progressively changing physical intuitions about the models' target physical systems. This idea is demonstrated by an analysis of the single-particle shell model of nuclear structure. I also draw attention to the fact that phenomenological models are distinct entities from theory-driven models that cannot be viewed as approximations of the latter. Thus if they represent their targets it is in virtue of their explanatory power and not because of their closeness to theory. I describe the construction of the single-particle shell model with spin-orbit coupling of the nuclear structure, and try to illustrate the complexities involved in rendering the model a (partial) representation of the nucleus. I choose this particular model to emphasize the importance of phenomenological thinking, because it is a model that relies also on theory, and thus demonstrates my thesis that it is theory together with phenomenological considerations that render the model a representation of the nucleus. It does not belong to that class of phenomenological models, such as the liquid-drop model of nuclear structure, which demonstrate a high degree of theory independence. The latter kind, although of equal significance, would guide my argument away from the interplay between theoretical considerations and phenomenological thinking in scientific model construction, that I wish to highlight. The construction process of the single-particle shell model shows the role played by the phenomenological underpinnings of the model in providing the latter with its explanatory power and thus its representational capacity. In fact, the process clearly demonstrates that without its phenomenological features the model cannot be rendered a representation of the nucleus.

METAPHORS AND SCIENTIFIC DISCOVERY

Jarmo Pulkkinen
University of Oulu

Traditionally the initial act of inventing a new scientific idea was considered to be irrelevant to philosophy of science. It was believed that scientific discovery is based on either induction from data or lucky hunch. For its part, history of science provided numerous anecdotes in which scientific discoveries were explained by sudden “flashes of insight”. Nowadays it is widely accepted that metaphors are important in the early stages of scientific discovery. They often provide the first vague hunch or insight into “how things are”. When metaphors are used as cognitive instruments, the unknown (target domain) is explained and understood with the known (source domain).

These metaphors can be classified with the help of their source domain. First, the source domain can be the physical world, i.e. living and nonliving material objects (e.g. “electron is a wave”). Second, the source domain can be the world of mental states, i.e. the world of subjective experiences. For example, in contemporary particle physics quark has a “colour”. Third, the source domain can be the realm of social relations, institutions, and structures (e.g. Plato’s midwife metaphor or “molecular chaperone” in modern biology). Next, the source domain can be the abstract product of human mind, e.g. scientific concepts and theories (e.g. “DNA is a code”). Finally, the source domain can be technology (e.g. “mind is a digital computer”).

Generally speaking, metaphors derived from different source domains provide different kinds of conceptual frameworks. Consequently, some source domains have been more important than others with regard to scientific discovery. For example, a source domain is the better the more we know about it. In particular, we have a very thorough knowledge of artifacts, i.e. maker’s knowledge. As a result, they are very useful in our attempts to explain the unknown with the known. Moreover, unlike inanimate nature and living organism, technology develops constantly. New artifacts can be used as new and possibly insightful metaphors.

On the one hand, scientific metaphors are based on the “lucky guesses” of a particular scientist. On the other hand, the use of metaphors, like all other human activities, relies on existing material and other resources. Thus, the setting in which a discovery occurs is important. The environment we live in becomes constantly “richer”, i.e. there are more possibilities to create new insightful scientific metaphors.

The classification of the different source domains of metaphor offers an interesting methodological framework that can be applied in history of science. In this respect, metaphor has also other interesting properties. A metaphor suppresses some details, emphasises others, — in short, it organizes our view of the unknown phenomenon. For example, machines may be described in terms of mathematical relations among their moving and stationary parts. When using a machine metaphor, these mathematical relations can be transferred from the source domain of technology to the target domain of physical world. Consequently, natural phenomenona can be subjected to quantitative description.

SIMPLE OR SIMPLISTIC? SCIENTISTS VIEWS ON OCCAM'S RAZOR

Hauke Riesch
University College London

Many well-known popular accounts of science, such as Sagan's famous "baloney detection kit" generally portray Occam's razor and the value of simplicity for theory choice and as a demarcation criterion, very positively. That simplicity is a good thing in science is apparently one of the least controversial philosophical concepts – debates on Occam's razor centre more on how it can be justified, rather than on whether it should be justified at all.

In a more systematic survey of 24 recently published popular science books (whose authors are scientists), the picture of what scientists themselves think about simplicity becomes a little bit more complex. While there is a general tone of acceptance of Occam's razor, there are also people who disagree with the whole concept. In the books, Occam's razor visibly fulfils several rhetorical functions of "boundary-work" between science and pseudoscience or of helping to close an argument by appeal to (philosophical) authority. There is a discernible difference between Occam's razor as a demarcation criterion between science and pseudoscience like Sagan does, and simplicity (without mentioning Occam's razor by name) as a criterion for theory choice within science. Underneath that surface however, there is a rather complex and nuanced spectrum of opinions and representations of Occam's razor, of what simplicity actually is, and of the structure of the world and of science.

The question of the value of simplicity was then put to practising scientists themselves in a series of 40 semi-structured interviews. Again the range of opinions vary enormously from a "gut-feeling" acceptance that looking for simplicity in science will yield results that are more likely to be true, to an outright rejection of the principle, stating that it may even have done more harm than good in science. In short, people's understanding of Occam's razor and simplicity, its usefulness for science, and what it actually says about the world or about our way of making sense of it, varies greatly.

In the light of the range of representations and opinions of Occam's razor, and the variations in which it is discussed in the different contexts and disciplines, I will explore what possible usefulness it still has as a normative philosophical principle. If Occam's razor is to retain its place as an authoritative "baloney detector", then it must have some agreement with how scientists themselves understand their work. As popular science is in many ways the public face of science, the way it represents science should at least not be too far removed from the practice of it. In this context, Occam's razor, despite its intuitive appeal, may not be so useful for popular science after all.

THE CONSTRUCTION OF INSTRUMENTS FOR MEASURING UNEMPLOYMENT

Peter Rodenburg
University of Amsterdam

This paper provides an analysis of how economists and statisticians built (or have build) measuring instruments for various concept of unemployment, and how this process deviates from the normative, dominant theory of measurement in the philosophy of science, the Representational Theory of Measurement.

The Representational Theory of Measurement is the dominant theory of measurement in the philosophy of science. This theory, which decent from Logical Positivist' thinking, employs a set-theoretical approach to measurement, and defines measurement as bringing about a non-degenerative isomorphic (strict one-to-one) mapping of a set of empirical relations on to a set of numerical relations. However, due to its strict formal and normative nature, this theory has very little guidance to offer to practitioners in the field of how to establish such an isomorphism and so to do sound measurement in practice. In addition to that, does the Representational Theory of Measurement fails to provide a satisfactory account of measurement errors.

In this paper I will investigate how scientists and statisticians have constructed measuring devices in practice by analyzing which ways they used to bring about (and judge) the isomorphic mappings embedded in the design and construction of measuring instruments, and which resources – not incorporated in the Representational Theory of Measurement – are needed to get the measuring devices to work. In order to do so I have conducted a number of case studies in economics, where scientist tried to measure particular concepts of unemployment, such as unemployment in the Netherlands from 1900-1940, cyclical unemployment and the 'non-accelerating inflation rate of unemployment'.

Based on these case studies the paper argues that the requirement of an isomorphism is in fact an idealisation and, in practice, is replaced by a representation of an invariant-as-possible relationship between the phenomenon and numbers. A variety of representations of invariant relations can fulfil this purpose: causal relations, correlations, regressions, stable mechanisms, diagrams, models, standardized quantitative rules and so on. The paper argues further that classification (and division) – which turned out to play an important role in concept formation – is not just a pre-requirement for measurement, but is in fact a rudimentary form of measurement. Finally, the paper provides some suggestions of how a positive theory of measurement and a theory of measurement errors might be developed, based on Bogen and Woodward's (1988) phenomenon-data distinction.

WHAT DO WE ACTUALLY LEARN FROM VIRTUAL WORLDS? THE LIMITS OF COMPUTER EXPERIMENTS AND SIMULATIONS IN ASTROPHYSICS AND COSMOLOGY.

Stéphanie Ruphy
Université de Provence

Computer simulations have become a major tool of investigation to learn about real-world systems for which data are inexistent or very sparse. It is especially the case in astrophysics and cosmology, where computer simulations function as experimental tools by producing data used to test specific hypotheses concerning past or distant events and processes we have no direct access to. This widespread use of computer simulations in scientific practice today raises two related epistemological issues. First, how reliable is the knowledge produced by a simulation, knowing that a simulation does not simply inherit the epistemic credentials of its underlying theories? Second, to what extent can a simulation function as a virtual experiment?

Drawing on my past practice of model building in astrophysics (Ruphy 1996, Ruphy et al. 1996, 1997), I will first put to the fore features of computer simulations of complex real-world phenomena that directly bear on the issue of their epistemic credentials, but which have not yet received proper philosophical attention. I will explain how, what I call the « path-dependency » and the plasticity of a simulation, undermine a realistic take on the results it produces. I'll show in particular how these features account for the embarrassing epistemological situation created by the existence of a persistent plurality of incompatible but equally empirically successful modelings of a given phenomenon. I'll also explain why path-dependency and plasticity support a non-realistic interpretation of the stability and empirical success of a model or simulation when new data come in. To back up these claims, I'll present in some detail two case studies, one in cosmology and one in galactic astrophysics.

In light of the previous analysis, I will then critically examine the use of simulations as virtual experiments. I will show in particular that computer simulations can only teach us something about plausible worlds (i.e. world pictures consistent both with the observations at hand and with our current theoretical knowledge), but there is no good reason to believe that they teach us something about the actual world. Otherwise put, computer simulations can provide us only with simplified and stylized versions of what possibly is, and not of what actually is. So that only at the price of confusing plausible real-world systems and actual real-world systems can cosmologists talk of experimental test in the same sense as physicists do, that is, as a mean to learn something about our world. I conclude by emphasizing that a close attention to the practice of computer simulations calls for epistemological prudence and for a reassessment of the epistemic goals actually achieved by computer simulations of complex real-world phenomena.

Ruphy, Stéphanie (1996), "Contribution à l'étude de la distribution spatiale des étoiles du disque de la Galaxie à l'aide des données DENIS", PhD Dissertation, Paris VI / Paris Observatory.
Ruphy, Stéphanie et al. (1996), "New Determination of the Disc Scale Length and the Radial Cutoff in the Anticenter with DENIS Data.", Astronomy and Astrophysics, 313, L21-L24.
Ruphy, Stéphanie et al. (1997), "Stellar Populations and Inhomogeneities of the Galactic Plane from DENIS Star Counts." Astronomy and Astrophysics, 326: 597-607.

EVIDENCE-BASED PHILOSOPHY OF SCIENCE

Joachim Schummer
Australian National University

By developing ideas of how scientific practice could be or ought to be, speculative philosophy of science has always been running the risk of loosing contact to the actual scientific practice. Even if speculative ideas are supported by individual historical case studies, it remains unclear if such ideas have any descriptive meaning regarding the current scientific practice or other historical cases. This paper starts with discussing methodological issues that any philosophical approach to scientific practice is confronted with. With reference to Max Weber's methodology, I argue that speculative philosophy of science, rather than describing scientific practice, provides useful conceptual and category frameworks for studying scientific practice. However, because of the sheer size of contemporary science, which produces several million papers per year, any such study also needs to be informed by empirical and statistical methodology if it aims at descriptive results of general significance. Evidence-based philosophy of science combines both approach by employing speculative philosophy as a conceptual framework for conducting empirical studies.
The second part of this paper presents some results of evidence-based philosophy of science from a series of earlier studies on the methodology of synthetic chemistry [1]. Synthetic chemistry is particularly interesting because, owing to its traditional focus on the practice of synthesis, it has largely escaped the attention of speculative philosophy of science, despite being the model of many flourishing disciplines, like synthetic biology and materials science. I will demonstrate that by using the results of speculative philosophy as a framework for conducting empirical studies of science, we can develop a much richer and more accurate picture of the methodologies in scientific practice.

[1] Schummer: J.: "Scientometric Studies on Chemistry I: The Exponential Growth of Chemical Substances, 1800-1995", Scientometrics, 39 (1997), 107-123; "Scientometric Studies on Chemistry II: Aims and Methods of Producing new Chemical Substances", Scientometrics, 39 (1997), 125-140; "Challenging Standard Distinctions between Science and Technology: The Case of Preparative Chemistry", Hyle, 3 (1997), 81-94; "The Impact of Instrumentation on Chemical Species Identity", in: P. Morris (ed.): From Classical to Modern Chemistry: The Instrumental Revolution, Cambridge: Royal Society of Chemistry, 2002, pp. 188-211; "Why do Chemists Perform Experiments?", in: D. Sobczynska, P. Zeidler & E. Zielonacka-Lis (eds.), Chemistry in the Philosophical Melting Pot, Frankfurt: Peter Lang, 2004, pp. 395-410.

RELIABILITY, VALIDITY AND THE EXPERIMENTAL PROCESS: A NEUROBIOLOGICAL CASE STUDY

Jacqueline Sullivan
University of Pittsburgh

Reliability has traditionally been characterized by philosophers of science as a virtue of various aspects of experimentation including scientific methods and techniques, experimental arrangements, data and knowledge claims. I develop an account of reliability that restricts reliability ascriptions to processes involved in the production of scientific data. On my account, a data production process is reliable just so long as it results in statistically analyzable data that can be used as evidence for knowledge claims about effects produced in the laboratory. I use a case study from the neurobiology of learning and memory to show that if an investigator wants a data production process to be reliable, measures are taken to build such reliability directly into an experimental design and its adjoining protocol. In this way, the design and protocol can be understood to specify an idealized process type. Subsequently, when that process type is implemented repeatedly in the laboratory, steps are taken to ensure that each individual instantiation of the process type exhibits its fundamental and idealized features. In developing my account of reliability, I appeal to Goldman's (1979, 1986, 1994) process reliabilism as well as Woodward's (2000) and Mayo's (1996, 2000) accounts of the reliability of experiment. I identify what I take to be problems for applying Woodward's and Mayo's accounts of reliability to actual cases of experimentation in the biological sciences and show that my account overcomes such problems.

While the reliability of experimental processes is a primary goal of an investigator, achieving reliability often comes only at the cost of sacrificing the achievement of another desirable goal of experimentation namely, the validity of interpretive claims made on the basis of data obtained in the laboratory. I define validity in accordance with the ordinary language definition, as a feature that an interpretive claim has just in case that claim has a sound basis and is appropriate given the circumstances or context to which it is applied. I use the neurobiological study of learning in order to illustrate how an express commitment to achieving the goal of reliability has resulted in the invalidity of mechanistic claims about learning emanating from this area of research. I provide a set of guidelines geared to function to maintain the reliability and increase the validity of those neurobiological experiments under consideration. The proposals that I make rely on methodological tools that have been proposed historically by experimental psychologists and more recently, by cognitive psychologists. I claim that given the fact that validity has different parameters, any solutions must proceed in a piecemeal fashion. I arrive at the conclusion that at best we can achieve some middle-ground in terms of guaranteeing simultaneously the reliability of data production and any given parameter of the validity of interpretation. This coincides with what Galison (1987), Hacking (1983; 1991) and Cartwright (1983; 1999) have claimed about other laboratory and non-laboratory sciences.

SCIENTIFIC PRACTICE IN THE CONTEXT OF LITERARY PARADIGMS: NEW APPROACHES TO LITERATURE AND SCIENCE

Priya Venkatesan
Dartmouth Medical School

I have been intensively involved in laboratory investigation in a molecular biology laboratory for the past two years at Dartmouth Medical School. This engagement in science is not by itself unusual, except for the qualification that I am a literary theorist and have a doctorate in Literature. I undertook this endeavor with the objective of attempting to understand the nature of scientific practice and how scientific exploration differs among scientists and humanists such as myself. For this paper for the first biennial conference of the Society for Philosophy of Science in Practice, I will show how pragmatic explanations of how science is conducted can be reconciled with the rational and positivist philosophies of science that seemed to have guided the scientific method in the laboratory. My explorations begin with, but will not be limited to, the theoretical basis offered by the actor-network theory and sociological explanations of scientific practice, and the practical considerations offered by my own professional experience in the laboratory.

My scientific research concerns how genes are transcribed; the genes that I have been dealing with are the ones that control for expression of proteins that are involved in red blood cell production. I have had previous experience in the laboratory prior to my current one, however, only on a very limited basis. From this particular project, I gained enormous insight into how science is conducted in terms of how experiments are designed and executed, and how data is interpreted and disseminated. The models that I have developed in assessing these experimental elaborations are the subject of this paper. These models assist in understanding the nature of problem-solving in science, how methodologies in the laboratory are constructed and assimilated, and how scientific explanations are arrived at. These assessments are unique in that they are arrived at from the perspective of a literary theorist. Literary theory and postmodernism essentially guide the spirit of my reflections. Through these insights, I hope to mitigate many of the dichotomies that characterize interpretations of what scientific knowledge consists of.

UNDERSTANDING THEORIES IN PRACTICE: REPRESENTATIONAL AND COMPUTATIONAL ASPECTS

Marion Vorms
IHPST

In this paper, I construe scientific understanding not only as understanding the phenomena by means of some theoretical material (theory, law or model), but more fundamentally as understanding the theoretical material itself that is supposed to explain the phenomena. De Regt and Dieks (2005) emphasise the contextual aspects of the intelligibility of theories, showing that it depends on their "virtues", on the historical standards of intelligibility, and on the particular "skills"of their users. My paper aims at continuing this proposal, first by giving a more precise definition of one's understanding of a theory and then by emphasising the importance, for this issue, of the particular formats in which a theory is expressed and hence grasped by its users. To defend this, I take the example of the versions of classical mechanics and the various formats of representation of its main principles and models.

What does "understanding a theory" mean? At first sight, we could say that it amounts to having a clear view of the logical relations between its core principles and theorems. This kind of understanding, though global, is quite abstract: one can understand the logical structure of a theory without being able to connect it to the phenomena. Moreover, this definition depends on how one construes the structure of theories: it will vary according to whether one defines theories as logical sets of statements with interpretative rules (following the "syntactic conception" of theories) or as families of models ("semantic conception"). I thus suggest that there is another sense of "understanding a theory" that itself has two aspects. To understand a theory, one has to understand both what the theory says or means and how it works; in other words, one has to understand it as representing the phenomena (representational aspect) and to be able to manipulate it and make it fit the phenomena (computational aspect).

I claim that these are essentially contextual and practical matters, and that the particular format in which the theoretical content is displayed is crucial to them. Following Humphreys' proposal (2004), I claim that one never accesses to a theory as a whole. Be it a set of statements or a class of models, in practice, it is always displayed in some particular equations, statements, images, graphs, diagrams. Humphreys' proposal of the notion of "template" to complement the classical "units of analysis" of science, like theories and models, may be a good candidate to study the relationship between the representational and computational aspects of understanding: a template is a "concrete piece of syntax" (most of the time an equation, but I suggest that Humphreys' claim could be extended to other formats) that has both a representational and computational function. With the example of classical mechanics, I show how these two functions are interrelated and, as Humphreys suggests, are sometimes in tension with each other. Adressing these issues by focusing on the particular formats that are dealt with in practice may enlight this problematic relationship.

de Regt, Henk, and Dennis Dieks. (2005)"A Contextual Approach to Scientific Understanding." Synthese 144: 137-70.
Humphreys, Paul. (2004).Extending Ourselves: Computational Science, Empiricism, and Scientific Method: Oxford University Press.

HOW VALUES INFLUENCE THE OUTCOME OF RISK ASSESMENTS – THE CASE OF TRANS FATTY ACIDS

Birgitte Wandall
Royal Institute of Technology

Abstract: Scientific evidence plays an important role as a basis for health risk assessments. When a potential hazard is assessed it is done on basis of available evidence, which may consist of experimental studies on animals or of epidemiological data (or both).

When scientific evidence is used in this way, it is often assumed that the outcome of the risk assessment will be a value-free assessment of facts. On this view, if different risk assessors reach diverging conclusions in an assessment of a potential hazard, the difference in conclusion must be due to either a mistake on the part of one of the risk assessors, or to one of them not having access to all relevant facts.

In this paper, I compare two assessments of the influence of trans fatty acids on human health made by Danish Nutrition Council (1)and by the European Food Safety Authority (2), respectively. The two assessements were made at roughly the same time and the risk assessors had access to the same evidence regarding the effects of trans fatty acids. In spite of this, there was considerable disagreement in conclusions and recommendations for risk reduction; one expert group recommended a severe reduction in the content of trans fatty acids allowed in foodstuffs for human consumption, while the other made no recommendations at all.

Why did the conclusions differ so much? To find an answer to that question, I have compared the two reports, looking for differences in what evidence was included and in how that evidence was interpreted. Surprisingly, I found that the two expert groups based their opinions on almost the same set of studies, and that they agreed very much in their interpretations of that evidence.

It turned out that the differences in conclusions were due mainly to differences in what was perceived as an appropriate level of precaution when dealing with uncertainty and lack of data. It seemed that the Danish Nutrition Council was quite concerned with the risk of making a type II error (false negative): It was explicitly stated that a justified suspicion of adverse effects sufficed for a recommendation of risk reduction measures. The European Food Safety Authority on the other hand seemed much more wary of committing a type I error (false positive), even if this was not explicitly stated.

The lessons that may be learned from this example are twofold:

(a) What seems to be a disagreement on an empirical fact (“do trans fatty acids have negative influence on human health”), may well turn out to be a disagreement on a much more normative issue (“how much evidence is needed before a recommendation for risk reduction should be given”).

(b) A norm that is well-established in basic science, such as the norm of trying to reduce the risk of type I errors, may not be equally unquestionable when put to use in an applied science, such as risk assessment. While it is not necessarily wrong to prioritise a reduction of type I errors even in risk assessment, it may be useful to consider the rationale behind the norm and its consequences in relation to the outcome of a particular risk assessment.

1. Stender S; Dyerberg J (2003): The influence of trans *fatty acids on health. Fourth edition. Danish Nutrition Council, publ. no. 34. http://www.ernaeringsraadet.dk/pdf/Transfedt_UK_ny.PDF*
2. EFSA (2004) Opinion of the Scientific Panel on Dietetic Products, Nutrition and Allergies on a request from the Commission related to the presence of trans *fatty acids in foods and the effect on human health of the consumption of* trans *fatty acids (Request N° EFSA-Q-2003-022). The EFSA Journal (2004) 81, 1-49. http://www.efsa.eu.int/science/nda/nda_opinions/catindex_en.html*

CRITICAL ANALYSIS OF THE PROCEDURES AND CRITERIA OF THE IARC MONOGAPHS ON THE EVALUATION OF CARCINOGENIC RISKS TO HUMANS

Erik Weber
Ghent University (UGent)

In the Preamble to the IARC Monographs (http://monographs.iarc.fr) we read [1]:

> Through the Monographs programme, IARC seeks to identify the causes of human cancer. (p. 1)

More specifically, the objective of the programme is ...

> ... to prepare, with the help of international Working Groups of experts, and to publish in the form of Monographs, critical reviews and evaluations of evidence on the carcinogenicity of a wide range of human exposures. (p. 2)

The exposures include individual chemicals but also ...

> ... groups of related chemicals, complex mixtures, occupational exposures, physical and biological agents and lifestyle factors. (p. 1)

How is the carcinogenic risk of exposures assessed? The available evidence is divided into epidemiological studies (field experiments with humans), animal experiments and information about mechanisms. The assessment consists of three phases. Only in the third phase, the different types of evidence are combined. In the first phase, each study (no matter what kind it is) is evaluated separately. In the second phase an assessment is made of the strength of the evidence for each group (epidemiological evidence, evidence from experimental animals, evidence from mechanisms).

In my paper, I will analyse the procedures and criteria used by IARC (in the three phases) from the perspective of contemporary philosophical theories of causation. More specifically, I will argue that IARC procedures do not fully take into account the possible evidential role of causal mechanisms. I will propose some changes to the practice of IARC, which would improve the reliability of their results.

[1] Quotations are from the version of January 2006 (the most recent one at this moment).

THE PROBLEM OF BIAS IN SCIENTIFIC RESEARCH

Torsten Wilholt
Bielefeld University

Bias is more and more recognized as a serious problem in many areas of scientific research, especially in private research (e.g. drug testing) and in policy-related areas (e.g. climatology). But how exactly should one describe and define the phenomenon of bias, and characterize it as a shortcoming of the research in question? I will first propose a prima facie plausible characterization of bias in terms of inductive risk. In testing a hypothesis, a lower risk of committing a false positive error can often be traded off against a higher risk of committing a false negative (or vice versa), by altering problem selection, experimental design, data analysis or even one's practices of disseminating and publishing results. Bias can then be regarded as a researcher's failing to be impartial between the two kinds of risk, and allowing her different attitudes with regard to the desirability of a positive or negative result to influence the set-up of the test or even the whole research project.

However, this analysis of bias faces a serious problem. From the times of C. West Churchman on, philosophers of science have again and again argued that there is no non-arbitrary and convincing way to mark out any particular balance between the two types of inductive risk as the correct or "impartial" one. Researchers who test hypotheses will always have to evaluate the consequences of errors in order to make their methodological choices. A researcher testing the toxicity of a food additive will and should strike a different balance between the two kinds of inductive risk than a scientist contributing another experimental probe to the ongoing discussion of some academic hypothesis. Against this background, it might seem that often (viz., in cases that don't involve outright deception), to speak of bias is merely to express one's disagreement with the particular kind of value-judgement concerning the consequences of error that must have been applied in the respective case. In a way, bias-talk would thus be revealed as involving the charge of a moral shortcoming rather than an epistemic one. However, one need not rest with this counter-intuitive conclusion.

The solution, I will argue, is to consider scientific practices (as governed by methodological conventions) as some kind of social institutions. Standards of experimental design, data analysis, and the like, are often highly conventional. Such conventions often imply a certain balance between types of inductive risk and thereby an implicit evaluation of the consequences of error. They can differ from discipline to discipline and even from one type of research institution to another. They are nevertheless not arbitrary, because they serve the social purposes of organized science. Conventional standards in scientific research permit other actors to develop differentiated attitudes of trust towards different kinds of institutionally sanctioned scientific "results". Bias often involves deviation from conventional standards and thereby disrupts this trust. Bias thus comes out as an epistemic inadequacy under the wider perspective of social epistemology.

ATTENTION TO PRACTICE: REPRESENTATION IN MOLECULAR CHEMISTRY

Andrea Woody
University of Washington

Many, perhaps most, contemporary philosophers of science would assert that considerations of scientific practice are relevant, and perhaps even crucial, for philosophical analysis. Yet there is no developed, self-conscious discussion regarding to exactly what notion of practice we should appeal. An adequate response no doubt depends upon both the particular philosophical project at hand and one's conception of philosophy of science generally. The most general aim of this talk is to make explicit both the nature of the practice under investigation and the relation between this practice and general philosophical issues.

My concern is with representational practices within chemistry, in particular mathematical, linguistic, diagrammatic, and material model representations of molecules. I contend the rather obvious point that our only means of understanding, and making intelligible, the diversity of representations in molecular chemistry comes through attention to practice properly described. Less trivial is the task of articulating the right level of description, one that will allow us to grapple with the thoroughly normative nature of the questions we ask in philosophy of science.

From a certain vantage point, all molecular representations, regardless of format, are means of capturing and presenting theoretical claims. A central concern of philosophy of science, arguably the central concern, has been how to determine the justificational status of theories: When should we accept theories? When should we believe them? Are these two questions actually one and the same? How are theories connected to the world? What are the conditions for evidential relevance? I will approach these general questions within the specific context of representational practices in molecular chemistry. I will argue that in this context an epistemic distinction between acceptance and belief is both intelligible and necessary. I will also argue that attention to representation as practice, rather than as artifact abstracted from practice, allows us to articulate certain conditions, involving utility, grounding a notion of rational theory acceptance.

A presupposition of this entire line of reasoning is that we can usefully identify theoretical claims with the specific representational artifacts used to communicate them. To presuppose this, however, raises questions that fall outside traditional stomping grounds for philosophy of science (but well within the territory of analytic philosophy). These questions concern the ways in which theoretical representations are used by individuals and especially communities to enhance inferential capacities and make them more robust. In essence, theories are no longer primarily, or merely, descriptive of aspects of the world but are tools used to reason about and engage with aspects of the world. That theories so function is obvious; how they are able to perform this function is not at all obvious. My discussion of molecular chemistry aims to demonstrate that grasping this functionality requires attention to particular characteristics of particular practices.

WHAT KNOWERS KNOW WELL: STANDPOINT THEORY AS A RATIONALE FOR COLLABORATIVE RESEARCH PRACTICE IN ARCHAEOLOGY

Alison Wylie
University of Washington

Standpoint theory, I argue, is best understood as a framework for epistemic analysis that focuses attention on the social conditions—the composition and dynamics of epistemic communities—by which knowledge production and authorization can be systematically skewed: it is a theory of epistemic injustice (in Fricker's sense) and a concomitant theory of how we might improve epistemic practice given a robust appreciation of epistemic advantage that may accrue to those who are otherwise marginalized. So conceived, standpoint theory need not assume an essentialist, or falsely universalizing, conception of the social categories or collectivities—the social kinds—in terms of which standpoints are characterized, nor need it attribute to subdominant standpoints any categorical or automatic epistemic privilege. All it requires is that contingent lines of social differentiation be robust enough to make a systematic difference to what epistemic agents are likely to know, or know well. When such conditions obtain, standpoint theory is a useful framework for understanding consequential patterns of epistemic exclusion or marginalization, and pivotal shifts in understanding that can arise when insights from marginal standpoints throw into relief the partiality of a dominant world view.

In this paper I draw on the resources of standpoint theory, so understood, to make sense of and to articulate a philosophical rationale for various forms of community based collaborative research that have been taking shape in, and that have been the focus of active methodological debate within, a range of social sciences. I focus on an unlikely site for CBPR practice—archaeological research undertaken in engagement with descendant communities—and compare the principles guiding collaborative practice in these contexts with those now well articulated by feminist advocates of participatory action research, by activist-scholars involved in community based ecological research, and in the sharply acrimonious debate about participatory development. While the motivation for these forms of practice is, appropriately, a set of commitments to social justice, its justification is as much epistemic as normative. Standpoint theory provides a framework for explaining how it is that research practice can be substantially enriched, empirically and conceptually, by extra-disciplinary collaboration. At the same time, the examples I consider put epistemically consequential pressure on the ideals of objectivity that underpin standpoint theory and some standard standpoint-inspired guidelines for doing (social) science.

VALUES IN ACTION: THE HWANG CASE OF STEM-CELL RESEARCH FRAUD

Sang-Wook YI
Hanyang University

Scientists usually love to describe science as value-neutral, and Korean scientists are no exceptions. The recent scientific fraud case of Dr Hwang's stem-cell research team however raises a number of illuminating issues in science and value. In this paper, I discuss the Hwang Case and draw a few implications.

First of all, I point out that the very ideal of value-neutral scientific research is a contradiction. Scientists have no choice but to make epistemic value-judgments all the time. They have to evaluate routinely the reliability of their experimental equipments, of their analysis methods, of their computer algorithms and so on. Also during their theory-choice, scientists necessarily rely on a number of epistemic values such as simplicity, fruitfulness or explanatory power, and assess the relative merits of competing theories against another. In short, scientific research is a collection of thoroughly value-laden activities.

Dr. Hwang always made it clear that his research is highly value-laden; it is for the welfare of people, or for the glory of Korean science (in consistent with his famous claim that scientists have nationality, though science itself does not). Korean scientists, who were mostly silent about Dr Hwang's value-laden claims when they were convenient for them, are now claiming that the very value-laden nature of Hwang's research was the source of the problem. Only if he and his team had done science properly (that is, in the value-neutral way), the shameful turmoil resulted from Hwang's scandal wouldn't have happened. They also extend their claims to the defects of government-driven science policy. Ultimately they are asking for more freedom in utilizing public fund for their 'pure' research. Interestingly, the civil opponents of Hwang's research also tends to think that the value-laden nature of Hwang's research such as 'impure' connection with influential politicians was the essence of the problem, although they are asking for more public participation in the decision making process of science and technology policy.

I argue that the Hwang case vividly shows how the intricacies of scientific research are highly value-laden in multi-dimensional ways; epistemologically, socially and institutionally. The problematic part of the Hwang case is not that he and his team did value-laden activities which are forbidden to scientists with integrity, but that their research practices and behaviors diverge significantly from their colleagues' value-laden, but well regulated research. In fact, neglecting the value-laden nature of scientific research has hindered tackling the problem of scientific fraud in South Korea. The parallel between the Baltimore case and the Hwang case is striking here.

My broader conclusion is the following. An effective way to guard science from undue interferences or scientific fraud is to appreciate the fact that scientific research should be executed within the socially acceptable boundaries. The value-laden nature of scientific research is nothing to fear but to embrace.

SOCIAL MECHANISMS AND THE ILLUSION OF DEPTH OF UNDERSTANDING

Petri Ylikoski
University of Helsinki

The idea of mechanistic explanation has become increasingly popular in the philosophy of the social sciences. The most visible advocates of mechanistic perspective have been social scientists inspired by rational choice theory (RCT) (Elster 1989, Hedström & Swedberg 1998). This paper will critically evaluate this approach from the point of view of explanatory practice. I argue that the highly abstract RCT accounts of social mechanisms have a tendency to produce a false sense of understanding.
The illusion of depth of understanding (IDU) refers to the possibility that a person overestimate the detail, coherence, and depth of her understanding. I have earlier (Ylikoski 2007) argued theoretically that the IDU is not limited to everyday reasoning and that it is also a serious possibility in scientific cognition. This paper argues for the same conclusion by the way of case study. I argue that certain problematic features of RCT explanations make them especially prone to produce the IDU in social scientists. These features are not intrinsic to all RCT explanations, but they have an important role in current explanatory practices. The first problematic feature is the highly idealized and abstract nature of RCT explanations. This is not a big problem when the intended explananda are of the same abstract kind. However, when the explananda are concrete historical events the situation is different. Judging the relevance and sufficiency of the explanatory mechanism is extremely difficult in these circumstances. It is easy to fool oneself to think that one has captured “the essence of the phenomenon”. This illusion is further enhanced by the common practice of “stylizing” of the facts to be explained. In practice, this means that the intended explanandum is never characterized independently of the suggested explanation. This makes it difficult to see the challenges the concrete facts could pose for the suggested RCT explanations and the theorist ends up working with “theoretically interesting” cases that happen to fit to her explanatory resources. The second problematic feature is the lack of explicit standards for evaluating explanations. The RCT provides a malleable store of mechanisms that can be quite easily fitted to abstract characterizations of concrete situations. This creates a temptation for ad hoc mechanistic storytelling. Quite often these sketchy RCT narratives create a strong sense of understanding in people proposing them. As the standards of explanatory assessment are not made explicit (apart from the requirement that one should prove an explanatory mechanism), this feeling of understanding is often taken as sufficient criterion for the acceptance of the proposed explanation. Because the social mechanisms movement treats explanation as the central aim of social science, the explanatory success creates a sense of accomplishment that makes the checking of empirical details lose its urgency. As a consequence, the factual presuppositions of the explanatory account are often not properly checked. This lack of empirical challenge contributes to the persistence of the IDU in RCT theorists.

Elster, Jon 1989, Nuts and Bolts for the Social Sciences, Cambridge University Press.
Hedström, Peter & Swedberg, Richard (eds.) 1998, Social mechanisms: An analytic approach to social theory, Cambridge University Press.
Ylikoski, Petri 2007, 'The illusion of depth of understanding in science', forthcoming in Scientific Understanding: Philosophical Perspectives (De Regt, Sabinelli & Eigner eds.), Pittsburgh University Press.

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