Abstracts are published alphabetically by title: first the Plenary Lectures, and then the Concurrent Sessions.

An author index has also been provided below.

Author Index

Plenary Lectures

Critiquing the Foundations of Economics, and Why it Matters (Plenary Lecture 1)

Julie Nelson
University of Massachusetts Boston
(Session chair: Joe Rouse, Wesleyan University)

We economists rarely delve into questions of epistemology or methodology: The assumption that economic life is machine-like, and so best studied using quantitative methods that imitate Newtonian physics, is rarely questioned. This talk will, first, briefly explore the (heavily gendered) historical and psychological roots of this persistent belief. The negative consequences of unreflective approaches to economics and to gender will be illustrated two ways: first, via a meta-analysis of recent empirical behavioral economics research on "gender differences in preferences," and second, through a discussion of how these approaches damage our ability to think in ethical terms about economic matters.

Protocols and Potentiality: From Biological Practice to Scientific Metaphysics (Plenary Lecture 2)

Alan Love
University of Minnesota
(Session chair: Sabina Leonelli, University of Exeter)

Traditional debates about scientific realism revolve around theories. Realists argue that there is no other good explanation for the predictive success of our best scientific theories. Anti-realists argue that our best scientific theories have been overturned in the past, which implies that present theories will eventually suffer the same fate. Although a variety of arguments and counterarguments are available within this dialectic, what would happen if we shifted attention from theory to practice? Is there a way to talk about the success of practices that is not parasitic on the success of theories? If so, does it escape worries from the history of science and therefore provide a stable platform for metaphysical inference? Following the seminal insight of Ian Hacking that manipulability is an important indicator for realism, I argue that a practice-centered approach to realism can be developed and extended apart from considerations of theoretical entities. Using the case of stem cell potentiality, I focus on standardized experimental protocols and efforts by researchers to optimize them (i.e., increase their efficiency or yield). Standardization is a marker that the protocol achieves a genuine outcome (rather than an artifact) and the pursuit of optimization is otherwise inexplicable. This is especially evident when new protocols are proposed and fail (e.g., "stimulus-triggered acquisition of pluripotency"). Additionally, different kinds of experimental protocols that achieve the same type of outcome vary in their precision. When combined with robustness, the optimization and variable precision of experimental protocols for stem cells permit a delimited range of metaphysical inferences about types of potentiality. In closing, I discuss the contours of and prospects for a research program that moves from biological practice to scientific metaphysics, as well as similarities and differences between it and extant alternatives.

Format and Function: Representational Practices in Chemical Contexts (Plenary Lecture 3)

Andrea Woody
University of Washington
(Session chair: Rachel Ankeny, University of Adelaide)

One aspect of the turn to practice in philosophy of science is paying greater attention to the details of the embodied practices of scientists. In this talk, I consider certain representational practices in chemistry as a means of highlighting ways in which the visual culture of chemical representation influences the reasoning employed in both explanatory and evidential contexts. In particular, I will consider some ways in which communities of chemists tailor their representational systems to distinctive epistemic aims and support certain methodologies by coupling representational choices to perceptual capacities.

The Historical Epistemology of Evidence-Based Medicine (Plenary Lecture 4)

Miriam Solomon
Temple University
(Session chair: Hanne Andersen, University of Copenhagen)

Since the early 1990s, evidence-based medicine has come to be a model of objectivity in medical research and practice. This paper explores how evidence-based medicine superseded other accounts of objectivity in medicine, and how the recent developments of translational medicine, personalized medicine, and precision medicine are responses to the shortfalls of evidence-based medicine.

Concurrent Sessions

Abstraction, Idealization, and the Ontic View of Explanation

Marta Halina
University of Cambridge
United Kingdom

Over the last two decades, philosophers have come to recognize the prevalence and importance of mechanistic explanation in science. Indeed, one of the great strengths of the mechanist account of explanation has been its ability to elucidate a wide range of scientific practices—practices that were difficult to account for under traditional views of explanation, such as the DN model (Bechtel and Abrahamsen 2005). Recently, however, mechanistic explanation has come under criticism. The criticism holds that this account of explanation is actually at odds with the common practice of constructing abstract and idealized models. One particular version of mechanistic explanation—that advanced by philosophers such as Carl Craver—is taken to be especially problematic in this regard because it appears to endorse a representational ideal of completeness (Levy and Bechtel 2013; Batterman and Rice 2014; Chirimuuta 2014; Levy 2014; Love and Nathan 2015). According to the critics, this ideal conflicts with the practices of abstraction and idealization because it calls for maximizing the completeness and accuracy of an explanatory model, whereas abstraction and idealization involve the removal of such content. If the critics are right, this poses a serious problem for mechanistic explanation because all of those involved in the discussion agree that a good philosophical account of explanation should be descriptively adequate.

In this paper, I argue that the above criticism conflates various senses of the term "explanation" and once these senses are distinguished, one can see that Craver's preferred account of mechanistic explanation—the ontic view—has no problems accounting for the practices of abstraction and idealization. Indeed, it is the rejection of the ontic view that leads to problems. The problems identified by the critics stem from a failure to recognize the ontic commitments that one must make when constructing and evaluating explanatory models. I conclude by responding to the objection that ontic explanations are merely ways of talking about truth or causes. I argue that neither truth nor causes can do the work that ontic explanations do in accounting for the practices and norms of science.

Batterman, R. W., & Rice, C. C. (2014). Minimal model explanations. Philosophy of Science, 81(3): 349-376.
Bechtel, W., & Abrahamsen, A. (2005). Explanation: A mechanist alternative. Studies in History and Philosophy of Biological and Biomedical Sciences, 36: 421-41.
Chirimuuta, M. (2014). Minimal models and canonical neural computations: The distinctness of computational explanation in neuroscience. Synthese, 191: 127-153.
Levy, A. (2014). What was Hodgkin and Huxley's achievement? The British Journal for the Philosophy of Science, 65(3): 469-492.
Levy, A., & Bechtel, W. (2013). Abstraction and the organization of mechanisms. Philosophy of Science, 80(2): 241-261.
Love, A. C., & Nathan, M. J. (2015). The idealization of causation in mechanistic explanation. Philosophy of Science.

Animal Models of Pain and the Puzzle of Similarity

Nina Atanasova
The University of Toledo
United States

The Puzzle of Similarity is a problem for animal experimentation in general but it is especially troublesome in neurobiological experiments involving animal models of pain. It can be stated as follows: If animal models (of pain) are valid, they are morally impermissible and if they are morally permissible, they are useless. Either way, experiments involving animal models (of pain) should be abolished.

The purpose of this paper is to explore the possibility to solve this puzzle. Animal models are a fundamental tool of experimental neurobiology. They are commonly used in experiments involving invasive interventions impermissible for human subjects. Nevertheless, some question the moral justification of this practice. For example, Regan and Singer hold that because animals are relevantly similar to humans they should not be subjected to suffering which animal experimentation causes. LaFollette and Shanks, on the other hand, argue against animal experimentation because of its epistemological failures. On their account, animals are too dissimilar to humans to serve as valid models of human conditions.

The complication with animal models of pain is that they need to exhibit some similarity to the human experience of pain. However, if they did this would confirm Regan's and Singer's worry that this kind of experiment causes animals to suffer. Defenders of animal experimentation may agree that animal models of pain subject animals to suffering but argue that the benefits from this practice are greater than the harm it produces. This position is reflected in the 3R policy, according to which experimental animals should be replaced with phylogenetically lower and presumably less sentient species whenever possible. However, adopting this position leaves the door open for questionable human experimentation in cases where the benefits could override the harm caused.

Defenders of animal experimentation may fare better if they can show that the animals involved in the study of pain are not capable of experiencing pain like humans. However, the animal experimentation supporter will have to find a way to show that animal models of pain are still valid as representations of human pain although the animals involved are relevantly dissimilar to the humans they represent.

A way to approach this problem is by adopting Bolker's notion of animal models as surrogate models as opposed to exemplary models. Exemplary models represent by example. They include animals as representatives of a broader class to which they belong, whereas surrogate models represent by substitution. Surrogate models are designed to study specific phenomena. The animals in these models serve as proxies for other species, most often human.

Assuming that animal models of pain are surrogate models, one could argue that the experimental system as a whole, rather than the organism it contains, models a given human condition, in this case pain. Therefore, the system will have to be evaluated for its validity as a representation of the studied condition rather than the animals for their similarity to humans. Thus, animal models of pain may be valid even though the animals involved in them do not experience pain like humans.

Articulating Organic Chemistry into Structural Biochemistry

William Goodwin
University of South Florida
United States

This paper will focus on Linus Pauling's contributions to the foundations of structural biochemistry. Not only was Pauling an early advocate for, and defender of, the polypeptide theory of proteins, but he also played a crucial role in characterizing both the content and usefulness of the notion of structure as it is understood in this discipline. It was Pauling who first established that bottom up structural modeling could provide useful insights into what is now know as the secondary structure of proteins. This approach was successfully extended, though not by Pauling, to the structure of DNA as well. Furthermore, Pauling articulated a broader research program where biological specificity generally was to be explained by the sorts of non-bonding inter- and intra-molecular interactions that he had identified as the central players in structural biochemistry. This vision, if not the details that Pauling focused on, is still central to the field today.

Pauling's background as a theoretically-minded organic chemist proved crucial to his successes in structural biochemistry. It was the articulation, in the Kuhnian sense, of the theory of organic chemistry to apply to novel biological questions that supplied the concepts and techniques that allowed for progress in structural biochemistry. For instance, in arguing for the polypeptide theory of proteins, Pauling subjected the competing proposed protein structures to the sort of energetic stability analysis common in organic chemistry, thereby finding points of contact between live questions in biochemistry and accessible empirical facts. Additionally, his most famous work as a chemist had found ways to integrate the newly developed theory of quantum mechanics with traditional theories of chemical bonding. This resulted in a refined notion of chemical bonding that could, to a certain extent, accommodate the delocalization of electrons required by quantum mechanics. Though not the only way to make such accommodations, Pauling's theory of resonance proved very successful in organic chemistry. Some of the insights derived from Pauling's theory of resonance supplied the rationale behind both hydrogen bonding and partial double bonds, which were the central theoretical insights in his account of protein secondary structure. It was in part, therefore, Pauling's articulation of the notion of chemical bonding in response to theoretical results from physics that paved the way for his progress in structural biochemistry. Lastly, Pauling's bottom up approach to protein structure depended on his development, using electron scattering and X-ray crystallography of amino acids and simple peptides, of a library of known bond lengths and angles which formed the database from which models of longer polypeptide chains could be plausibly projected. Thus Pauling's work as an organic chemist supplied the conceptual and experimental resources for his successful, and foundational, new approach to biological questions.

Aside from demonstrating the usefulness of some of Kuhn's conceptual apparatus for talking about non-revolutionary progress in science, Pauling's work on structural biochemistry can also function as a model of (one important kind of) new discipline formation. And, contrary to some of Kuhn's later speculations, structural biochemistry seems to have become its own discipline without any sort of incommensurable barrier separating it from its predecessors. Instead, it appears to be a cumulative addition, distinct from its ancestral disciples because of the sources of its questions and the central role of certain sorts of concepts and techniques in supplying answers to those questions.

Autism: A Kantian Imagination-Based Theory

Susan V. H. Castro
Wichita State University
United States

Autism spectrum disorder is traditionally diagnosed by atypical development patterns in sociality, communication, and imagination (DSM-IV criteria IIA-C; DSM-V criteria A1-3). According to the DSM-V diagnostic criteria, severity is determined not simply by the severity of dysfunction within this triad, but by restrictive and repetitive behaviors (RBs) that are prima facie neither constitutive of the triad nor consequences of it (DSM criteria B1-4). The conceptual disconnect between the triad and RBs might lead one to suspect that one's place on the spectrum is determined more by the level of disruption one's RBs impose on neurotypical caregivers and peers than by genuinely patient-centered criteria. Given that diagnosis has very real personal consequences for autistic people, the conceptual coherence and justice of the category A and B criteria are in need of defense. Though diagnostic criteria need not always carry explanatory power, in the case of ASD the criteria cannot be applied without an understanding of what is meant by its non-trivial basic terms, e.g. "persistent deficits in social communication". One cannot avoid interpreting the ASD diagnostic criteria as the framework of a definition of autism, one that ought to have explanatory power. In order to make sense of these DSM-V criteria, then, we need a psychological framework that relates the disparate signs of autism like hand flapping and flat affect in a phenomenologically coherent and articulable understanding of autism—a theory of what autism is and of what it's like to be autistic. The purpose of this paper is to propose an imagination-centered theory of autism based on Immanuel Kant's theory of imagination. According to Kant, imagination in general is a synthesizing faculty that mediates between sense and understanding as well as an ability to intuit what is not present to the senses. Its uses include our spatially formative, temporally associative, and communicatively affinitive production, including sympathy and fantasy. Imagination is thus the faculty of sensory integration, embodied subjectivity, empathy, mindreading, and social cooperation. (Kant need not choose between an embodied theory of autism and a mindblindness theory.) Sensibility and understanding play their parts, granted. Sensibility provides much of the material on which imagination operates, and the demands of understanding set ends for these uses of imagination. Many autistic people have intellectual impairment, and many have hyper- or hyporeactivity to certain stimuli. Autism is not, however, an intellectual or sensory disorder. Kant provides a name and a theory for what kind of thing it is. According to this theory, the distinctive features of autism predictably arise when imagination deviates from the neurotypical norm. Kant's framework is comprehensive enough, yet sufficiently determinate to make sense of how particular idiosyncrasies of imagination could have predictable downstream effects as disparate as echolalia and absence of spontaneous imaginative play. For those who retrospectively recognize imagination as the source of difference or disability in themselves or others, a Kantian imagination-based theory may facilitate understanding, empathy, and cooperation. The theory behind the label matters very much. Theory can itself be therapeutic when it adequately fits our experience.

Between the Empty and the Blind: N. R. Hanson's Historiography of Science

Matthew Lund
Rowan University
United States

In 1960, N.R. Hanson became the founding chair of Indiana University's Graduate Program in the History and Logic of Science, the first program of its kind in the United States. Despite having put the concept of HPS on the institutional map, Hanson's distinctive account of the interdependence between history of science and philosophy of science has been largely forgotten, and often misinterpreted where it is remembered. Now that the many are considering HPS to have been a failed institutional experiment (Shapin and Shaffer, Giere), the time is ripe to revisit Hanson's most mature views on the interrelation of history and philosophy of science.

This paper aims to show that a fruitful and transformative framework for understanding the interrelation of the history and philosophy of science is obtained by uniting three separate, and not wholly harmonious or fully developed, elements of Hanson's philosophy of science: his analysis of the conceptual dynamics of science, his championing of Keynes's interpretation of probability as providing a philosophically respectable means for appraising the evidential support for a theory at some point in history, and his discussions of the significance of the genetic fallacy for philosophical explorations of history.

Acknowledged for introducing the concept of "theory-laden observation", Hanson expressed what could be considered a "higher order" version of the theory-laden observation thesis: historical content can only be made intelligible by being organized and interpreted philosophically. At the same time, philosophy of science would be without content were it not about science and its history. As Hanson expressed it (along with Feigl and Lakatos), "Philosophy of science without history of science is empty; history of science without philosophy of science is blind." Hanson's mature historiography resulted from his interpretation of this aphorism, but his last writings on the subject have been neglected.

A central concern for Hanson was the status of historical evidence in the context of philosophical claims concerning science. While concerned that inferences from the contingent (history) to the necessary (epistemology) could be read as an obvious instances of the genetic fallacy, Hanson nevertheless stressed that historical understanding is not reducible to mere chronologies or neutral descriptions. Instead, history always comes in the form of structured arguments and explanations. As a consequence, the "genetic fallacy" would only properly apply to arguments that take history to be descriptive in the narrowest of senses. In Hanson's final publication on this topic, he took seriously the idea that there can be legitimate genetic arguments, and thought that philosophical scruples concerning the genetic fallacy impoverish much philosophy of science. This paper attempts to show how Hanson's basic approach to the interrelation of history and philosophy of science can be improved, using other elements of Hanson's thought, such that one can define a posteriori conditions of justification. More generally, Hanson's basic approach affords a reformulation of the relation between normative and descriptive accounts of science, one in which normative judgments are corrigible by empirical facts. It is argued that the essential elements of Hanson's view remain legitimate and that extension of his approach is more likely to overcome the contemporary rift between philosophers and historians of science than other historiographic approaches.

Bibliography

Feigl, Herbert. (1970). "Beyond Peaceful Coexistence." In Minnesota Studies in the Philosophy of Science, Vol. V: Historical and Philosophical Perspectives of Science, edited by Roger H. Stuewer, 3-11. Minneapolis: University of Minnesota Press.

Giere, R. (2011). History and Philosophy of Science: Thirty-Five Years Later. Boston Studies in the Philosophy of Science. 263, 59-66.

Goudge, T. A. (1961). The Genetic Fallacy. Synthese : An International Journal for Epistemology, Methodology and Philosophy of Science. 13, 41-48.

Grau, K.T. (1999). "Force and Nature: The Department of History and Philosophy of Science at Indiana University, 1960-1998." Isis 90, S295-S318.

Hanson, N.R. (1962). "The Irrelevance of History of Science to Philosophy of Science." Journal of Philosophy 59, no. 21, 570-586.

Hanson, N.R. (1967). "The Genetic Fallacy Revisited." American Philosophical Quarterly 4, 101-113.

Lakatos, I. (1978). Philosophical Papers I: The Methodology of Scientific Research Programmes. Edited by John Worrall and Gregory Currie. Cambridge: Cambridge University Press.

Shapin S., & Schaffer, S. (2011). Leviathan and the Air-Pump. Hobbes, Boyle, and the Experimental Life. Princeton: Princeton University Press.

Bias and Social Science Experiments

Sharon Crasnow
Norco College
United States

Until the second half of the 20th century experiments were rare in the social sciences. More recently there has been a marked increase in the use of experimental methods in the other social sciences, most notably economics and political science. Rebecca B. Morton and Kenneth C. Williams, authors of a key text for experimental method in political science, outline the key reason for this turn to experimentation: "nonexperimental methods have failed to answer some significant research questions, particularly causal ones…." In this paper I address the question of bias in experimental research, drawing a distinction between value bias—the way values might distort scientific findings—and inferential bias—factors that influence the strength of support for causal conclusions. I argue that while two features of experimental research—random allocation to control and experimental groups and masking—do contribute to stronger causal inference, neither fully eliminates two types of inferential bias: omitted variable bias and selection problems. Both observational research and experimental research strive to approximate ideal experimental design (Mill's Method of Difference). In practice, both fall short in similar ways and require additional assumptions and argument to support the inference to causal claims. Omitted variable bias is not fully eliminated since any random draw could result in an imbalance between the control and experimental group (Worrall 2002; 2007). While econometric techniques can be and are used to address such imbalances, these techniques are challenged from several different quarters. The statistician David A. Freedman has argued that such approaches require assumptions that have uncorroborated empirical content. Angus Deaton in economics has cast doubt on the widespread use of instrumental variables to produce "quasi-experiments" through statistical means. Selection bias can be addressed through various forms of masking of the control and experimental groups. However, questions of external validity—transportability of results—raise concerns about representativeness when the experimental and control groups come from a particularly non-representative population. These factors are present for both experimental and observational research and so undermine the sharp distinction between the two. Experiments are also sometimes claimed to be better than observational research because of their replicability (Duflo and Banerjee 2006). Replicability offers additional checks on bias, robustness checks, and opportunities for building on previous findings. However, at least some observational research appears to be replicable as well. Multiple regression analysis can be repeated using the same data, for example. Moreover, replications with different assumptions or technical models can result in quite different findings and so reveal the sensitivity of findings to such assumptions. I conclude with a claim that the experimental turn in the social sciences suffers from “methodological bias”: the belief that a particular method should dominate a field, perhaps even to the exclusion of others. Both for the reasons offered above and because of negative effects such a methodological bias can have on the training of future social scientists, I conclude with a plea for methodological pluralism.

Big Data and the Evaluation of Experts

Darcy McCusker
University of Washington
United States

The increase in the use of databases to share and use data in the sciences has generated both new sources of knowledge and new potential issues with generating that knowledge. Databases require substantial work to maintain, requiring personnel to manage the data as it comes in and adjust the structure and form of the database in response to new datasets. When researchers use the data in a database for new analyses and research, the researchers need enough information about the data to evaluate its reliability and quality. The purpose of creating a database is to provide a repository of data that can be consistently reused, but the majority of the datasets in these databases will never reach such a stage, precisely because there is not enough information to evaluate their reliability and quality. Much of the work done on databases is completed by data curators, who may have a wide range of experience and knowledge about the datasets contained in the database. Social epistemology may have resources that can help with some of the issues in Big Data. In “Experts: Which One Should You Trust?”, Alvin Goldman provides a way for a novice who has limited knowledge to evaluate the claims of two purported experts. The curators of a database may be in a similar position to the novice in Goldman’s account, so considering the types of evidence he says a novice can use could perhaps inform how curators should proceed. Goldman suggests that a novice can use meta-scores by other experts, credentials, possible biases, and the track record of a given expert to decide whether or not to trust that expert. Similarly, a data curator may be able to use the same sources of evidence to evaluate whether a researcher or research group’s data should be included in a database. Many of these sources of evidence seem intuitively plausible. However, the practicality of Goldman’s source of evidence may create further problems for a curator. When a curator decides whether a data set from a researcher or research group was worth including, they must be able to do so in a timely manner; extensive research on their part should not be required. Currently, to gather evidence for all the types of evidence Goldman suggests, time consuming research would be required. Even if that problem is surmounted, others remain. Meta-scores may be difficult to create, if the data sets come from different fields or subfields with different evaluations of methodology. Credentials are unlikely to be decisive, since the majority of researchers will have roughly the same credentials. Some biases, such as those stemming from funding sources that influence results, may be easier to detect than community-wide biases. If the track record of a researcher or research group was readily available, it would still be unclear what matters in determining the reliability of the data. In order for Goldman’s source of evidence to be useful to curators, many of these problems must be resolved.

Biochemistry Research Programs through the 20th Century Use: A Dual-Column Programmatic Practice to Resolve and Reconstitute the Molecular Economy of a Biological Activity

Alok Srivastava
Tremont Research Institute
United States

Research programs in biochemistry throughout the 20th century have utilized a practice at the level of metal-work—work that determines the organization of work (see Gerson, 2013)—Arthur Kornberg (2000) describes as ‘attempts to resolve and reconstitute biological events.' Starting in the 1910s with the chemical studies of fermentation process in the juices of yeast cells in the early days of intermediary metabolism there is a continuous historical line of such programmatic practice to the recent Nobel Prize winning work of Roger Kornberg which involved reconstituting the activity of RNA polymerase activity in vitro. This practice is somewhat analogous to double-entry bookkeeping practice in business accounting and I call it the 'dual-column program to resolve and reconstitute the molecular economy of a biological process'. The research program runs operations of characterizations and analysis in the two columns: (a) resolving the chemical entities and the chemical activities of the targeted biological activity, and (b) reconstituting the full measure of the biological activity from purified and defined chemical components. Like with financial transactions in double-entry bookkeeping the same set of chemical transformations & entities are characterized in partially separated juices of cells (the 1st column) and in reconstituted mixtures of purified and characterized components (the 2nd column). The work is carried out in both columns in parallel during each phase of the research program and the results from each column inform progress in the other column in an interdependent manner. This dual-column strategy and workflow was articulated in the first research programs of Intermediary Metabolism in the late 19th century and has been in use by modern day biochemists and cell biologists during the era of molecular biology. A key example from mid-century and during the transition from the disciplines of intermediary metabolism to Biochemistry and Molecular biology is the research program of Arthur Kornberg's laboratory and his coworkers' laboratories. Kornberg organized a research program in the 1950s to study the nucleic acid economy of the cell and "how a complex polynucleotide is assembled by the cell?". Over 20 years the laboratories of Kornberg and his coworkers resolved and reconstituted the process of DNA replication by the cell.

My analysis of this metawork practice of biochemistry laboratories will explore the interdependent play of two frameworks. The first framework articulated by the chemical engineer A.D. Little in 1915 observes that the chemist's laboratory in the early 1900s began to be organized around a standardized set of unit operations as and this afforded a generalized capacity to work out chemical processes (N. Rosenberg 1998). The second framework explores how, the style of scientific reasoning characteristic of the chemist's laboratory evolved into that of the biochemist's laboratory. For example, the work under the 'reconstitute' column is dominated by the experimental style and the 'chemist's style of thinking', particularly 'a specific way of knowing through making' (Bensaut-Vincente 2009) i.e. the biochemist's style of explanation by reconstitution of a biological activity from purified chemicals is analogous to the chemist's explanation by synthesis of a natural compound.

Boosts and Nudges Differ Because They are Based on Different Conceptions of Heuristics

Till Grüne-Yanoff
Royal Institute of Technology, Stockholm
Sweden

Previously, we have characterized boosts as a class of behavioral interventions different from nudges (Grüne-Yanoff & Hertwig 2015), and have defended his distinction against the claim (made by e.g. Sunstein 2015, Bar-Gill & Sunstein 2015) that boosts are kinds of nudges. In this paper, I will expand our analysis by arguing that the distinction is based on an implicit distinction in the concepts of "heuristic" proposed by 20th century psychologists and cognitive scientists.

Originally, cognitive scientists used "heuristic" to denote cognitive processes that guide information search and modify problem representation for solving problems that cannot be handled by logic or probability theory (Groner et al. 1983/2014). However, in subsequent applications in economics and engineering, as well as in various subdisciplines of psychology, this conception was refined and sometimes reformulated in a number of different ways. These differential specifications often had substantial implications for policy intervention aiming to change heuristic-mediated behavior.

To take just one (and perhaps the most famous) example, when Amos Tversky and Daniel Kahneman applied the notion of heuristics to decision theory, they emphasized that decision makers typically employed heuristics without being aware of this. Furthermore, they often drew the analogy between the misapplication of cognitive heuristics and visual illusions, thus suggesting that the cognitive processes are “hard- wired”, do not involve conceptual knowledge and are therefore hard to change. This contrasts with the heuristics concept developed by Herbert Simon and Allan Newell, who stressed that behavior-producing heuristics often are concept-mediated and epistemically transparent.

This analysis implies, so I argue, that valid prescriptions for how to design behavioral policy interventions depend on how heuristics are conceptualized. If heuristics are concept-mediated and epistemically transparent, then training (“boosting”) people in the use of novel heuristics or in more effective use of extant heuristics constitutes effective interventions. If however heuristics are not concept-mediated and epistemically opaque, then such training is likely to be futile. Instead, interventions that change the choice environment in such a way that extant heuristics produce “better” behavior (i.e. “nudge” people) might be the preferable intervention tactic here. Consequently, the proposed differentiation between boosts and nudges as two distinct types of policy interventions is supported by an in-depth analysis of the concepts of “heuristic” used in cognitive science and psychology.

The goal of the paper is twofold. In the first place, it provides an analysis of the concepts of heuristics in cognitive science, showing that different conceptions have substantially different policy implications. Secondly, the paper argues that this analysis gives further support for the boost/nudge distinctions as two separate types of behavioral policy interventions.

Chaos Regained: On the Possibility of a New Era of Orbital Dynamics

Isaac Wilhelm
Rutgers University
United States

I explore how the nature, scope, and limits of the knowledge obtained in orbital dynamics—the science concerned with the motions of bodies in the solar system—has changed in the past century. Innovations in the design of spacecraft trajectories, as well as in astronomy, have led to a new hybrid of theory and experiment, and suggest that the kind of knowledge achieved in orbital dynamics today is dramatically different from the knowledge achieved prior to these innovations. Thus, orbital dynamics may have entered a new era.

I begin with some historical background, focusing in particular on what Neugebauer demarcated the ‘Newtonian era’ (the period following Newton’s Principia). That era featured an extremely powerful methodology for constructing successively better theories of the motions of celestial bodies. Idealized theories (of the Moon’s motion, say) were refined by comparing calculations based on them with observations, and using systematic discrepancies to identify forces that made a difference to the motions but that the idealizations had left out. Those forces were incorporated into the idealization and the process was repeated, resulting in ever tighter agreement between theory and observation.

When Poincaré discovered mathematical chaos in 1890, he exposed a shortcoming in this methodology: if the dynamics generated by the gravitational field are chaotic, then the exact trajectories of bodies are infinitely sensitive to initial conditions. In such cases, because the initial conditions cannot be known exactly, there will always be irreducible discrepancies between calculations and observations even if all the relevant forces are accounted for in the idealized theory.

The discovery of the possible existence of empirical chaos—chaotic dynamical systems in the empirical world—unearthed a number of philosophical and evidential problems. Poincaré observed that irreducible discrepancies may render long-term prediction impossible. Hadamard and Duhem observed that such discrepancies may render certain empirical questions “ill-posed” or “meaningless”.

In this presentation, I discuss two interrelated ways that orbital dynamics has adapted to the problems posed by chaos. The first adaptation involves a shift of focus: rather than describe dynamical systems in terms of forces acting on bodies, astronomers now use gravitational fields. The second adaptation came in the form of two new methodologies for conducting empirical research: one in astronomy and one in the design of spacecraft trajectories. Together, these two methodologies may eventually overcome the problems that chaos poses. The astronomy methodology can be used to identify empirical chaotic systems and to measure how 'chaotic' they are. The trajectory methodology can be used to interact directly with chaotic regions of the solar system's gravitational field, exploiting chaos in order to transfer spacecraft (like the Hiten) from one orbit to another while expending almost no fuel.

Because of these adaptations, the kind of knowledge of orbital motion that can be achieved has changed. The development of contemporary orbital dynamics thus provides an excellent case study of how science evolves in response to philosophical and empirical problems. As I show, that evolution suggests that we may be in a new era of orbital dynamics.

Concepts as Forward-looking: Reframing the Question of Referential Stability

Corinne Bloch-Mullins
Marquette University
United States

The question of referential stability of scientific concepts throughout theory change has been taken as central to issues of rationality in theory choice. I suggest that the traditional framing of the question is unsatisfactory, as it leaves out an important part of the story. Concepts, I argue, are forward-looking; they are set up to accommodate a range of yet undiscovered phenomena. Drawing on data from the psychology of categorization and similarity judgments, I suggest a mechanism that gives rise to this property of concepts. I propose that, to accommodate the nature of concepts, the question of referential stability needs to be reformulated.

I begin with a discussion of the question of referential stability, in its traditional form. I then examine how the traditional question would apply to the case study of the concept SYNAPSE, examined throughout theoretical developments in the 20th century. I show that the application of the traditional question to this case study provides us with an unsatisfactory answer, which misses an important aspect of scientific concepts.

I argue that, while the traditional question is motivated by the acknowledgement that concepts may change as science progresses, it still holds to a mistaken assumption, namely, that concepts are static constructs. Thus, a concept whose reference has changed over time is taken as a series of static 'referential snapshots'. It is then the task of the historian and philosopher of science to determine the degree of overlap between these snapshots, and (at least on some accounts) the ways in which these snapshots are interconnected to enable continuity. The apparatus that connects the snapshots is taken, in a sense, as external to the concepts themselves (that is, it is not taken as part of what individuates concepts).

I suggest an alternative view of concepts. I argue that concepts are forward-looking. That is, that they are set up to accommodate a range of yet undiscovered phenomena. I use data from the psychology of categorization and similarity judgments to elucidate the mechanisms that facilitate this aspect of concepts. I argue that these mechanisms are rooted in the taxonomy, or conceptual hierarchy, within which one's concepts are formed. The conceptual hierarchy, and especially the concept's contrast-class (i.e., the class of items that the category is distinguished from, within the broader genus in the taxonomy) both facilitate and constrain the application of an existing concept to newly observed instances. They do so by determining which properties of a novel phenomenon will be taken as relevant for classification under an existing concept, as well as the range of values that these properties may receive. I further propose a concept's contrast-classes are an integral part of what individuates a concept.

Last, I return to the concept SYNAPSE, and show that the approach I recommend provides us with a more nuanced understanding of the continuity of the concept over time, than that provided by the traditional approach to referential stability. I conclude by discussing the implications of my view for questions of continuity of scientific concepts.

The Crocodile’s Snout: the Epistemic Function of Phylogenetics in Paleobiology

Adrian Currie
CSER, Cambridge
United Kingdom

I argue that phylogenetic analysis plays important (and partially epistemic) roles even when we shouldn’t believe they reflect the history of life—as is often the case in paleobiology, where phylogenetic data is restricted to morphology.

In a nutshell, in such circumstances phylogenies perform two functions. First, they play a kind of heuristic role, specifically driving further research into the relevant organismic traits. Second, a legitimately epistemic role: they explore the consequences of our theoretical commitments vis-a-vis the evolutionary and developmental features of traits and evolutionary processes in general. This shows that even when we don’t think we should believe phylogenies—when the ancestral trees such studies produce are most likely false—they still drive epistemic progress.

I will focus on the idea that assigning weight to characters represents hypotheses of the evolutionary lability and coupling of the relevant traits. In reconstructions of crocodilian phylogenies, a distinctive ‘long snout’ morphology is present in three extinct aquatic clades: thalattosuchians, dyrosaurids, and pholidosaurs. Some reconstructions have taken the thalattosuchians to be outliers: their long snout is convergent, as opposed to homologous, with other long-snouted crocodiles. Some analyses go so far as to use that group to root trees. Among other reasons, the similarity in snout shape was taken to be a ‘bad signal’ of relatedness, something which could be ignored, “… because crocodyliforms have demonstrably evolved similar skull shapes numerous times, it was assumed that snout shape is not a reliable phylogenetic character” (Wilburg, 2015). Here, a claim about the evolutionary lability of snout shape in crocodyliforms underlies the decision to assign less weight—or even disregard—that character.

Phylogenetic trees, then, are not only hypotheses about the history of life, but (implicitly) hypotheses about the ontogeny and evolution of the characters themselves. The construction of phylogenies promote two kinds of testing. First, straightforward tests familiar to systematics (jack-knifing, robustness, increased data etc…). Second, investigation into the relevant aspects of the characters themselves.

Phylogenetic analysis helps us explore the consequences of our commitments to hypotheses about characters, and encourage further analysis of such characters. Whether to include snout length as a character in your crocodilian phylogenies depends in part on whether you think snout length is a ‘good’ character—will it be stable over evolutionary time, is it developmentally coupled with other traits, and so forth. And this is true for each character.

This means that each phylogenetic analysis in paleobiology—with anything from 20 to 400 characters—implicitly explores the consequences of a large range of evolutionary and developmental hypotheses about those characters. This drives further research into their nature, and therein, I argue, lies the justification of phylogenetic practice in paleobiology.

David Gooding’s Philosophy of Reconstruction

Laura Georgescu
Ghent University
Belgium

In Experiment and the making of meaning (1990), David Gooding proposes a philosophical conception of scientific research that departs from the received propositional understanding of scientific knowledge and that aims to go beyond the traditional dualisms that long dominated the epistemology of science (e.g., experiment/the world, world/beliefs about the world, theory/experiment, practice/theory). We might say, perhaps somewhat synoptically, that Gooding offers a philosophical interpretation of those aspects of scientific activity that have a productive role to play in the constitution and stabilisation of scientific phenomena while acknowledging that, in doing so, the embodied agent's active role in scientific knowledge making needs to be brought back into philosophical focus—and its role in making the empirical both accessible and accountable needs to be explained. As a way into dealing with such problems, Gooding introduces his notion of 'construals', one of the few aspects of his philosophy of science to have received some recognition.

Here I want to put aside Gooding's construals and focus instead on a part of his philosophy of scientific practices that has not received much attention; nor have the full extent of its implications, especially for the philosophy of experimentation, been studied. That is, Gooding's claim that reconstruction is a productive and necessary feature of scientific life. Much like Nickles (1988), Gooding holds that reconstruction happens whilst moving from the individual note taking in the laboratory to the publicly acceptable scientific products (since scientists do not publish records of their results but linear reconstructions of their achievements). But, Gooding does not stop there. On my interpretation, his claim is much stronger: reconstruction itself is an essential feature of experimentation; it is what makes possible the transition from a pre-articulated individual encounter with the world to an articulated, intelligible one that can then be (via further reconstructions) socially communicated and made acceptable to the scientific community. If reconstruction is how ordered accounts are generated (i.e., how construals are made), and is a necessary part of both 'construing' and, later on, stabilising scientific phenomena, reconstruction is a fundamental part of what makes science possible.

In the talk, I begin by quickly overviewing Gooding's types of reconstruction, and then go on to present what are, on my reading, his arguments for why reconstruction has fundamental roles in science and for what those roles are. I show that a consequence of Gooding's analysis is to assent to the claim that, in a substantial sense, experiments are already reconstructed entities. I then go on to show that, entwined with this philosophical move with respect to experimentation, come revisions in our philosophical understanding of what a scientific object (and/or phenomenon) is, of the relation between theory and experiment, and ultimately about what scientific knowledge is. By bringing Gooding's insights back into philosophical discussion, my overall goal is to show that while scientific research is undeniably prospective, it is also retrospective and reconstructed. The latter seriously undermines any philosophical claim to a 'pure' encounter of the science with 'the world'.

Debating Darwin's Debts to Philosophy

Phillip Honenberger
Consortium for History of Science, Technology and Medicine
United States

The question of Darwin's intellectual debts to the philosophy of science of his day emerged in several canonical works of late twentieth-century history and philosophy of biology (for instance: David Hull 1972, 1973, 1983; Michael Ruse 1975, 1979; Ernst Mayr, 1982, 1991; Michael Ghiselin, 1969; and M. J. S. Hodge, 1989). However, the extent and character of these debts have never been settled, and the controversies that emerged about them instruct us not only about Darwin's relationship to his contemporaries, but also about the relationships of the historical debaters to their subject matter, their own intellectual contexts and commitments, and their dialectical relationships to one another. This paper explores the thesis that differences of a disciplinary and doctrinal character have played a role in how the late twentieth-century debates over "Darwin's debt to philosophy" (Ruse, 1975) have proceeded. From this perspective, I critically review four major disputes on this issue: (1) Mayr, Ghiselin, and Hull's construals of Darwin as a skilled and original philosopher of science, compared to Ruse's construal of Darwin as steeped in and carrying out the pre-existing philosophical protocols of Herschel and Whewell; (2) Hull's claim that John Herschel and William Whewell's philosophies of science were fundamentally incompatible with Darwin's theory, in contrast to Ruse's claim that Darwin was directly guided by Herschel and Whewell's philosophies of science in the discovery and development of his theory; (3) debates between various of these discussants, and others, over the relative significance of such factors as John Gould's comments to Darwin on his ornithological collections, Darwin's reading of Malthus, and Darwin's first- and second-hand study of the practices and observations of domestic breeders; and finally (4) the debate between Hodge and many contemporary philosophers (such as Lloyd, Kitcher, and others) about the applicability of ideas from late twentieth-century philosophy of science to the description of the very different situation of Darwin's development of his theory in the mid-nineteenth century. In commentary on these controversies, I suggest (following Hodge) that certain important forms of ideological distance between historical narrator, on the one hand, and the narrated figures and events, on the other, have allowed for a variety of mutually antagonistic readings of Darwin's debts to philosophy, but I argue that such mutually antagonistic readings be viewed as both methodologically inevitable and philosophically instructive, rather than collapsed in favor of a purely contextualist history of Darwin's ideas. A second-order review of these debates allows us to see how certain dimensions of Darwin's scientific practice can be more richly understood through such philosophically motivated and informed approaches.

Defending Limited Non-Deference to Science Experts

Lawrence Lengbeyer
United States Naval Academy
United States

Scientists and their supporters often portray as exasperatingly irrational those laypersons who refuse to accede to practical recommendations issued by expert scientists and 'science appliers' (e.g., public health authorities and regulators). The latter groups' standard explanations for such non-deference focus upon irrationalities besetting the laity, but a better explanation for at least some of the non-deference is that many laypersons rationally elect to substitute their own judgments for those urged upon them by the scientific community. Science-based recommendations, as I treat them, have the general form

In light of the science on X, if you seek outcome O, you ought to V.

Non-deferring laypersons deny the soundness or cogency of the V-supporting argumentation—though supposedly they are not competent to do so, given their gross epistemic inferiority to the scientific authorities who create and endorse the arguments. Being thus epistemically irrational, they end up instrumentally irrational, making poor choices for serving their own interests (as well as broader societal interests). The non-deferring laypersons are thought to violate two mandates of rationality: for 'internal deference' to the underlying science as true (or probable) enough to constitute an unproblematic background for decisionmaking, and for 'external deference' to the practical application of that science to the concrete extra-scientific circumstances. I argue that rationality does not require categorical adherence to these mandates. In any given case, non-deference by some laypersons might be warranted by one or more of four distinct rationales.

1. Value-ladenness: The science-based recommendation discernibly, to these laypersons, depends upon non-scientific (political, legal, moral, or prudential) value-choice or value-weighting assumptions. It embodies a certain prioritizing of the plurality of relevant values, including specific values packed within the recommendation's O parameter (which is typically stated either generally, e.g. "health" or "safety," or not at all), perhaps along with others overlooked or slighted by the recommendation. There is no rational requirement for laypersons to adopt the value prioritizations of scientists and science appliers.

2. Non-scientific-reasoning-ladenness: The V-supporting argumentation for the science-based recommendation discernibly, to these laypersons, relies upon reasoning moves that are not distinctive to science—moves whose critical assessment demands no scientific expertise—and whose perceived weaknesses reasonably undermine the case for conformity to the recommendation.

3. Overgeneralization/Overaggregation: The science-based recommendation discernibly, to these laypersons, is not adequately tailored to their specific situations, however well it suits the broader target population. The research-based rationale for the recommendation reasonably seems not to apply to them.

4. Untrustworthy science: The science-based recommendation discernibly, to these laypersons, is based upon scientific research that is of doubtful quality.

The first three rationales cast doubt upon the ‘external deference mandate,’ questioning not the existence of sound underlying research but the recommendation’s judgments about how this research ought to be applied. The fourth is a challenge to the ‘internal deference mandate,’ and embodies a more contentious proposal: that the presupposition of an impermeable expert-layperson dichotomy is unrealistically simplistic, and that some outsiders are rationally licensed to engage directly and critically with insider scientific analysis and reasoning.

Democracy and Planetary Defense: Involving Citizens in Technical Decisions Through Participatory Technology Assessment

David Tomblin
University of Maryland, College Park
United States

Zachary Pirtle
National Aeronautics and Space Administration
United States

Engineers try to develop systems and perform missions that will have significant value for the public. However, sometimes it is difficult to understand what members of society value, and it is even more difficult to consider the public prior to actually developing a system. While some have called for thought experiments to assess what an informed public may want (Kitcher 2001), there are valuable ways to both involve and inform the public. Participatory technology assessment (pTA) seeks to gain public perspective such that it can inform government decision-making (Sclove 2010).

We will discuss an experiment in using pTA to inform early technical decision making. In partnership with NASA, the Expert and Citizen Assessment of Science and Technology (ECAST) network conducted a pTA-based forum of NASA’s Asteroid Initiative. The goal of the forum was to assess everyday citizens’ values, or their preferences on what decisions and goals NASA should embrace. ECAST, using prior experience with social science techniques for structuring dialog among citizens, developed exercises to help participants debate specific issues that NASA wanted input on.

The forums took place in Phoenix, Arizona and Boston, Massachusetts respectively on November 8th and 15th, 2014, with 183 citizens attending in total. ECAST led the participant selection process and helped ensure that demographics were roughly comparable to local populations, while working to minimize self-selection biases on the part of space advocates among the participant pool. The citizens had facilitator-led, structured discussions, where NASA personnel were on hand to answer basic participant questions but weren’t allowed to interfere with the discussion.

Here, we will only focus on deliberation results related to Planetary Defense, which is beginning to get increased recognition as a national priority, with NASA receiving increased funding for planetary defense and reorganizing its planetary defense initiatives into the Planetary Defense Coordination Office. We solicited structured and deliberated public input about different options about how to evolve asteroid detection and planetary defense capabilities. We will explore how values and perceptions of risk and technology intermingled in the participants’ responses. We’ll then discuss the importance of and challenges with affecting early ‘upstream’ technical decisions and the ways in which informed public input about values and technical options can play a role.

Developing Model Organisms for Model Organisms in the Study of Sleep

William Bechtel
Department of Philosophy and Center for Circadian Biology, University of Calfornia, San Diego
United States

Practice-oriented philosophers of science have shown how researchers established model organisms as research tools and have justified extending results obtaining using a given model to a target organism (usually humans). In the case of sleep, there was a substantial legacy of using rodents as models. In this talk I focus on how, beginning in 2000, sleep researchers developed three non-mammalian model organisms— round worms (Caenorhabditis elegans), fruit flies (Drosophila melanoganaster), and zebra fish (Danio rerio)—and pursued them as models for sleep in rodents. A major motivation for going beyond mammalian models is that it has been very difficult to identify and decompose the mechanisms that govern sleep in mammals. On the one hand, many different mechanisms seem to be involved and on the other hand, no proposed components explain much of the variability found in sleep across mammalian species. I explore both the challenges in establishing these model organisms for the study of sleep and some of the ways they have been put to use.

A major challenge in developing non-mammalian models for sleep was to establish that other organisms sleep. By the late 20th century, slow-wave rhythms (< 4 Hz) resulting from synchronized hyperpolarization of cortical neurons measured by electrophysiology had become the standard for identifying sleep. To identify sleep in non-mammalian organisms, researchers had to return to the behavioral criteria that had been the basis for establishing the electrophysiological criteria: (1) period of quiescence, with a species specific posture, (2) increased arousal threshold, but (3) rapid return to wakefulness, (4) homeostatic demand for sleep, including compensation for lost sleep. I explore the experimental strategies and procedures that were developed to show that fruit flies and zebra fish experience sleep each day. I then describe how similar states were identified in round worms, occurring not on a daily basis but between larval stages.

Once sleep was demonstrated in these model organisms, researchers employed them to gain insight into conserved genes and proteins involved in sleep and to combine these into mechanistic accounts of sleep. In some cases, such as signaling involving cAMP and epidermal growth factor receptor (EGFR), the research began with identification of genes or proteins that affected sleep in rodents; researchers turned to the non-mammalian model organisms to elucidate the mechanism. In other cases, such as the Shaker gene, the entities were first discovered and the mechanism worked out in one of the models (flies) and then homologs were sought in mammals.

The discovery of sleep in flies and worms deepened a long-standing mystery as to why animals sleep given that sleep leaves them inactive and vulnerable several hours of the day. Beyond providing insights into the mechanisms of sleep, discovering sleep in invertebrate model organisms provides a new strategy for determining the function of sleep. Since it occurs in multiple non-mammalian animals, it presumably answered a demand in the common ancestor of all these species, perhaps a demand resulting from the mechanism of nerve transmission itself or of the need for nerve transmission to exhibit plasticity.

Disciplinary Capture and Path Dependence: When Interdisciplinarity Goes Bad

Evelyn Brister
Rochester Institute of Technology
United States

Many scientific problems, in particular the kinds of problems addressed by applied sciences such as agriculture, medicine, environmental science, and engineering, require collaborative, interdisciplinary solutions. Some interdisciplinary collaborations aim for multidisciplinarity: here, researchers simply contribute their disciplinary knowledge to a larger project in a way that does not expose or call into question different disciplinary standards of evidence, background assumptions, or theories of causality. Multidisciplinarity can be an efficient way of dividing intellectual labor. However, some complex problems require collaborations that, in order to construct novel solutions, must transform, reshape, or integrate the epistemic frameworks of different disciplines.

This level of integration (or transdisciplinarity) is a rare achievement. One significant barrier is what I call "disciplinary capture" (me, 2016). Disciplinary capture is what happens when an attempt at transdisciplinarity defaults to a single established disciplinary approach, one which prioritizes the concepts, methods, and evidentiary standards of a dominant discipline. When this happens, one discipline effectively "captures" the other and the collaboration fails to be genuinely transdisciplinary.

In order to better understand disciplinary capture, and how it may be avoided or repaired, this paper looks at whether disciplinary capture is path dependent. Path dependence is a concept in political science used to describe how political or economic processes unfold over time, such that historical contingencies have an irreversible impact (Page 2006). Most often, the concept of path dependence has been applied to economic events (Arthur 1994) and policy development (Kay 2005), but it has also been applied to biological evolution (Desjardins 2011) and to the production of scientific knowledge (Peacock 2009). In a minimal sense, path dependence identifies events and processes that explain historical contingency. In a stronger sense, an outcome is path dependent if it 1) depends on the timing and sequence of contingent decisions, 2) has increasing returns, and 3) is irreversible.

I argue that the concept of path dependence is useful in analyzing interdisciplinary collaborations and the problem of disciplinary capture. As scientists pursue interdisciplinary research, there are a number of decision points where strategic decisions may give one discipline "first-mover" priority over another. These decisions may then be sufficient to frame the process so that later decisions fall into place in a way that unintentionally favors, and eventually "locks in," the standard practices, concepts, and methods of one discipline. For example, an early decision to isolate one type of causal process rather than another may determine the kinds of data collected, and that may lock in further research decisions concerning standards of evidence and data analysis. We see this in the (not uncommon) complaint of social scientists who object that they are often assigned a service role in their collaborations with natural scientists (Viseu 2015).

I evaluate how and when disciplinary capture is strongly path dependent and what that means for conceptual evolution and the development of scientific inquiry. Given the importance of interdisciplinary research, I also analyze various institutional means of counteracting the forces that favor disciplinary capture.

Discovery: Abduction in Everyday Practice of Science

Frederick Grinnell
UT Southwestern Medical Center
United States

In this paper, I offer a non-canonical interpretation of abduction, C. S. Peirce's logic of scientific discovery. Peirce's well known abduction scheme describes a series of events that sometimes occur in everyday practice of science.

The surprising fact, C, is observed. But if A were true, C would be a matter of course. Hence, there is reason to suspect that A is true. (5.189)

Most commentators understand the second and third lines as a certain type of inference to explain the surprising fact or the path to establish a new hypothesis worth pursuing in the future, and they take it for granted that the scheme refers to a contextual problem-at-hand P1 for which C and A both are relevant. What I will discuss is the situation in which the surprising fact C is observed in the course of studying P1, but the researcher recognizes that hypothesis A, according to which C would be a matter of course, is relevant to a completely different research problem P2. The original experiment designed to test some aspect of P1 becomes an unintended experiment that tests some aspect of P2.

In the examples that I will describe, the surprising observation resembles a puzzle piece that can be seen as fitting into two completely different research problems, P1 and P2. The investigator who sees both possibilities experiences a gestalt switch. Peirce discussed this situation as an aspect of perceptual judgement when he described a gestalt switch as the connection between abduction and perception that comes "like a flash" putting previously co existing elements together in a way not previously dreamed of (5.183). Abduction understood along these lines resembles the transformation of scientific world view as described by Kuhn. However, rather than Kuhn's macrohistorical revolutions of scientific communities, I will focus on abduction as a key mechanism for the microhistorical changes in individual understanding (e.g., as in the thought styles described by Ludwik Fleck) experienced by researchers during everyday practice of science.

"Surprising" is a key ingredient in Peirce's abduction scheme for two reasons. First, not only must the researcher have the necessary background knowledge and be open to the possibility of noticing the unexpected, but also the surprising fact must be sufficiently surprising to attract attention. Results that fall outside a researcher's expectations often go unnoticed. In my own case, I can pinpoint examples of abductive moments gained and lost. Second, noticing alone will be inconsequential without corresponding action. The surprising fact C and new research problem that it brings to mind must be sufficiently surprising to abduct a researcher's mind away from the initial problem at-hand so as to begin investigation on the new project, often a scary prospect to undertake because time, energy and money are limited resources in everyday practice of science.

When the version of abduction described above occurs in the life of a researcher, the consequences for the individual can be profound leading to discovery of a new research problem that was not recognized previously and not the subject of investigation beforehand.

Discrimination and Accumulation

Ron Mallon
Washington University
United States

Systematic discrimination results in disadvantage, but explaining exactly how this occurs turns out to appeal to diverse and complex entities. For instance, many contemporary accounts of discrimination focus on small acts of discrimination that aggregate to produce large scale disadvantage, but how does this aggregation occur? This talk develops the idea of accumulation mechanisms that serve to aggregate individual events by discussing several different kinds of such mechanisms. Because different accumulation mechanisms operate for different sorts of categories, they offer a means of understanding how discrimination intersects differently for different social groups. They also offer a possible point of intervention to alleviate or rectify disadvantage.

Disentangling Discourse on Misconduct

Hanne Andersen
University of Copenhagen
Denmark

Douglas Allchin
University of Minnesota
United States

Since the mid-1980es, when scientific misconduct became a major issue in public policy, discussions have often focused on how to define misconduct and how to distinguish it from so-called 'honest error' and from the 'grey zone' of poor practice.

In this talk we show how concepts used to describe misconduct and error in science are ambiguous and combine epistemic and ethical elements. Further, we shall show that additional difficulties arise from common distinctions between different degrees of misconduct phrased in terms intentions.

We shall first analyze the commonly used expressions 'honest error' and 'serious derivations from commonly accepted practices' and articulate a concept of research malpractice and its requisite professional contexts. By drawing on analogies to the concept of malpractice within professions such as medicine, law and engineering, we shall show how the concept of research malpractice will help address central but difficult questions about intent, recklessness and negligence.

Ultimately, we argue that we need to distinguish more fully between the epistemic, ethical, and professional dimensions of science and develop deeper epistemic understanding of both error and expertise.

Diverse Practices With Model Organisms (Symposium)

William Bechtel
University of Calfornia, San Diego
United States

A number of philosophers as well as researchers in other science-studies disciplines have examined the practices of establishing model organisms and drawing inferences from research on model organisms to target organisms. This symposium seeks to build on this work in part by expanding the focus to additional examples, each of which is intended to provide additional insights into (a) processes through model organisms are developed in the biomedical sciences and (b) the reasoning practices scientists employ in drawing results with the model organisms. It will also use each case to elaborate on practical challenges researchers confront.

Rebecca Hardesty attempts to integrate a philosophy of science and a science studies perspective on model organism research, focusing her discussion around an ethnographic study of a laboratory that has developed a new mouse model of Down Syndrome. In particular, she will show that the laboratory set a very high epistemic standard and how this has created practical problems for the lab.

Veli-Pekka Parkkinen develops case studies involving animal models used in arteriosclerosis research and humanized mice models used in cancer research and will use them to analyze the reasoning researchers engage in. He challenges claims that the difference between theoretical models and model organisms is that the later does not draw on analogies with the target but on phylogeny. Instead, he argues that the difference has to do with the evidential role model organism research plays in establishing causal dependences and the ways in which researchers establish that the same dependency is found in model and target.

William Bechtel focuses on why, given the existence of rodent models, sleep researchers have cultivated non-mammalian model organisms and how, in doing so, they have had to develop alternative criteria for identifying of sleep. He argues that a major objective in developing especially invertebrate models was to find simpler mechanisms in which it is possible to identify components and he evaluates their success to date.

Presenters and Titles: Rebecca Hardesty, An Ethnographic Approach to Integrating Perspectives on Model Organisms Veli-Pekka Parkkinen, The epistemic role of model organisms in biomedicine: are animal models theoretical models? William Bechtel, Developing Model Organisms for Model Organisms in the Study of Sleep.

Does Autism Spectrum Disorder Have a Gender?

Ruth J. Sample
University of New Hampshire
United States

Autism Spectrum Disorder (ASD) is a brain-based developmental syndromic disorder that results in a cluster of atypical behavioral traits (ASD is also associated with, but does not include, other traits such as cognitive disability (mental retardation, or MR)). ASD is also strongly male-prevalent, with an estimated sex ratio of 4:1 (male-to-female) in “classic” autism and a sex ratio as high as 10:1 in Asperger’s or High Functioning Autism (autism without cognitive disability or speech delay). Hans Asperger himself based his construction of the syndrome on his observations of 400 boys. More recently, Simon Baron-Cohen has argued that ASD is the “extreme male brain,” and that autism should be seen as an exaggerated form of the male mental phenotype in which “systemizing” is heightened and “empathizing” is diminished. But what is the relationship between disorders that are male-prevalent and those that are male disorders? How could having autism be meaningfully understood as having the “extreme male brain” when so many girls and women have autism? And what would it mean to say that one has a male brain or a female brain? In this paper I investigate the intelligibility of distinguishing between kinds of brains based on gender and its implications for brain-based psychiatric diagnoses such as ASD. 1. What is the relationship between male prevalence and a disorder being a “male disorder”? Many disorders are male-prevalent due to the fact that having a single copy of the X chromosome, as males typically do, provides no protection from a mutated allele on that chromosome. For example, hemophilia and colorblindness are X-linked disorders that are exclusively male. However, we do not think of color-blindness or hemophilia as male traits. Similarly, there are over 70 X-linked mutations that cause mental retardation in the males, but not the females, who have them. Are these cases “male forms” of cognitive disability? How is male-prevalence relevant to our classification of traits or clusters of traits as male or female? 2. What would one have to show in order to establish that there are male brains and female brains? Baron-Cohen argues that there are male brains and female brains, although some females have male brains and have some males have female brains. Recently, Daphna Joel has argued that there is no reason to think there are male brains and female brains. However, Baron-Cohen’s distinction is based on average differences in functioning, whereas Joel argues against a gendered typology of brains on the basis of morphology, not functioning. Are they arguing at cross-purposes? Is there some other basis for meaningfully distinguishing between male brains and female brains? 3. Finally, what we gain if we were to view psychiatric diagnoses as gendered? What problems does thinking of certain disorders as male or female create? I briefly consider the attempts by Crespi and Badcock to understand schizophrenia as ASD’s opposite number: The Extreme Female Brain.

Distributed Cognition in Critical Care Medicine

Thomas Cunningham
University of Arkansas for Medical Sciences
United States

This paper proposes an epistemological model of critical care medicine. It is argued that thinking of critical care in terms of distributed cognition improves our understanding of medical practice by revealing its complexity and exposing areas of practice to scrutiny that are hidden when described solely in terms of individual cognition.

The paper begins with a prototypical case of critical care medicine: RM, a 73-year-old man, is hospitalized after presenting to the emergency room complaining of a cough, chest pain, fever, and chills. RM has a history of bacterial pneumonia following aspiration. In the past he has required respiratory support and antibiotics. He delayed coming to the emergency department because he was spending time with his family over a holiday. He has a girlfriend and four children, only two of whom live nearby.

The second section of the paper distinguishes between types of cognition that occur in the prototypical case. Building on Eddy's (1996) work, one type of cognition is clinical judgment about the patient's disease, where empirical evidence is collected and evaluated to develop a differential diagnosis, prognosis, and reasonable treatment options. Another type of cognition is treatment decision making, where patient or proxy values and preferences are elicited and related to probable health outcomes and treatment options.

Section Three summarizes distributed cognition theory. On Hutchins' (1995) account, distributed cognition captures situations where individuals think together in order to reach shared goals, for example, during ship navigation. Philosophers have argued that Hutchins' view should be circumscribed to situations where the cognition required to reach a goal can conceivably be performed by a single human with the aid of computational devices (Magnus 2007).

To show that both clinical judgment and treatment decision making in critical care may be fruitfully characterized in terms of distributed cognition and in a way that meets Magnus' description, RM's case is analyzed in greater detail in the fourth section. For example, during clinical judgment a differential diagnosis for bacterial pneumonia includes atelectasis, chronic obstructive pulmonary disease, and both fungal and viral pneumonia. To discriminate between these and other etiologies requires clinical studies, such as chest radiography, blood cultures, and thoracentesis. Findings from these studies are interpreted by specialists, such as radiologists and pathologists. Thus, in thinking through the alternative etiologies for RM's illness, a healthcare team will separately work on different aspects of the problem, lending each of their expertise to one or more specific sub-problem. Likewise, in order to determine which treatment option is appropriate, RM's family members must think about his healthcare values and treatment preferences and whether they determine a treatment that is best for him in the circumstances (Scheunemann et al 2012).

The final section of the paper concludes by considering two objections, that distributed cognition is reducible to individual cognition and that critical care is a special case where distributed cognition is a useful model but there is no reason to think that distributed cognition is useful more widely in the epistemology of medicine.

References

Eddy, David M. (1996). Clinical Decision-making From Theory to Practice: A collection of essays from the Journal of the American Medical Association. Sudbury, MA: Jones and Bartlett Publishers.

Hutchins, Edwin (1995). Cognition in the Wild. Cambridge, MA.: MIT Press.

Magnus, P. D. (2007). "Distributed Cognition and the Task of Science." Social Studies of Science 37: 297-310.

Scheunemann, Leslie P., Robert M. Arnold, and Douglas B. White. (2012). "The Facilitated Values History: Helping Surrogates Make Authentic Decisions For Incapacitated Patients With Advanced Illness." American Journal of Respiratory and Critical Care Medicine 186(6):480-486.

“Each Chief Step in Science has been a Lesson in Logic”: Pragmaticist Lessons from Peirce’s History of Science

Chiara Ambrosio
UCL
United Kingdom

The 1890s were Charles S. Peirce’s most productive years. Along with refining his own brand of Pragmatism—subsequently re-labelled “Pragmaticism”, in explicit contrast to William James’ appropriation of his own philosophical brainchild—Peirce was busy developing his theory of signs, fleshing out his “scientific metaphysics”, and completing his diagrammatic system of Existential Graphs in logic. In parallel to all this, Peirce was also writing a history of science in one volume, promised—but never delivered—to the editor Putnam’s Sons.

In this paper, I will draw on Peirce’s historical works to develop novel links between Peirce’s philosophy of the history of science and current debates in historiography. My main aim is to show that Peirce’s contributions to epistemology and even metaphysics can only be fleshed out in full when placed in the context of his continued engagement with the history of science. This is not only limited to his writings of the 1890s: indeed, it has been rarely pointed out in the literature that one of Peirce’s key Pragmati(ci)st writings, “The Fixation of Belief” (1877), opens with a discussion of the history of science. “Every work of science great enough to be remembered for a few generations affords some exemplification of the defective state of the art of reasoning when it was written; and each chief step in science has been a lesson in logic” (EP1: 111), Peirce claims in his 1877 paper. Peirce’s idea, fully developed later on, but already visible in his early pragmaticist writings, is that history discloses patterns of reasoning construed in their broadest form, including—perhaps most interestingly—instances in which reasoning goes wrong. Taken as a project in which epistemological lessons and historical understanding are inseparable from each other, Peirce’s philosophy of the history of science exemplifies what Peter Galison (2010) has described as a truly relentless historicism: “a history and philosophy of science with no day pass from history, one where the philosophy enters the stage with the history, not before the account begins” (Galison 2010:123).

The second part of my paper will focus on a specific aspect of Peirce’s history of science: the neglected role that his historical works played in shaping his classification of the sciences. This will serve as a case study to demonstrate the practical role that history played in the shaping of one of the most discussed aspects of Peirce’s philosophical work. Peirce scholars have traditionally read his work on the classification of sciences as a primarily metaphysical, epistemological and logical project (Atkins 2006; Kent 1987). Various accounts relate his classification of the sciences to his mature formulation of the three categories of Firstness, Secondness and Thirdness, and to his quest for the place of logic amongst the normative sciences. So far, the literature has entirely neglected the fact that Peirce’s first outline of his classification appears in the context of his 1892 Lowell Lectures on the History of Science, the very lectures that would form the basis for his history of science for Putnam’s Son. Drawing on archival material, I will argue that this is not a coincidence at all, and that what is usually considered as one of Peirce’s most metaphysical projects ultimately sprung, conceptually and materially, from his work on and with the history of science.

Bibliography

Richard Kenneth Atkins (2006), “Restructuring the Sciences: Peirce’s Categories and his Classifications of the Sciences”, Transactions of the Charles S. Peirce Society, vol. 42 no. 4, pp. 483-500.

Peter Galison (2010), “Ten Problems in History and Philosophy of Science”, ISIS vol. 99 no. 1, p. 123.

Beverley Kent (1987), Charles S. Peirce, Logic and the Classification of the Sciences, Montreal: McGill-Queen’s University Press.

Charles S. Peirce (1992-98) The Essential Peirce (EP), ed. The Peirce Edition Project (2 vols.), Bloomington, Indiana: Indiana University Press, 1992-1998, pp. 42-56

eBirding, Conservation, and Mapping the Moral Terrain of Science at a smaller Scale

Jon Rosenberg
University of Washington
United States

The role that values play in scientific practice continues to attract important philosophical attention, as do questions about how to make science more socially responsible and responsive. These discussions often revolve around which types of values (epistemic vs. non-epistemic) can legitimately guide scientific practice and which ways (directly vs. indirectly) they can legitimately do so. Though undoubtedly important, such general distinctions about how science and values interact construe both "science" and "values" quite broadly. Without filling out, they provide little guidance on how to answer these questions in more specific and localized contexts of scientific practice. Who are these particular scientists responsible to? For what should they be held responsible? Whether or not science is being conducted responsibly and whether or not the practice is socially engaged will hinge largely on how these questions are answered at a local level. Further, answering these questions may require determinations for which these categories are too coarse. In 'The Moral Terrain of Science,' Heather Douglas takes some steps towards articulating a framework for determining the responsibilities that science has to other individuals and communities, though her framework also suffers from the same problem of generality. In this paper, I extend Douglas' framework so that it can describe the more fine-grained details of science's moral obligations to more localized communities and practices. First, I describe Douglas' "dimensions" for mapping the terrain of moral considerations in scientific practice. I then discuss a citizen science project, called 'eBird', which gathers data from Birders for use in scientific research and conservation work. I then argue that Douglas' account cannot properly articulate the moral terrain of the interactions between these scientists and the birding community. This is because the first of Douglas' dimensions of consideration—the bases of scientific responsibility—is both too general and too specific. It is too general insofar as it takes society as a whole to be the community toward which science is ethically responsible. It is too specific insofar as it specifies that science is embedded within the community to which it is responsible. The scientific community is neither a subset of the eBirding community (as it is of society), nor is it valued by eBirders for all and only the reasons for which it is valued by society. Thus, the problem in applying Douglas' framework to the eBirding community shows that mapping out the moral terrain of science requires something more flexible. We must be able to consider the variety of communities to which science may be responsible, the different relationships they may have, as well as the particular responsibilities they may have to each other. I conclude by sketching a step in the right direction: First, I suggest that we can distinguish between bases of responsibility to one's own practice and bases of responsibility to other practices. Further, the details of the latter can only be determined by understanding the overlap and interplay between the aims, values, and teleoaffective structures of science and those of the practices with which it engages.

An Engineering Paradigm for the Philosophy of Science: The Discovery of Piezo-Electricity as a Paradigm Example of Science

Mieke Boon
University of Twente
Netherlands

Nadine de Courtenay
Université Paris Diderot, Sorbonne Paris Cité, laboratoire Sphere UMR
France

Philosophical investigation and interpretation of (evolving) methodologies and epistemological characteristics of concrete scientific practices such as the biomedical sciences, systems biology and synthetic biology (e.g., Kitano 2002a&b; Knuuttila and Boon 2011; Boon 2012; MacLeod and Nersessian 2013; Knuuttila and Loettgers 2013 and 2014; amongst many others) show that the scientific enterprise is driven by creative utilization, construction and invention of: scientific concepts, theoretical constructs, technological devices, ways of scientific reasoning, ways of representing measured data and knowledge, and increasingly sophisticated epistemic strategies in so doing. These investigations by philosophically inclined scientists and philosophers of science seems to have opened up the possibility to reflect on the paradigm of science that has led the philosophy of science. Why, indeed, does it take decades to overcome a normative and ideological picture of science that is not supportive nor explanatory for current scientific practices, and that may even be harmful in regard of societal and educational capacities of science? In spite of the pervasive awareness of Kuhn's notion of paradigm, scientists and philosophers of science seem to have been captured by a paradigm of science that prevented them from recognizing aspects that disagreed with it. Contrary to the suggestion in current philosophy of science that engineering approaches as exemplified in the life-sciences are relatively new to scientific practice (e.g., Nordmann 2015, Knuuttila and Loettgers 2013; MacLeod and Nersessian 2013), we will defend that the philosophy of science at the beginning of the 20th century, instead of taking theoretical physics as its paradigm example of what science really is, could have adopted the concurrent discovery of piezo-electricity as a paradigm case of scientific research. We will use this case to develop an engineering paradigm for the philosophy of science.

References

Boon, M. (2012) Scientific concepts in the engineering sciences : epistemic tools for creating and intervening with phenomena. In: Scientific concepts and investigative practice.

U. Feest & F. Steinle (Eds.). Berlin studies in knowledge research (3). De Gruyter, Berlin, 219-243.

Kitano: H. (2002a). Computational Systems Biology. Nature, 420: 206-210. doi:10.1038/nature01254.

Kitano: H. (2002b). Looking beyond the details: a rise in system-oriented approaches in genetics and molecular biology. Current Genetics, 41: 1-10.

Knuuttila, T.T and M. Boon (2011) How do models give us knowledge? The case of Carnot's ideal heat engine. European journal for philosophy of science, 1 (3): 309-334.

Knuuttila T.T and A. Loettgers (2013). Synthetic Modeling and Mechanistic Account: Material Recombination and Beyond. Philosophy of Science, 80(5): 874-885.

Knuuttila T.T and A. Loettgers (2014). Varieties of noise: Analogical reasoning in synthetic biology. Studies in History and Philosophy of Science, 48: 76-88.

MacLeod, M. and N.J. Nersessian (2013). Coupling Simulation and experiment: The bimodal strategy in integrative systems biology. Studies in History and Philosophy of Biological and Biomedical Science. 44: 572-584.

Nordmann A. (2015). Synthetic Biology at the Limits of Science. in: Synthetic Biology, Risk Engineering. B. Giese et. al. (eds.) Springer International Publishing: 31-58.

Emerging Model Organisms, Platform Development and Repertoire Acquisition

Christopher DiTeresi
George Mason University
United States

One exciting development to come out of work on the philosophy of scientific in practice is an increasingly widespread recognition of scientific practices as epistemologically significant objects in their own right. This recognition affords the conceptual space to pose and investigate new questions about the practical epistemology of science: How and under what conditions do particular scientific practices become possible? How do such conditions come to be realized in different places and within different research contexts? What is to be done in order to know, and how can the capacity to make knowledge be cultivated? And more generally, how, and with what concepts, are we to think about the practical possibilities for knowledge generation and how they vary in and across research situations? Platforms and repertoires are two promising concepts for addressing these questions. Both concepts have figured in recent philosophical studies of cases from the life sciences, where they refer to research arrangements (such as model organisms and model taxa) that scaffold many related projects over time in different labs/sites and spanning multiple specialties. But while the importance of such arrangements to shaping the practical possibilities for research is clear, the concepts for describing these arrangements are not. Broadly speaking, the usage of platform and repertoire reflects the familiar senses of these terms in other contexts. The idea of operating systems as platforms on which a variety of applications can run is familiar from software development. And the idea of a repertoire as a set of pieces that a group of players or musicians is able to perform is familiar from music and theater. Both ideas involve arrangements that facilitate and support variation in contents or operations, but more work is needed to understand precisely how to cash out platforms and repertoires in relation to scientific research. This paper contributes to the conceptual clarification of research platforms and repertoires by exploring how they can be used to analyze the special case of ‘emerging model organisms.’ Emerging model organisms are the subject of a multi-volume series of Cold Spring Harbor Protocols beginning in 2010. The organisms that appear as entries in the series are (non-model) experimental organisms that the entries’ authors argue are promising research models that other labs might do well to adopt. I argue that what is remarkable about this series as a case for thinking about platforms and repertoires is that it is designed not to seed new model organism communities, but to re-engineer existing model organism research platforms by outfitting them with companion non-model organisms that make it possible in practice to do comparative research using canonical model organisms. Moreover, the editors of the series explicitly recognize that the usefulness and power of emerging model organisms depends directly on the creation of (not very large) communities of researchers that work with and from a common repertoire of techniques and practices. Lastly, I consider whether the standardized form for entries in the series—a protocol for protocols—suffices to make the series itself a platform that facilitates the acquisition of similar repertoires in a variety of research platforms.

The Epistemic Role of Model Organisms in Biomedicine

Veli-Pekka Parkkinen
University of Kent
United Kingdom

Is an animal model like a concrete version of a theoretical model; a surrogate for its intended target from which results are transferred to the target based on known analogies between the two systems? Recently, Levy and Currie (2015) have argued against this picture by pointing out how the strategies of justifying model-to-target inferences differ between the practices of theoretical modelling and model organism research. According to Levy and Currie, instead of serving as a surrogate for its target in virtue of deliberately constructed analogy, an animal model serves as a basis for empirical extrapolations via phylogenetic inferences. I present two case-studies—animal models in atherosclerosis research and humanised mice as models of human cancer—to first argue that biomedical scientists may rely on strategies similar to theoretical modelling in justifying conclusions drawn from model organism research. These strategies include deliberate construction of analogies between the model and the target, and the evaluation of robustness of results derived from a family of models. However, I intend this not as an argument against the importance of phylogenetic inference that Levy and Currie have stressed. Instead I argue that there is a further difference that distinguishes animal models from theoretical models, having to do with explanation. I argue that theoretical models explain by describing causal and constitutive dependencies between phenomena. In the model, these dependencies are explicitly expressed, often as mathematical equations, which allows derivations of outcomes of various manipulations of the model that one then takes to represent the behaviour of the target in various hypothetical conditions. These results are validated based on model-target analogies that the modeller has deliberately coded into the model, whereas robustness analysis is used to secure that the results do not crucially depend on further, unrealistic auxiliary assumptions of the model. An adequate model thus explains by answering 'what-if?' questions about the target's behaviour. By contrast, a result produced by studying a model organism is evidence that some causal or constitutive dependency obtains, but these dependencies are not in any obvious way explicitly expressed in the model—rather they are discovered through experiment. Transferring a result from the model to the target requires a hypothesis about the exact nature of the dependency of interest, and an assumption that the dependency is exhibited similarly in the target, such as assuming that a similar mechanism is operating in both. The reasoning strategies of phylogenetic inference, analogy, and robustness, are strategies for evaluating the quality of the model-based results as evidence for explanatory hypotheses about the target. In conclusion, theoretical and animal models have different epistemic roles, even though broadly similar reasoning strategies may be employed for justifying model-to-target inferences in both cases.

References

Levy A & Currie A (2015). Model Organisms are not (Theoretical) Models, British Journal for the Philosophy of Science, 66(2), 327-348.

An Ethnographic Approach to Integrating Perspectives on Model Organisms

Rebecca Hardesty
University of California, San Diego
United States

Due to advances in genetic sequencing, model organisms have emerged as a key feature of biological, particularly biomedical, research where they serve as "cutting edge" material stand-ins for the study of other organisms. Authors in the philosophy of science and in science and technology studies (STS) have become interested in various aspects of the use of model organisms. Philosophers have become concerned with the status of model organisms and the ways in which biologists use them to make justifiable inferences. STS practitioners have focused on the construction of their "model" status and the kinds of knowledge that research involving them produces. Despite the same topical focus of model organisms, there is a pre-existing rift between the two approaches stemming from different theoretical and methodological commitments. Philosophy of science, dominated by support for scientific realism, has taken issue with STS's attention to social constructivism and its explicitly anthropological leanings. STS finds philosophical approaches to science to be too focused on normative concerns and overly focused on the finished products of science. Perhaps as a result, there is a gap in the study of model organisms; however, Michael Weisberg's work on modelling practice is an example of scholarship that can be seen as bridging these two schools. Weisberg is interested in the traditional philosophical concerns of ideal norms of success. He is also interested in providing a descriptively adequate account of scientific practice. In Weisberg's terminology, there is a lack of work on how biologists collaboratively develop the construal of a model organism, specifically its fidelity criteria, i.e. whether it is a sufficiently adequate representation of the target real-world phenomena. This under-discussed aspect of modeling practice requires bringing together STS's interest in ethnography and sociality, as well as philosophy's concerns regarding the norms of successful science and justification. This is the project which I will undertake in this paper. I adopt a broadly ethnomethodological and ethnographic approach to describe how a group of neurobiologists in "Lab X" at "West Coast University" has developed a new mouse model of Down Syndrome (DS). I focus on Lab X's everyday practices involved in determining whether their new "GFDS" mouse model is a faithful representation of the target human condition of DS and how it compares to other DS mouse models. The lab has shown that the GFDS mouse has high genetic fidelity to human DS despite it not replicating the cognitive and behavioral phenotype of human DS to the degree to which previous mouse models have. Nonetheless, Lab X has asserted that the genetic fidelity of the GFDS mouse should be the "gold" standard for DS mouse models. However, based on my ethnographic work, I show that their call for this high epistemic standard for model organisms in DS research presents practical challenges for the lab. I conclude by articulating the future work that could be done by adopting this inclusive approach.

An Ethnography of Scientific Imagination

Michael Stuart
Center for Philosophy of Science, University of Pittsburgh
United States

My talk has two parts, one philosophical and one empirical.

In the first, I argue that intelligibility and fruitfulness are two tests for scientific understanding. When we understand something new, it is often as a result of its becoming intelligible to us, that is, we come to know what it means and implies for actual and possible experience. For example, we understand General Relativity if it is intelligible to us, and the more intelligible it is, the better we understand it. However, to demonstrate fully that we understand something, we must also be able to use it, either in thought, conversation or action. General Relativity may be intelligible to us, but this is not the same as being able to use it to make predictions, give explanations, or calibrate GPS satellites. Intelligibility and fruitfulness, together, comprise joint tests for understanding. And this reflects pedagogical orthodoxy. To discover whether someone understands x, we make them repeat x in their own words (to prove x is intelligible to them) and then have them apply x to a new case (to prove that x is fruitful for them). We reserve the highest marks for those who pass both tests, whether in end-of-year exams or essays.

If this is right, the empirical question arises: how do practicing scientists gain understanding once they have surpassed the limits of existing pedagogical resources? More specifically, how is a new formalism or experimental result interpreted and made sense of, so that it becomes intelligible and fruitful to the cutting-edge scientist?

One answer to this question is provided by an ethnography that I am now performing on an integrated computational systems biology laboratory. What I have found is that increasing intelligibility and fruitfulness requires a departure from given sensory experience in every case. In other words, scientists in fact imagine the kind of world a new formalism or result might imply. This provides an interpretation, which is then explored and tested in the mind. This is how the scientists I am studying curve and plot their data, model it, and create new tests for subsequent hypotheses based upon it. In other words, imagination appears to be necessary for making a new result or formalism intelligible and fruitful, which is necessary for the production of new scientific understanding.

An interesting result of this work is that if imagination is important for producing new understanding, perhaps we can increase understandability by manipulating the amount and quality of imagination required of students during science education. This is not currently done, and in fact, existing sociological findings suggest that scientists are taught to deny or underplay any reliance on their imaginations in undergraduate and graduate school. They are taught instead to emphasize only the "facts." If we admit the importance of subjective factors (like imagination) in science, perhaps we can thereby begin to improve scientific education and communication, including science journalism and public relations. I finish by discussing some steps to achieving this end.

Ensuring Objective Validity: Functional Triangulation in Neuroscience and the Effects of Social Biases

Derek Oswick
University of Western Ontario
Canada

Central to the aims of cognitive neuroscience is the pursuit of adequate evidence to support claims of functional localization. Scientists and philosophers appear to agree that such evidence is best attained by pooling results across laboratories and/or areas of neuroscience involved in the study of cognition. However, how evidence produced from different experiments may be said to converge, or how results from different laboratories are integrated remain topics of debate. Adina Roskies has claimed that such convergence is achieved by means of a process she terms "functional triangulation"—a process of multi-modal and multi-disciplinary corroboration and convergence of localization results. She also argues that this sort of enterprise is generally characteristic of scientific progress (Roskies 2010). As such, it seems to reflect how neuroscientists generally hold that investigative claims come to have epistemic warrant.

In this paper I aim to show that while functional triangulation may grant robustness to functional localization claims, we may still distinguish between robustness and objective validity, and while convergence may allow for robustness, it is no guarantee of objective validity. This is because social biases may go undetected in experimental research and negatively influence both the methodologies chosen and/or resulting conclusions. As a result, functional triangulation is vulnerable to the possibility that the labs that arrive at convergent results share entrenched background assumptions or social biases that could render the convergence invalid if these assumptions are insufficiently recognized and justified. Ultimately, this means that the objective validity of any convergence within functional triangulation is contingent on the (actual) diversity of positions within the discourse. This is best assured through some form of pluralism in line with feminist standpoint theory, since this sort of deliberate move towards diversity offers the best (and possibly only) means of identifying otherwise-invisible social background assumptions involved in a given paradigm.

To argue this point, I proceed in three sections. Section 2.0 will explain and characterize Roskies' understanding of the role of functional triangulation in neuroscience, and demonstrate what it does well. I then draw on the work of Robyn Bluhm and Cordelia Fine in Section 3.0, to provide illustrative examples of where convergence through functional triangulation has occurred, and yet the results fail to be objectively valid. In Section 4.0, I attempt to provide a working solution, first by turning to Sandra Harding's (1995) analysis of science as a social enterprise in order to more effectively diagnose the problem. I then argue that some form of pluralism (in line with her arguments for feminist standpoint theory) generates more effective diagnostic tools, which allows us to be more certain of the objective validity of convergent results. By implementing this sort of shift towards standpoint diversity and the diagnostic tools it affords, we are able to improve upon what functional triangulation already does well—effectively offering a way to shore up the processes by which neuroscientists already seem to think our claims are granted epistemic warrant.

The Evolution of Instruments for Empirical Investigation: Choosing Between the Material and the Algorithmic Options

Vincent Israel-Jost
Université Catholique de Louvain
Belgium

Most investigations in empirical science have been carried out with instruments for a long time: microscopes in biology, telescopes in astrophysics, X-rays radiographs in medicine, and all sorts of measuring devices and detectors in physics. Until recently, the only possible way to make these instruments evolve (arguably to obtain clearer or more reliable results) was to intervene materially on the instruments. For instance, one would design better ways to polish lenses to avoid some kind of aberration. In the past twenty years, about all of those instruments have gradually become digital. This permits investigators to use them more easily, since digital data can be displayed, stored and circulated with no effort, but the investigators can also improve the outcome computationally in various ways with algorithms. For example, when a measuring device is shown to underestimate the measurement, one can either work on the instrument to make it more accurate, or apply a digital (algorithmic) correction to the data. The existence of these two parallel options raises one main question: does there remain reasons why material improvement should be preferred over algorithmic improvement?

To answer this question, I first propose to present what material and algorithmic improvements can do. This first step aims to demonstrate that these two possible ways to improve the quality of scientific data do overlap but that there are also certain things that can only be done with one or the other. Namely, I argue that, for instance, 3D reconstruction techniques such as MRI or X-ray CAT scan must be based on algorithms. On the other hand, also as an example, when it was found that the Hubble telescope was sent into space with a major defect, the optical aberration could only be corrected by adding several (material) parts, one of which having the same aberration in reverse. I conclude that there are cases in which only one or the other type of improvement works.

In the second part of this talk, I focus precisely on those improvements that can be achieved with one or the other approach. For example, when an imaging instrument produces images that are homogeneously blurry, this can often be rectified either materially, by working on, say, a better optics, or digitally, by deconvolution methods. In this case, is it desirable to still prefer the traditional way (material) over the digital solution and if so, why? I will show that, though material improvements are still implemented when possible, scientists are less and less reluctant to consider digitally corrected data as being just as good. My explanation for this relies on those algorithmic procedures seen in the first part, which cannot be performed otherwise (e.g. 3D reconstruction). I argue that the situation when scientists don't have a choice has led them to trust digitally processed data, to the point that this trust now also applies to the situation when they do have a choice.

Evolutionary Process

John Dupré
Egenis, University of Exeter
United Kingdom

In this talk I aim to show how a process ontology enables us to move on from the increasingly discredited neo-Darwinist view of evolution. From a processual perspective, lineages are processes within which evolution occurs. Evolutionary change is then to be understood as change in the features of the subprocesses—organisms—that make up lineages. However, one of the crucial implications of a process ontology is that in comparison with traditional substance-based alternatives, we need to focus more on the explanation of stability and less exclusively on the explanation of change. To understand the changes that occur to lineages we should first provide an account of the processes that make possible their persistence.

The persistence of a lineage depends on a number of factors. It presupposes, familiarly enough, the persistence, to an appropriate degree, of its constituent organisms, and also their ability to produce new organisms (reproduce) at least as fast as organisms are lost to various causes of mortality. Replacement organisms, moreover, must either be more numerous or have equal capacities to survive and reproduce. Since reproduction is not precise copying and is not wholly uniform, this maintenance of capacities requires excess production of replacement organisms. Together with natural selection, this excess production combines to maintain a population, the cross section of a lineage, with capacities for survival and reproduction adequate to maintain the lineage. This brief account is intended to highlight the fact that Darwin's key ideas indicate a set of processes crucial to the stability of lineages quite independently of reference to any role in explaining changes in linages. I shall argue, in fact, that partly because of an inappropriate substantialist context, neo-Darwinism has concentrated excessively on the role of selection in bringing about change, and far too little on its importance for explaining the persistence or stability of lineages. Various consequences of this account will be briefly outlined. First, it evidently indicates sympathy in various key respects with the neutral theory of evolution. Second, it provides a framework within which the importance of various criticisms of the creative potential of natural selection, as well as a range of possible processes for generating novel adaptations can be assessed including symbiosis and lateral gene transfer, and phenotypic plasticity and the so-called Baldwin effect. Finally it emphasises the importance of multi-track inheritance, including various pathways for the inheritance of acquired characteristics (cultural inheritance; epigenetic inheritance; developmental niche construction).

Expanding Epistemic Risk

Justin B. Biddle
Georgia Institute of Technology
United States

Rebecca Kukla
Georgetown University
United States

The process of generating empirical knowledge is riddled with a variety of epistemic risks. At each stage of inquiry, our actions, choices, and judgments carry with them a chance that they will lead us towards mistakes and false conclusions. One of the most vigorously discussed kinds of epistemic risk is 'inductive risk'—that is, the risk of inferring a false positive or a false negative from statistical evidence. Original discussions of inductive risk focused narrowly on the final inductive step from evidence to conclusion within a scientific study (e.g., Rudner 1953, Hempel 1965). Beginning with Douglas (2000), an ever-growing body of literature has identified moments of purported inductive risk and its management not just in the final transition from evidence to hypothesis-acceptance (or rejection), but throughout the scientific process, from methodological design through data classification and interpretation. We develop a more fine-grained typology of epistemic risks and argue that many of the epistemic risks that have been classified as 'inductive risks' are actually better seen as examples of a more expansive category. We dub these 'phronetic risks' in order to mark how they pervade practical rationality more broadly. We show that classifying these as inductive risks requires distorting their practical and epistemic structure. Finally, we argue that this more fine-grained typology helps to show that 'values' in science often operate not primarily at the level of individual psychologies, but rather at the level of knowledge-generating social institutions.

References

Douglas, Heather (2000). "Inductive Risk and Values in Science." Philosophy of Science 67: 559-579.
Hempel, Carl (1965). "Science and Human Values." In Aspects of Scientific Explanation and other Essays in the Philosophy of Science, 81-96. New York: The Free Press.
Rudner, Richard (1953). "The Scientist Qua Scientist Makes Value Judgments." Philosophy of Science 20: 1-6.

Experimentation on Analogue Models

Susan Sterrett
Wichita State University
United States

Analogue models are actual physical setups used to model something else. They are especially useful when what we wish to investigate is difficult to observe or experiment upon due to size or distance in space or time: for example, if the thing we wish to investigate is too large, too far away, takes place on a time scale that is too long, does not yet exist or has ceased to exist.

Some recent historical studies provide some historical perspectives on the practice: Simon Schaffer's "Fish and Ships: Models in the Age of Reason" discusses the use of Leyden jars (and later, electric piles) to model electric fish and the use of tabletop models of house and thundercloud configurations to investigate effects of lightning. Schaffer's study also looks at the use of specially constructed scale models used in the eighteenth century in investigations comissioned for publicly funded mechanical structures, such as bridges and ships. These were more quantitatively characterized, but different principles were used by different practitioners, and there was much disagreement about methods; Schaffer's observation is that "the rival claims of custom and principle were fought out with experimental models." How did that situation, when the methods and practices of scale modeling were so hotly contested, finally give way to common agreement? Portions of Naomi Oreskes' study of the use of scale models in geology "From scaling to simulation: changing meanings and ambitions in geology" likewise recount a path from disagreement about principles of using small physical scale models to investigate the origin of geological formations such as mountain ranges.

But analogue models, and the discussion of appropriate methods for using them, are not merely of historical interest. Not only can physical analogue models be found in use today for bridges, ships, and geological formations such as volcanoes and earthquakes, but the current range and variety of analogue models is so widespread that it is too extensive to even attempt a survey. I also describe some recent uses of analogue model experiments, briefly describing important points about the results of those model experiments, and the basis for constructing them and interpreting their results. Examples of analogue models I'll menton are: models built to study surface waves in lakes, earthquakes and volcanoes in geophysics, and black holes in general relativity. I'll examine the bases for claims that these analogues are appropriate analogues of what they are used to investigate.

Finally, it is shown how the historical case studies and the more recent examples I'll discuss counter three common misconceptions about the use of analogue models.

References

Oreskes, Naomi. "From Scaling to Simulation" in Chadarevian, Soraya , and Nick Hopwood. Models: The Third Dimension of Science. Stanford, Calif: Stanford University Press, 2004. Print. pp. 93-124

Schaffer, Simon. "Fish and Ships" in Creager, Angela N. H, Elizabeth Lunbeck, and M N. Wise. Science Without Laws. Duke University Press Books, 2007. Print. pp. 71-105

Explanation and collaborative practices

Melinda Fagan
University of Utah
United States

The aim of this paper is to outline a 'collaborative' approach to explanation. It builds on my previous work on this topic ([reference omitted for anonymous review]) by approaching explanation as a collaborative activity among practicing scientists. In many scientific fields, explanatory models are constructed by integrating results from diverse research groups, frequently across traditional disciplinary boundaries (e.g., Craver 2007). I use two cases from recent systems biology to show contrasts between successful and unsuccessful explanatory collaborations (Huang 2011, Jaeger and Crombach 2012). Successful integration requires compatibility in the aims and standards of participating researchers or research groups. Often, this compatibility takes the form of complementarity rather than correspondence; i.e., research aims and standards differ, but can be combined into an overall plan that is coherent and effective in its own right. That is, construction of some explanatory models is a special case of cooperative social action, requiring compatible goals, intentions, and sub-plans (e.g., Bratman 2014). Norms for constructing such explanations can be seen as specifications of more general requirements for social action. These include, minimally: (1) tolerance of diversity in approaches to explanation; (2) an openness to connection with different approaches to explanation; and (3) mutually realizable explanatory aims, comprising diverse parts of a more inclusive project.

Next, I extend this approach to outcomes: the explanatory models produced by these collaborative practices. I use an important example of explanation in molecular biology: the operon model (Jacob and Monod 1961) to illustrate analogs of the three requirements noted above: (1) diverse components; (2) interactions among components grounded in complementarity; and (3) multilevel structure. These features, I argue, confer explanatory virtues on the models, which can be explicated in terms of collaborative concepts. Briefly: lower-level components represented in a multilevel model are unified in the sense of being organized together, interconnected to form a complex system. In describing how components are unified, the explanation traverses multiple levels; organized components and overall system are shown to be different perspectives on the same thing. Multilevel explanations of this kind yield understanding by putting together different perspectives on the target of inquiry. Explanatory models that represent diverse parts 'working together,' and thereby constituting higher levels of organization, are both an outcome of collaborative practices and built around collaborative concepts.

I conclude by discussing the wider implications of this account. The features I've identified as central to this mode of explanation are just those that characterize successful collaborations across scientific disciplines; i.e., attempts to build explanatory models that integrate diverse ideas about explanation. This approach applies reflexively to philosophical accounts of explanation as well. Philosophers can, and should, ask how different kinds of explanation might mutually support one another in more inclusive efforts toward understanding. By conceiving explanation (both practice and product) in collaborative terms, philosophy of science can make diversity of explanatory models a resource for engaging science, as problems and projects become increasingly interdisciplinary. I also discuss how this collaborative approach relates to recent work on scientific understanding (e.g., Eronen and van Riel, 2015).

References

Bratman, M (2014) Shared Agency: A Planning Theory of Acting Together. Oxford: Oxford University Press.

Craver, C (2007) Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience. Oxford: Oxford University Press.

Eronon, M, and van Riel, R (2015) Understanding through modeling: the explanatory power of inadequate representation. Synthese 192, Special Issue, pp.3777-4008.

Huang, S (2011) Systems biology of stem cells: Three useful perspectives to help overcome the paradigm of linear pathways. Philosophical Transactions of the Royal Society, Series B, 366, 2247-2259.

Jacob, F, and Monod, J (1961) Genetic regulatory mechanisms in the synthesis of proteins. Journal of Molecular Biology 3: 318-356.

Jaeger, J, and Crombach, A (2012) Life's attractors: Understanding developmental systems through reverse engineering and in silico evolution. In O. Soyer (Ed.), Evolutionary Systems Biology (pp. 93-119). London: Springer.

Explanatory Agnosticism in Translational Medicine

Spencer Hey
Harvard Medical School
United States
James Overton
Knocean
Canada

A randomized controlled trial (RCT) is justified in part by a scientific explanation of the expected effectiveness of the experimental intervention. Published studies invariably include some such explanation, and those explanations guide clinicians as they make treatment recommendations for their patients. However RCTs are agnostic about explanation: an RCT can provide a reliable answer to the clinical question—Which is the better therapy to prescribe in practice for a given clinical condition?—without needing to explain why that therapy is better. According to some commentators, this explanatory agnosticism is a virtue of the current research enterprise, but it raises questions about the function and value of explanations in translational medicine.

In this essay, we examine the comprehensive drug development portfolio for the tyrosine kinase inhibitor, sunitinib, as a case study in the function and value of scientific explanations across a translational research program. Sunitinib has been tested in over one hundred clinical trials involving over 9,000 patient-subjects and many different types of cancer malignancies. To date, the drug has been approved for treating three types of cancer—pancreatic neuroendocrine tumor, renal cell carcinoma, and gastrointestinal stromal tumor. However, it has been tested against nearly 30 other types of malignancy—the vast majority of which do not respond to the drug.

We systematically analyzed the text from a random sampling of sunitinib trial reports to illustrate how or whether the theoretical explanation for sunitinib's effectiveness—and the justification for exposing human subjects to the harms of research—evolved as the drug failed to show efficacy against one malignancy after another. Contrary to our expectations, we found that the theoretical explanation and justification for testing sunitinib grew weaker over time. That is, as the accumulating body of evidence grew, less information was used to guide the research program and design future studies.

We conclude that there is philosophical tension surrounding the proper role and function of scientific explanation in translational research. If drug development were still predominantly an empirically driven enterprise, then the failure to update the proposed explanation in light of the evidence would not be problematic. But as the research enterprise becomes increasingly theory-driven, it is no longer acceptable for the scientific community to remain agnostic about the "true" explanation for the accumulating body of data. Indeed, unless the theoretical explanations are responsive to the evolution of evidence, human subjects will be exposed to excessive and avoidable harms and their burdens will not be sufficiently redeemed by gains in generalizable scientific knowledge.

From "Personalized" to "Precision" Psychiatry: What's in a Name?

Kathryn Tabb
Columbia University
United States

Over the past decade the hegemony of the Diagnostic and Statistical Manual of Mental Disorders (DSM) has increasingly been challenged by a new approach to identifying targets for psychiatric research: the National Institute of Mental Health's Research Domain Criteria project (RDoC). RDoC encourages researchers to gather patient populations for psychiatric studies on the basis of biomarkers rather than the signs and symptoms traditionally used for diagnosis. Thomas Insel, the erstwhile director of the NIMH who spearheaded the RDoC initiative, has described it as the product of a new approach that has been called, variously, "personalized" or "precision" medicine. The term "personalized medicine" was introduced in the late 1990s to signify medical care that was customized to the individual pathology of the patient, an important revision of, it was argued, traditional "one size fits all" approaches. Over the last decade, however, there has been a shift in the ambitions of the initiative, reflected by the White House's recent adoption of the language of "precision" for their $215-million-dollar investment in data-driven medical research. Precision medicine (PM) aims to revolutionize diagnostic and treatment practices through the employment of genetic, molecular, or cellular signatures to better specify and target discrete diseases and conditions. Individual variation at the level of the person is considered at best an inefficient approach to obtaining such precision, if not an irrelevant one. Instead, the aim is to use statistical analyses of enormous data sets to identify types of diseases that are more accurate than contemporary symptomologies.

The RDoC initiative has been criticized for reducing psychiatric distress to underlying biomedical dysfunctions at the expense of the patient, whose phenomenological experience has traditionally been the target of psychiatric interventions. Insel is happy to bite this bullet, calling psychiatry nothing more than "applied neuroscience." In this paper, I assess the potential for a precision medicine approach to improve treatment outcomes for mentally ill patients. I first show the desperate need for what might meaningfully be called personalized medical care, based on three factors: the unreliability of current diagnostic categories for predicting treatment response, the central relevance of environmental factors to onset and outcome among the mentally ill, and the uselessness of current diagnostic categories for guiding basic science research into the causal mechanisms underlying mental disorder. I next argue, however, that the vision of precise medical care that is starting to be successfully realized in other areas of medicine—such as the use of genetic information in oncology, for example—is not yet appropriate for psychiatry. Lower levels of analysis have not proved to be helpful in understanding what causes specific cases of mental disorder, or in suggesting possible ways forward for intervention. While it may well be that genetics, neuroscience, and other basic sciences ultimately can make valuable contributions to psychiatry, given the limited resources available for psychiatric research, the recent turn towards precision psychiatry comes at a cost. I conclude that while scientific investigation into these lower levels—such as genes and circuits—may prove to pay dividends later, policy makers must carefully assess whether the methods of PM are promising enough to warrant the abandonment of research into other modes of care, such as those that aim to treat the whole person.

From Neural Engineering to Scientific Understanding

Maria Serban
University of Copenhagen, Department of Media, Cognition, and Communications
Denmark

One challenge is at the core of cognitive neuroscience: to provide an integrated account of what neural systems do and how they do it, i.e., how are cognitive functions implemented. Despite this foundational concern for an integrated account of neurocognitive phenomena, the autonomy of the functional (or computational) level of description and explanation from the implementational level has been one of the cornerstones of research in cognitive neuroscience. In contrast to more traditional theoretical frameworks, neural engineering approaches challenge this divide, aiming to show that an integrated account of cognitive function and neural implementation can be achieved by using the right methodological tools. Modern control theory is offered as a candidate framework for achieving the explanatory aims of cognitive neuroscience. The framework combines the powerful mathematical toolbox of dynamic systems theory with the conceptually unifying categories of control variables and design principles for describing the interactions between a neural system and its environment as well as the quantitative relations between physically measurable variables of the system. Control-theoretic approaches rely on a broadly mechanistic model of scientific explanation, claiming that adequate quantitative descriptions of neurocognitive systems must support interventions that lead to novel predictions and explanations of how specific neurobiological mechanisms produce and support observable cognitive patterns and behaviours.

This paper analyzes the conceptual underpinnings of control-theoretic accounts of neurocognitive phenomena. Drawing on two successful applications of modern control theory in cognitive neuroscience, I evaluate two broad claims made by advocates of this position within systems neuroscience. First, the framework promises to discover and validate both general principles about brain functioning and organization and detailed-informational accounts of particular target neurocognitive systems and tasks. How do control-theoretic tools and principles achieve this goal and how do these strategies differ from mechanistic ones? Second, modern control theory has been put forward as a unifying theoretical framework for cognitive neuroscience which will (eventually) substitute the fragmented picture that is currently dominating the field. Can the local explanatory and empirical successes of control-theoretic accounts support this bold theoretical expectation? What epistemic features of this approach might licence its unifying role with respect to neurocognitive phenomena?

Focusing on the unificatory aims of engineering approaches in cognitive neuroscience allows us to unpack how scientific knowledge and understanding of neurocognitive phenomena (or systems) is built through the interactions that occur at the boundary between various disciplines (in this case, engineering, neurobiology, and cognitive psychology). Engineering-based modeling in cognitive neuroscience emphasizes the link between understanding how a system works and being able to construct a system that exhibits the same (or a relevantly similar) behaviour. This supports a dynamic-pragmatist view of scientific understanding as a process of constructing a comprehensive body of information that is grounded in facts and enables non-trivial inferences and interventions on the systems being investigated. I argue that this view is responsive and reflective of the epistemic features of engineering approaches in cognitive neuroscience.

The Functions of Repertoires in Scientific Research

Rachel Ankeny
University of Adelaide
Australia

Sabina Leonelli
University of Exeter
United Kingdom

A vast body of scholarship in the historical and social studies of science underscores the critical role of collaboration in the development of scientific knowledge, and developed the concepts of platform (Peter Keating and Alberto Cambrosio) and regime (Michel Foucault, Dominique Pestre) to capture these aspects. It is evident that the organization of research communities, and the ways in which they are constructed and managed through platforms and regimes, has a major impact on the quality and types of outputs that are produced. Hence the organization of research is a topic of crucial interest to philosophers of science. Some philosophers have analyzed the mechanisms that underlie collaborative work, focusing particularly on the division of labor involved, the typologies and patterns of epistemic dependence characterizing interdisciplinary work which include group learning, negotiation, and integration, and the use of theories, models, and tools as conduits to communication and integration. However, there is relatively limited philosophical work on what constitutes a research community, how communities change over time, and how the development of collaborations relates to the production of knowledge within the various environments in which scientific research occurs. Existing characterizations of communities in terms of shared theories, which in turn constitute a discipline or field, have greatly enhanced our understanding of the dynamics of scientific change and how to conceptualize research 'progress' (e.g., Thomas Kuhn, Stephen Toulmin, Dudley Shapere, Lindley Darden and Nancy Maull). However, these accounts have limited value for making sense of multidisciplinary efforts, where successful collaboration involves the harmonious merging of different types of expertise and disciplinary training. They also fail to account for the critical roles played by social, political, and economic factors in the development and outcomes of scientific research practices; and do not engage with the conceptual apparatus developed within the social sciences to that effect. In this paper, we propose a philosophical framework for analyzing the emergence, development, and evolution of collaborations in science that we believe will facilitate philosophical exploration of critical questions around the functioning, flexibility, durability, and longevity of research communities, in fruitful dialogue with relevant social science discussions. We are interested in tracing the material, social, and epistemic conditions under which individuals are able to join together to perform projects and achieve common goals, in ways that are robust over time despite environmental and other types of changes, and can be transferred to and learnt by other communities interested in similar goals. We refer to these conditions, which include specific ensembles of skills and behaviors as well as related methods, materials, resources, participants, and infrastructures, as repertoires. We argue that the creation or adoption of one or more repertoires has a strong influence on the identity, boundaries, and practices of research communities, whether their individual members explicitly recognize this impact or not. At the same time, not all research communities have a repertoire, and many creative and innovative scientific initiatives grow at the margins of, or in outright opposition to, the most long-lived repertoires. This argument builds on empirical insights on practices within contemporary research communities in the experimental life sciences, as well as cases drawn from social and historical studies of other sciences including physics, psychology, and medicine.

Fuzzy Practice: Homologizing Liminally Comparative Parts

Catherine Kendig
Missouri Western State University
United States

Defining characters and homologues as dissociable modular units of phenotypic evolution (Wagner 1996, 2014) assumes that these units can be identified and compared independently—as a well-bounded part. I suggest homologues are not modules in this sense, because they are the kinds of things that may have fuzzy spatial or ontogenetic boundaries or be liminally comparative. In some cases, they may be better described as either "mosaics" or "clusters of set-aside cells" (Minelli 2003: 252). There has been, and continues to be, sustained debate over the meaning of homology thinking (Brigandt 2007; Ereshefsky 2012; Griffiths 2007; Hall 1992, 2003, 2012; Love 2007; Minelli 1996, 2003, Minelli & Pradeu 2014, Wagner 2014). In particular, what is the nature of the correspondence relation that it implies (e.g. whether homology thinking implies an all-or-nothing relation or whether it admits of degrees), and what are the units of comparison (e.g. whole organisms, traits as morphological outcomes, behavioural activities, biochemical mechanisms, developmental processes, or certain properties of traits) that are the objects or relata to which it refers. Comparative biology in general, and homology in particular, compares corresponding traits of organisms to reconstruct their common origins in order to identify organisms as members of the same or different species, or the same or different lineages.

My aim is to discuss homology thinking from an explicitly practice-focused approach rather than one that is concept-driven. This practice approach is taken to be a precondition for knowledge-making and concept-making rather than the result of it. I do so by investigating fuzzily bounded developmental and spatial organismal parts with regard to the delimination of the organismal segments in plants, e.g. leaf, stem, root and in the tagmata of animals, e.g. head, thorax, and abdomen.

"Root," "leaf," and "stem" are the names for simplified bodily organs of a vascular plant. They demarcate the plant in terms of their different structural features. Although these are often treated as organs with sharp spatial and developmental boundaries they often overlap with one another. This has been recognized in the study of botanicals, with the result that the fuzzy nature of these organs is now spelled out in the names used by botanists: root, root/stem mosaics, stem, stem/leaf mosaics, and leaf (Fisher 2002).

I suggest that these kinds of transient or fuzzily-bounded developmental organismal kinds are ubiquitous. This kind of indeterminacy can be seen in the formation of the leaf-stem organs of the bladderworts, Utricularia, and the root-shoot organs of the perennial river weed Podostemaceae especially Podostemum ceratophyllum (Ameka et al. 2003), and the leaf-shoots of Asparagaceae (Sattler 1984). A different kind of indeterminacy of part-hood is also present in holobionts such as those of photobionts and mycobionts in lichens (Nash 2008).

If only strictly bounded modules are possible units of comparison, homologizing these fuzzy intermediates would not be possible. If this is possible, the ability to homologize fuzzily individuated parts may introduce a practice-based fuzzy epistemology able to resolve parallel problems of vagueness in other fields.

Works cited

Ameka, K. G. et al. (2003) Developmental morphology of Ledermanniella bowlingii (Podostemaceae) from Ghana. Plant Systematics and Evolution 237: 165-183.

Brigandt, I. (2007) Typology now: homology and developmental constraints explain evolvability. Biology & Philosophy 22 (5): 709-725

Ereshefsky, M. (2012) Homology thinking. Biology & Philosophy 27 (3):381-400.

Fisher, J. (2002) Indeterminate leaves of Chisocheton (Meliaceae): survey of structure and development. Botanical Journal of the Linnean Society 139 (2): 207-221.

Griffiths, P. (2007) The phenomena of homology. Biology & Philosophy 22 (5): 709-725.

Hall, B. (1992) Evolutionary Developmental Biology. London: Chapman and Hall.

Hall, B. (2003) Descent with modification: the unity underlying homology and homoplasy as seen through an analysis of development and evolution. Biological Reviews 78 (3): 409-433.
Hall, B. (2012) Parallelism, deep homology, and evo-devo. Evolution & Development 14 (1): 29-33.
Love, A.C. (2007) Functional homology and homology of function: biological concepts and philosophical consequences. Biology & Philosophy 22:691-708.
Minelli, A. (1996) Segments, body regions and the control of development through time. In M. Ghiselin and G. Pinna (eds.) New Perspectives on the History of Life. San Francisco: California Academy of Sciences.
Minelli, A. (2003) The Development of Animal Form. Cambridge: Cambridge University Press.
Minelli, A., Pradeu, T. (2014) Towards a theory of development. Oxford: Oxford University Press.
Nash, T. (2008) Lichen Biology. Cambridge: Cambridge University Press.
Sattler, R. (1984) Homology—a continuing challenge. Systematic Biology 9: 382-393.
Wagner, G. (1996) Homologues, Natural Kinds and the Evolution of Modularity. American Zoologist 36:36-43.
Wagner, G. (2014) Homology, Genes, and Evolutionary Innovation. Princeton: Princeton University Press.

How and How Not to Be Whiggish About 'Phlogiston'

Jonathon Hricko
National Yang Ming University
Taiwan

Old scientific texts can pose challenges to our understanding because they contain out-of-date theoretical terms like 'phlogiston.' Understanding such texts requires us to assign referents to these terms (where reference failure is a possible reference assignment). My aim in this paper is to defend one way of making such retrospective reference assignments. I develop a theory of reference according to which theoretical terms refer by means of the operations that scientists use to identify their putative referents. I argue that such a theory can avoid reference assignments that are Whiggish, in the sense that they distort the past in various anachronistic ways. And I make use of the example of 'phlogiston' in order to do so.

I begin by drawing on Jardine's (2000) way of distinguishing vicious from non-vicious anachronism in order to distinguish between what I call Whiggish and non-Whiggish reference assignments. Referring to unobservable entities requires scientists to have rather sophisticated beliefs and/or abilities. Whiggish reference assignments are viciously anachronistic, in that they require us to attribute to past scientists beliefs and/or abilities that they lacked. In contrast, non-Whiggish reference assignments do not. Such reference assignments can be non-viciously anachronistic, if they specify the referent of a term using modern language unavailable to past scientists. Otherwise, such reference assignments are not anachronistic.

I go on to consider two retrospective reference assignments to the term 'phlogiston' as used in the late eighteenth century. On the first, 'phlogiston' sometimes referred to hydrogen. I argue that this reference assignment is not Whiggish, on the grounds that it is not anachronistic, since a number of chemists at the time identified phlogiston with hydrogen. On the second, 'phlogiston' sometimes referred to free electrons. I argue that this reference assignment is Whiggish, on the grounds that scientists did not have the beliefs and abilities required to refer to electrons until the late nineteenth century.

Finally, I introduce my non-Whiggish theory of reference, according to which theoretical terms refer by means of the operations that scientists use to identify their putative referents. In order to do so, I draw on Chang's (2009, 2011) notion of operational meaning. I propose that we can avoid Whiggish reference assignments by first examining the operations that past scientists associated with a theoretical term t, and then looking at current science to see what, if anything, those operations are sufficient for identifying. If they are sufficient for identifying some entity x in our present-day ontology, then we can non-Whiggishly assign x as the referent of t. I argue that this way of making retrospective reference assignments yields the non-Whiggish result that 'phlogiston' sometimes referred to hydrogen, and avoids the Whiggish result that it sometimes referred to free electrons.

References

Chang, H. (2009). Operationalism. In E. Zalta (Ed.), Stanford Encyclopedia of Philosophy, Fall 2009 Edition, http://plato.stanford.edu/archives/ fall2009/entries/operationalism
Chang, H. (2011). The persistence of epistemic objects through scientific change. Erkenntnis, 75(3), 413-429.
Jardine, N. (2000). Uses and abuses of anachronism in the history of the sciences. History of Science, 38(121), 251-270.

Hypothesis-Testing, Exploratory Experiments, and the Space in Between

Emily C. Parke
University of Auckland
New Zealand

The classic picture of scientific experiments makes them subservient to theories: Theories are central in science, theories give rise to hypotheses, and experiments are in the business of testing hypotheses. A more recent pocket of literature, pioneered by Franklin-Hall (2005) and Steinle (1997), has highlighted that not all experiments fit this model; they can also be exploratory. While these are two important roles for experimentation, plenty of experimental work in practice fails to fit both the classic hypothesis-testing paradigm, and the paradigm of exploratory experiments as characterised in the literature. This talk develops a characterisation of the under-examined conceptual space in between the two.

As a starting point for this account, I focus on examples from experimental evolution, where populations of organisms are propagated in the laboratory as a means to study evolution in real time. These cases do not fit nicely into either category of experiment, and the ways in which they fail to fit are revealing. I propose a new characterisation of a key aspect of the wider conceptual space of experimental inquiry: the relationship between experiments and theories. The difference between hypothesis-testing and exploratory experiments has been discussed in terms of the role theory plays; the former are designated theory-driven, while the latter are (only) theory-informed. Characterisations of both kinds of experiment tend to suggest that a body of theory plays one role, either driving or informing, in experiments. My updated view accounts for the many roles that multiple theories can play, in practice, in an experiment's origin, direction, and ends.

This discussion draws on my research experience in a biology laboratory studying long-term evolution of E. coli. Against this background, a secondary thread in the paper is exploring a common incongruity between how research happens in practice, and how it is presented in grant proposals and submissions for publication. The latter put pressure on scientists to describe their work in terms of the hypothesis-testing paradigm, even when the actual context in which it took place was more exploratory, or somewhere in the middle ground characterised here. Developing a more accurate portrayal of that middle ground is a step towards addressing the common and problematic perception that experimental work that does not fit the hypothesis-testing model is less legitimate.

References

Franklin-Hall, L. (2005). Exploratory experiments. Philosophy of Science, 72(5), 888-899.
Steinle, F. (1997). Entering new fields: Exploratory uses of experimentation. Philosophy of Science, 64, S65-S74.

Ideals in Practice: Discovery and Decision-Making in Biomedicine (Symposium)

Sophie van Baalen
University of Twente
Netherlands

Medical science and practice are often steered by powerful "catchphrases" that seem self-evident, but at second sight are conceptually inconsistent, unclear or misguiding, such as 'evidence-based medicine', 'evidence-based clinical practice' and 'personalized medicine'. Philosophy can contribute to the practice of medicine and biomedical research by explicating the underlying assumptions and conceptual frameworks of such notions, identifying conflicting ideas and analyzing their fruitfulness as guiding concepts for these practices. For example, philosophers of science have criticized the epistemology of 'evidence-based medicine' for being based on a narrow view of science, focusing on quantitative, clinical evidence and rule-following instead of basic science, theories and judgments. Likewise, the notion of 'personalized medicine' has recently gained a lot of attention, but was criticized for overly focusing on genetics and (molecular) biomarkers and the possibility of proving the efficiency of personal treatments was questioned. Hence, philosophy of science in practice has something to contribute to medicine, in a wide range of areas including biomedical research, clinical practice, psychiatry and medical decision-making, by analyzing different ways of knowing in medicine.

In this symposium, ways of knowing in medical practice and science are explored. The first two papers focus on the notion of 'personalized medicine', which has gained both much approval and skepticism. In the first paper, a recent attempt to demarcate a precise definition of personalized medicine is criticized for being too limited. Rather, personalized medicine must be seen as a negotiable ideal that accommodates an aspiration for a more personal approach in health care and a personalized knowledge base. In psychiatry, diagnosis was long guided by the Diagnostic and Statistical Manual of Mental Disorders (DSM) defining mental disease at the level of the person, but this has recently been challenged by 'personalized' or 'precision' medicine, which aims to define mental diseases at a molecular level. The second paper argues that, although there is a desperate need for personalized treatment of psychiatric diseases, the methods of precision medicine do not warrant the abandonment of other approaches to psychiatric care.

In the last two papers, the epistemology of clinical decision making is analyzed. First, it is argued that medical decision-making is a form of social knowing, performed by an assemblage of people and instruments. A detailed case study of a pulmonary disease demonstrates the pivotal role of trust in these social process of knowing and how trust is mediated by images. The last paper presents an epistemology of critical care medicine in terms of distributed cognition to show that two types of cognition in medical practice, i.e. clinical judgment and treatment decision-making, can be fruitfully characterized by that framework.

In summary, this symposium shows how a careful study of the concepts that guide medicine in relation to detailed study of actual practices will lead to a more fruitful interpretation of knowing in medical practice and biomedical research.

Session papers:

1. Personalised Medicine: a negotiable ideal without precise definition Giovanni De Grandis and Vidar Halgunset
2. From "Personalized" to "Precision" Psychiatry: What's in a Name? Kathryn Tabb
3. The socio-technical epistemology of clinical decision-making Sophie van Baalen and Annamaria Carusi
4. Distributed Cognition in Critical Care Medicine Thomas Cunningham

The Identification of Scientific Phenomena: Lessons from the Life Sciences

David Colaço
University of Pittsburgh
United States

A cursory examination of publications in the life sciences suggests that researchers aim to relate the results of their experiments to phenomena in the world; however, while one need not look far to find a scientist talking about their results in relation to a phenomenon of interest, comparatively little research in the philosophy of science discusses, let alone analyzes, the research practice that makes this possible. With few exceptions, philosophical descriptions of science take the researchers' identification of a phenomenon as a given. Given that there are a number of instances in the fields of psychology and neuroscience where successful identification is considered an achievement in and of itself, I aim to discuss the process by which researchers produce these kinds of achievements. The identification process is a scientific activity that best addresses certain kinds of questions; as such, I investigate the strategies that researchers employ to answer these questions. While identification complements theorization, explanation, and model building, it should be thought of as distinct from each of them.

I give a review of the character of scientific phenomena, drawing on the distinction between them and data. I present two principal features of phenomena: the stability feature and the repeatability feature. Then, I discuss the general features of the identification process, and introduce my two principal case studies: research on long-term potentiation in cellular neuroscience, and the investigation of reinforcement learning at the intersection of cognitive neuroscience and psychology. After introducing the cases, I outline the phases in the process, and describe the strategies that occur during each phase. In doing so, I describe how research starts, and how it can improve with additional tests. The interaction between the strategies and the products results in the process possessing a vindicatory character, which I relate to Hacking's self-vindicating laboratory sciences (1992). I further show that this interaction captures, in systematic terms, what William Bechtel and Robert Richardson call the reconstitution of phenomena (1993), and Carl Craver and Lindley Darden call the characterization of phenomena (2013). Throughout, I highlight the difference between the identification of phenomena and other scientific activities.

References

Bechtel, W., & Richardson, R. C. (1993). Discovering complexity: Decomposition and localization as strategies in scientific research. Cambridge, MA: MIT Press.

Craver, C. F., & Darden, L. (2013). In Search of Mechanisms: Discoveries across the life sciences. Chicago, IL: University of Chicago Press.

Hacking, I. (1992). The self-vindication of the laboratory sciences. In A. Pickering (ed.), Science as Practice and Culture (pp. 29-64). Chicago: University of Chicago Press.

Identifying Skills of Mathematical Modelling and Computational Simulations

Brandon Boesch
University of South Carolina
United States

Many philosophers have argued that we should think of models as epistemic tools which are used in scientific practice and that scientific representation is an activity (see, e.g. Knuuttila 2011; Suárez 2004; for an overview, see Boesch 2015). Under this pragmatic framework, an adequate understanding of the models, simulations, and representations will require that we pay attention to the particular activities and actions which are performed by scientists. An examination of these actions points to the capabilities that a scientist has that make her better able to use models, simulations, or representations for her aims, which should be thought of as skills or know-how. Such a skill (for scientific representation) was pointed to, but left undiscussed, by Mauricio Suárez (2004). Suvi Tala (2013) has also identified some skills associated with nanomodelling through an empirical study involving interviews. In this presentation, I will work to further develop these insights, drawing upon the philosophy of action, philosophy of science, and scientific literatures. I will first develop a theoretical framework of mental skills and then employ it to identify skills in real-world instances of modelling and computational simulation (of biological evolution). Discussion of skills often centers on physical skills, of the sort associated with athletes and artisans. The skills associated with these practices are more similar to the mental know-how which can be seen in the completion of mental puzzles (e.g. derivations in some logical system, etc.) and other mental activities (e.g. judging a diving competition, etc.). These skills show the same hallmarks that characterize physical skills: (A) the potential influence of some sort of natural propensity (e.g. some students are better at doing logic derivations than others); (B) a learned ability to mentally manipulate or pay attention to particular features (e.g., the diving judge will pay attention to the angle of the diver's feet); (C) an intrinsic reference to the broader practice (e.g. skillful derivations involve following the syntactical rules of the language). This framework is helpful in identifying skills in the mathematical modelling of biological evolution One of the skills is in manipulating these mathematical models to find stabilization points and equilibria, e.g. in models of rapid evolution and predator-prey systems. (Otto and Day 2007; Farkas and Morozov 2014). In numerical evaluations of mathematical models, parameters must be carefully selected for particular aims (Morozov et al. 2013). Another skill, managing general theoretical commitments, involves treating certain variables as only positive, being attentive to idealizations in the way of causal factors left out of the model, and being able to situate conclusions within the broader theoretical framework (Servedio et al. 2014). Computational simulations also demonstrate some unique skills. For example, in computational simulations of biological evolution through the use of programs like Avida and Mendel's Accountant, an important skill is in managing the underlying parameters of the program which deeply influence results (Nelson and Sanford 2013). A similar skill relates to understanding the relationship between these parameters and the broader theoretical framework of the conclusions of simulations (Lenski et al. 2003).

If You Go Too Far Western, You Can't Trust Your Data Anymore

Stephan Guttinger
Egenis, University of Exeter
United Kingdom

The question of how scientists ensure they have reliable data has gained considerable attention in philosophy of science in recent decades. The issue of how to distinguish fact from artefact has played a central role in this debate. Focusing mainly on the role of specific instruments (e.g. the electron microscope) and the manipulations required to use them to study specific entities (e.g. mesosomes), three different principles were proposed to be at work when scientists distinguish fact from artefact: robustness (Culp, 1994; 1995), reliable process reasoning (Hudson 1999; 2003) and social (i.e. non-epistemic) factors (Rasmussen 1993; 2001). There is currently no consensus on which of these principles is actually at work (or on how to unify the different accounts). Here I want to extend and modify this discussion by turning to a different example, namely the Far Western blot (FWB). The FWB is an in vitro assay used in the life sciences to detect protein-protein interactions. Interestingly, despite being used to analyse a key aspect of biological systems and despite being derived from the well-established Western blot, the FWB has never gained wide acceptance in the life sciences. The reason for this is the worry that the assay is prone to producing artefacts. Looking at the FWB will allow me to do three things: 1) it will allow me to move away from the study of a particular technique/artefact complex to the analysis of different types of experimentation and their respective links to artefact production. As I will show in the first part of this talk, the FWB belongs to a class of experiments that I will label Existence-Assessing (EA) assays. These assays are used to investigate the existence of entities (EA.e) or activities (EA.a) and are distinguished from assays that study the causal roles of entities or processes. The FWB falls into the category of EA.a assays. Western blots and microscope-based studies of the type used in the mesosome case fall into the class of EA.e assays. Using this distinction is important as it will allow me to show 2) that EA.a assays, such as the FWB, do not use any of the above-mentioned criteria to check for artefacts. Instead, EA.a assays depend on a system of positive and negative controls that are used to ensure the reliability of the data. Importantly, the main goal of this strategy is not to avoid the production of artefacts (as Hudson's idea of 'reliable processes' implies) but to be able to identify them. As I will show in the last part of my talk, this extended understanding of the struggle with artefacts in EA.a assays also has important implications for our understanding of EA.e assays. Working with a framework that foregrounds the crucial role of controls and the aim of seeing artefacts will 3) allow me to propose an alternative reading of the mesosome case and the three criteria originally proposed by Culp, Hudson and Rasmussen.

Individual vs. Collective Justification: What happens in research groups?

Haixin Dang
University of Pittsburgh
United States

Most philosophers who work on the problem of group justification tend to begin their analyses with the individual. I will argue that we need a more rich and complex account of how group justification is related to individual justification and not simply how it is an extension of the individual. In this paper, I will survey existing views on group justification and explain why I find them wanting. At the end of the paper, I will briefly sketch a positive account of group justification as inspired by the works of Staley (2004) and Rehg & Staley (2008) which force us to consider the importance of scientific practice in answering these questions.

Alvin Goldman (2014) explicitly gives an account of group justification which proceeds by extending the individual account: "Process reliabilism is traditionally advanced as an account of individual justification. With only a few tweakings, however, it can be extended to group justification" (21). Goldman's account of group justification is summative. It relies on the individuals' beliefs to be justified first. This view has several weaknesses; the most pressing being that it cannot in its current form account for degrees of justification. Other philosophers have come to endorse a non-summative account of justification. Another influential approach to group justification has been applying joint commitment to justification, which has been first introduced by Frederick Schmitt (1994) and expanded upon by Kristina Rolin (2010). While Goldman's view of group justification is primarily concerned with reliability, and therefore truth tracking, the joint commitment view is concerned with consistency.

I will argue that both the summative and non-summative accounts of justification are misguided. We have no reason to believe that group justification is merely an extension of individual justification in any straightforward sense; that is, we can simply carry over individual justification to the group. As the discursive dilemma has shown, individual rationality does not carry over to group aggregate rationality. The joint commitment account however does not tell us anything about what makes one group's justification better than another group's; it only suggests how group justification can come into being. Both these accounts actually tell us very little about the important aspects of group justification: What actually grounds group knowledge claims? This is particularly salient in scientific collaborations. When the ATLAS collaboration at CERN announce that the mass of the Higgs boson is ~125 GeV/c2, how were the 3,000+ physicists involved in the project come to be justified in their claim? In order to answer this question, we need to look beyond summative and non-summative accounts. Neither of these accounts are, even at the very least, descriptively adequate.

Rehg & Staley (2008) describe a collective justificatory process they call a "heterogeneous consensus." A heterogeneous consensus obtains when group members agree on the conclusion of the argument, but members disagree as to what evidence is relevant or how that evidence supports the conclusion. As scientists are under pressure to publish novel results, the capacity for scientists to negotiate between their personal beliefs and the group belief is crucial. Staley (2004) suggests not only that individual justification does not extend to group justification, but rather individual justification and group justification sometimes pull apart altogether. In cases of heterogeneous census, the reasons why one scientist endorses the group conclusion may not be the reasons made explicit in the group view, i.e. co-authored paper. Staley's account also shows the importance of looking into scientific practice for framework of group justification. Here, I will argue that a new account of group justification will need take seriously the negotiation between one's beliefs qua individual scientist and one's belief qua group member. I argue that group justification arises out of this negotiation and this is process is not reducible to the summative or non-summative views.

Goldman, Alvin. (2014). Social Process Reliabilism: Solving Justification Problems in Collective Epistemology. In Jennifer Lackey (ed.), Essays in Collective Epistemology. Oxford University Press.

Bratman, M. E. (2014). Shared agency: A planning theory of acting together. Oxford University Press.

Rehg, W., & Staley, K. (2008). The CDF collaboration and argumentation theory: The role of process in objective knowledge. Perspectives on science, 16(1), 1-25.

Rolin, K. (2010). Group justification in science. Episteme, 7(03), 215-231.

Schmitt, Frederick F. (1995). "The Justification of Group Beliefs." In F. F. Schmitt (ed.), Socializing Epistemology: The Social Dimensions of Knowledge, pp. 257-87. Rowman & Littlefield Publishers.

Staley, K. W. (2004). The evidence for the top quark: objectivity and bias in collaborative experimentation. Cambridge University Press.

Individualized Medicine and the Epistemological Challenges of Data Integration

Megan Delehanty
Department of Philosophy, University of Calgary
Canada

At this point, most of the philosophical work that engages with individualized medicine (IM) treats IM as simply genomic medicine, rather than directly engaging with the importance and difficulties associated with bringing in multiple other "omics" data sets (plus pharmacokinetic and pharmacodynamic data). This may be appropriate for work on legal and ethical questions associated with IM since it reflects the current state of the science/practice. However, from an epistemological standpoint, it misrepresents the actual difficulties faced in evaluation of evidence. It is recognized that the information we can acquire from this sort of single data-type approach is limited, but the tools to integrate multiple data types in order to get a more complete understanding of complex biological traits and phenomena are still very much works in progress themselves. Moreover, the kinds of meta-dimensional analysis and multi-staged analysis that are needed to produce something like a total (molecular) evidence picture are importantly different than the methods that are suitable for smaller, more uniform data sets.

In this paper, I will argue that this presents a challenge to the received view of evidence in evidence-based medicine (EBM) as well as to those who argue that by recognizing more fully the evidential role that mechanism-based information plays in EBM we can produce an adequate epistemological account.These tools for data integration a present epistemic challenges that are not present within randomized control trials, cohort studies, cross-sectional studies, or the sort of basic science that is often characterized by the search for mechanisms. The philosophical approach of finding ways to integrate mechanisms into an EBM-like hierarchy will not succeed once we recognize that the problem of data integration is not essentially the elucidation of very complex mechanisms. This requires that we focus our attention on the evaluation and analysis of data integration, and, ultimately, on how these tools can be incorporated into a total evidence approach for medical decision-making.

Induction and Evidence in Failure Analysis

Inge De Bal
Ghent University
Belgium

This paper is about induction in failure analysis and the evidence needed to warrant it. Failure analysis is a part of engineering science that deals with the analysis of (causes of) failures in artefacts. When an artefact breaks down, failure analysts study the specific circumstances that lead to this failure. For instance, in Creep failure of a spray drier, Paul Carter investigates the collapse of one specific spray drier (an artefact often used in mines to dry slurry by means of hot gas) “which had been in service for nearly 20 years” (Jones, p. 73).

Although failure analysts start from case studies, they do not simply want to explain what happened in this specific situation. They also aim to produce general knowledge that helps to prevent similar problems in the future. As Petroski says:

> When failures do occur, engineers necessarily want to learn the causes. Understanding of the reason for repeated failures […] typically leads to a redesigned product.(p.13)

In other words, they look for ways to use the knowledge about causal relations in one specific context, to draw conclusions regarding causal relations in other contexts. These contexts range from other instances of the same artefact, over similar artefacts, to very distinct artefacts. One of their goals is furthermore to find ways to alter design plans. To achieve this, failure analysts frequently use induction: they proceed from causal claims regarding (failure in) a specific artefact to causal claims regarding (other) types of artefacts. Unfortunately, they are often unspecific regarding the domain of their conclusion. Yet, the level of generality determines how strong the evidence for their conclusion needs to be. If they use one artefact failure to formulate conclusions or recommendations regarding all artefacts of a certain class (e.g. all spray driers), analysts will need stronger evidence to warrant their claims than if their conclusion only applies to other artefacts of the same type (e.g. other spray driers of the same type).

In this paper, I investigate the required type(s) and strength of evidence to warrant the conclusions of inductive reasoning processes in failure analysis. Using several case studies, I reconstruct the relevant reasoning processes and present an account of what this type of induction entails and how we can characterize it. Based on these findings, I explore what evidence failure analysts (should) put forward to warrant their conclusions. I specifically focus on the evidential role of knowledge of the underlying mechanism. For this part, I utilize Daniel Steel’s Comparative Process Tracing (CPT). This is a procedure Steel developed for extrapolating a mechanism found in a base population (e.g. mice) to a target population (e.g. humans) in the context of mechanistic evidence in the biomedical and social sciences (p.89). CTP depends on “knowledge of likely (dis)similarities between target and base model” (ibid., p88). I adapt Steel’s framework to fit the specific nature of failure analysis, so that CTP can function as a starting point to analyse the mechanistic part of evidence for induction in failure analysis.

References

Jones, D.R.H., ed. 2001. Failure Analysis Case Studies II. 1 edition. Amsterdam; New York: Pergamon.
Petroski, Henry. 2001. “Success and Failure in Engineering.” Practical Failure Analysis 1 (5): 8-15.
Steel, Daniel. 2007. Across the Boundaries: Extrapolation in Biology and Social Science. Oxford; New York: Oxford University Press.

Inductive and Other Epistemic Risks in Research: Conceptual Explorations (Symposium)

Torsten Wilholt
Leibniz Universität Hannover
Germany

The concept of inductive risk, introduced by Carl Hempel and revitalized by Heather Douglas, has played a crucial role in debates over science and values of the past fifteen years. Most significantly, it has been used to support the inevitability of value judgments at all stages of scientific research, as philosophers have pointed out that decisions at all kinds of junctures in the research process require an assessment of the acceptable level of risk of getting it wrong. The papers in this session explore different possibilities of developing the philosophical reflection on inductive and other epistemic risks further. They propose to make distinctions between different kinds of risk involved in inquiry and they seek to expand the philosophical use of risk concepts as tools of analysis in philosophy of science. Justin Biddle and Rebecca Kukla do so by stressing the fact that many of the epistemic risks that are usually treated under the rubric of "inductive risk" do not immediately involve a risky inductive step from evidence to conclusion. They argue that inductive risk should be regarded as a special case of a broader category that they term "phronetic risk" and develop a typology of research-related risks. Doing so helps to recognize how phronetic risks pervade practical rationality and how values often operate on the institutional rather than the individual level. Matt Brown will focus on how epistemic risks in research should be managed responsibly. While risk management requires value based decisions within the research process, a tenable account of the role of values in science must show how non-epistemic values can exert their influence without letting inquiry collapse into wishful thinking. Brown points out that the problem of wishful thinking is equally present in cases of practical reason as in theoretical reasoning. He argues that the problem can thus not be solved by relying on a dichotomy between science and practical reason. He proposes a form of pragmatism as a solution. Saana Jukola discusses the case of risks involved in the production of medical knowledge, focusing on meta-analyses. When it comes to identifying epistemic risks associated with meta-analyses, procedural accounts of objectivity focus attention on those decisions in the process that require individual judgment. Jukola argues that individual judgment is in fact required to produce the kind of information that serves the epistemic and non-epistemic goals of medical science, and that procedural objectivity is both unattainable in practice and insufficient in principle as a basis for managing epistemic risks in medical science. Torsten Wilholt will explore how the prospects of using considerations about inductive and other risks in research to articulate an account of scientific objectivity that does without the value-free ideal but still permits criticizing cases of biased science as lacking objectivity. He proposes to regard objectivity as a relation of appropriateness between methodology and underlying cognitive interest. A cognitive interest in turn is characterized by a set of objectives that each represent dimensions of the search for truth (such as specificity, sensitivity and productivity). Wilholt discusses how a good match between methodology and characteristic cognitive interest underlies the trustworthiness of science as an epistemic enterprise.

Individual papers: Justin B. Biddle (Georgia Tech) and Rebecca Kukla (Georgetown): Expanding Epistemic Risk Matthew J. Brown (University of Texas at Dallas): Inductive Risk, Wishful Thinking, and Practical Reason Saana Jukola (Bielefeld University): Risk, judgments, and objectivity in medical research—evaluating the strengths and weaknesses of meta-analysis Torsten Wilholt (Leibniz Universität Hannover): Objectivity and Cognitive Interests

Inductive Risk, Wishful Thinking, and Practical Reason

Matthew J. Brown
University of Texas at Dallas
United States

The problem of wishful thinking is the key problem structuring contemporary discussions of values in science. If, as many now believe, the value-free ideal is untenable, the main criterion for an account of the role of values in science is resolving the problem of wishful thinking. Otherwise, our non-epistemic values or goals threaten to drive inquiry towards predetermined conclusions or bias inquiry to suit our desires. One major approach to values in science, championed by Heather Douglas, is the argument from inductive risk (Rudner, 1953; Douglas, 2000, 2009; recently expanded to "Epistemic Risk" by Biddle, 2015). On Douglas's account, we introduce values into science in order to responsibly manage inductive risks, while we prevent wishful thinking by limiting values to an indirect role in scientific judgments, whereas in ordinary practical reasoning (and some non-epistemic judgments in science). I argue that this approach is problematic, because it the way that it misrepresents practical reason and thus the nature of value judgments. First, I show that the problem of wishful thinking arises not only in science, or for theoretical reason more broadly, but also for cases of practical reason, i.e., for making decisions about how one ought to act. Even those domains of inference that are uncontroversially value-laden must ensure that they are not driven to a predetermined conclusion. In trying to balance my prudential interests with my moral obligations, it is wishful thinking to merely assume that the two always align. In trying to determine whether to lie or be honest, it is wishful thinking to assume that the consequences of honesty will always work out for the best. I will show that these cases share structure with the problematic cases of wishful thinking previously identified for values in science. Second, I argue that any theory of values in science that resolves the problem of wishful thinking by relying on a dichotomy between science and practical reason must fail, either because wishful thinking remains possible or because it misrepresents practical reasoning, and thus distorts the way values work. For example, Douglas's distinction between direct and indirect roles assumes that the direct role for values is unproblematic for cases of practical reason, while arguing that the indirect role prevents wishful thinking in cases of theoretical reason, such as hypothesis acceptance or data characterization. But this misrepresents the nature of practical reason, where wishful thinking is indeed still a problem, and implies that values lack any cognitive / epistemic status. I conclude by suggesting that we must adopt a form of pragmatism, according to which we must reject the dichotomy between theoretical ("pure") and practical reason. According to such an account, the structure of practical reason is primary, and theoretical reason, where reasonable, is a form of practical reason. This form of pragmatism accords well with the increasingly shared view amongst philosophers of science that science is a practice, and the contextual and pragmatic factors inherent in scientific practice are deeply bound up with the rationality of science.

Inferentialism in Biological Practice

Daniel Burnston
Tulane University
United States

I offer a practice-based argument for inferentialism about scientific representation. The vast majority of views on scientific representation are referentialist, in one form or other—they posit that scientific representations have their explanatory value in virtue of being similar to, isomorphic to, or producing a model of the system of interest. The inferentialism defended by Suarez is the lone non-referentialist view currently on offer. Suarez claims that referentialist views posit the wrong semantic properties for explaining representation in general, and thus that inferentialism is the best option. While these arguments are effective as far as they go, they do not connect inferentialism to scientific practice. My aim is to give a positive argument for inferentialism based on a case study from mammalian chronobiology. Chronobiologists study biological time. The case study I will discuss involves the representation of molecular "clock" systems within individual cells, wherein the interaction of gene regulators and promoters produces oscillations in molecular quantities over a 24 hour period.

I will focus on a particular form of representational practice: that of representing the same data set in distinct ways. In a series of papers, Ukai-Tadenuma and colleagues developed a variety of ways of representing relationships between promoters and gene products, specifically to show important phase relationships between their activity and quantities. These representations include data graphs, a vector model for representing phase relationships, and a network diagram for showing the casual sequence between interacting components. I argue that each of these representations contributes something ineliminable for explaining circadian rhythms in mammalian cells, and that inferentialism is the only account that can explain this ineliminability.

The main argument against referentialism is that the distinct representations play distinct explanatory roles that are not based on their having distinct referential roles. In fact, a vital aspect of the relative uses of the representations is that they represent the same relationships in distinct ways. Since referentialism ties explanatory role to a reference relation, it cannot accommodate distinct explanatory roles where there is referential overlap. I claim that this is true for any variety of referentialism.

I then offer a characterization of inferentialism that can explain the representational practice in the case study. I argue that a given representation should be characterized as a triple, comprising (1) the set of inferences entailed by the representation (absent defeaters), (2) the set of inferences compatible with, but not entailed by the representation, and (3) the set of inferences incompatible with the representation. Individual representations are developed to convey specific inferences which they entail—however, these limited entailments can only explain parts of the phenomenon. Alternative forms of representation are developed to convey explicit inferences which entail other aspects of the phenomenon, and are in the set of compatible inferences for the already established types. I claim that this view adequately explains both the development and the explanatory import of the representations in the case study. I thus offer both a defense and extension of inferentialism through a detailed analysis of practice.

In Pursuit of History (Symposium)

Chiara Ambrosio

One of the main aims of the Society for Philosophy of Science in Practice, outlined in the Society's mission statement, is to focus on scientific practices past and present and "provide a rationale for history-and-philosophy of science as an integrated discipline". This symposium takes this methodological aim very seriously. Our purpose is twofold: to recast historiographical questions in light of insights derived from the systematic philosophical study of scientific practice, and recast philosophical questions concerning scientific practice in light of insights deriving from the historiography of science and from scientific practice in its historical context.

We explore three complementary angles concerning the relation between history and philosophy of science in practice. In our first contribution Chiara Ambrosio explores a key figure at the intersection between HPS and the philosophy of science in practice: Charles S. Peirce. Peirce's name is famously associated to Pragmatism, a philosophical tradition built on the complementarity, and inseparability, of belief and action. In exploring Peirce's much neglected works in the history of science, Ambrosio aims to cast new light on the historicity that characterises his distinctive Pragmatist outlook. Drawing on unpublished manuscripts, and referring to the concrete case of Peirce's classification of the sciences, she shows that Peirce grounded his logical, epistemological and even metaphysical questions in the history of the very scientific fields in which he was a practitioner.

In our second contribution Matthew Lund explores the works of another landmark figure in HPS: Norwood Russell Hanson. Now that many have declared HPS to have been a failed institutional experiment (Shapin and Schaffer, Giere), Lund investigates Hanson's last thoughts on the symbiosis between the philosophy and history of science. Acknowledged for introducing the formulation of "theory-ladenness of observation", Hanson expressed what could be considered a "higher order" version of the theory-laden observation thesis: historical content can only be made intelligible by being organized and interpreted philosophically. At the same time, philosophy of science would be without content were it not about science and its history. As Hanson expressed it (along with Feigl and Lakatos), "Philosophy of science without history of science is empty; history of science without philosophy of science is blind." These insights are used as the general backdrop to articulate broader methodological questions arising from Hanson's contributions to the field of integrated HPS. A central concern for Hanson was the status of historical evidence in the context of philosophical claims concerning science. While concerned that inferences from the contingent (history) to the necessary (epistemology) could be read as an obvious instance of genetic fallacy, Hanson nevertheless stressed that historical understanding is not reducible to mere chronologies or neutral descriptions. Instead, history always comes in the form of structured arguments and explanations. A consequence of this is that the "genetic fallacy" would only properly apply to arguments that take history to be descriptive in the narrowest of senses. Lund aims to show that Hanson's views afford a reformulation of the relation between normative and descriptive accounts of science, one in which normative judgments are corrigible by empirical facts.

In our final contribution Jouni-Matti Kuukkanen places the debates anticipated by Hanson in the context of relatively more recent discussions in HPS. The received view on theory-ladenness considers it as a serious threat for the integration of historical and philosophical accounts of science, or at best as an unavoidable inconvenience that would weaken the support philosophers can obtain from historical evidence. Contrary to these views, Kuukkanen advocates a new comparative model of the historiography of science. Drawing on Imre Lakatos' idea of "historiographical research programmes", he proposes that epistemic values, such as consistency, coherence, empirical adequacy, fruitfulness, etc. can be used in comparisons even if alternative frameworks provide very different accounts with different explanatory factors and ontologies. While epistemic values are not expected to determine an absolute framework, Kuukkanen argues that they nevertheless enable a comparative ranking of alternative reconstructions.

The symposium will be closed by a commentary on the three papers by Hasok Chang.

The Interwovenness of Societal, Institutional and Epistemic Dimensions of Risk Controversies

Jeroen van der Sluijs
University of Bergen, Centre for the study of the sciences and humanities
Norway

Science and society face vehement controversies on man-made risks such as climate change, pollinator decline, or endocrine disruptors. The backgrounds and dynamics of such controversies until now are only partially understood. Understanding the role of deep scientific uncertainty in the dynamics of scientific controversies requires enhanced understanding of the interwovenness of its societal, institutional and epistemic dimensions. This paper proposes and illustrates a novel approach to the systematic analysis of scientific controversies on environmental and health risks. The approach systematically analyses deep uncertainty, conflicts of interests and institutional practices and their interactions. On the one hand, controversies stem from scientific uncertainty, imperfect understanding and scientific dissent, noticeable in a plurality of conflicting yet tenable interpretations of the same risks and even of the same data by scientists with different backgrounds and using different styles of reasoning. On the other hand, risk controversies are closely interwoven with societal conflicts (regarding interests, values, and stakes) and institutional settings, thus indicating on going co-productions at work. Societal conflicts co-shape the ways in which scientific knowledge is produced, communicated and used, and how uncertainty is dealt with. Institutional settings and regulatory frameworks co-shape the process and findings of official science-policy interface bodies. By shining light on the dynamics of scientific controversies from 3 different perspectives (discourse analysis, evidence characterization, institutional analysis) we seek to reveal how three factors (deep uncertainties; societal discourses; styles of reasoning in institutional practices) co-shape one another to produce typical patterns in the dynamics of scientific controversies. • How do scientific complexities & deep uncertainties shape the societal discourses? • How do scientific complexities & deep uncertainties shape the styles of reasoning within institutional practices • How do the styles of reasoning within institutional practices shape scientific complexities & deep uncertainties? • How do the styles of reasoning within institutional practices shape societal discourses? • How do societal discourses shape styles of reasoning within institutional practices? • How do societal discourses shape scientific complexity & deep uncertainty? The analysis is complemented with systematic critical appraisal of substantive, societal, and regulatory assumptions underlying the discord. Overall, this gives a deeper insight in the dynamics and patterns of scientific controversy on complex risk issues. The approach will be illustrated using the controversy on the risks of neonicotinoid insecticides for bees.

An Introduction to OptiSci and Optimization of Quantum (OptiQ) Phenomena

Benjamin Russell
Princeton University
United States

Herschel Rabitz
Princeton University
United States

Optimization is pervasive throughout the sciences where the goal is to optimize the outcome of physical and chemical processes by identifying optimal experimental variables (controls). In like fashion, nature stochastically optimizes species evolution. Collectively, optimization in the sciences occurs over vast length and time scales: from events at atomic scales to evolution over geological time scales. The term OptiSci refers to a collection of observations and the theoretical understanding of optimization across many areas of science. The overriding finding within wide ranging applications is that optimization is far easier than one might intuitively expect given the often very large number of variables. Underpinning this assessment is the concept of a fitness landscape: the objective being optimized as a function of the variables. Conventional intuition would suggest that landscapes are highly rough, thereby impeding the performance of efforts to find a truly optimal (globally maximal) solution. However, this natural expectation is in stark contrast to the observed ease of optimization in many areas of science encompassing phenomena viewed as nominally complex. This widely existing observational behavior suggests that a common foundation forms its basis. We will first present a summary of the data justifying the breadth of this conclusion spanning chemistry, evolution and quantum control. Assessing this common control behavior is the content of the recently introduced OptiSci concept, which takes the form of a theorem with particular assumptions in the cases encompassing control in quantum mechanics (OptiQ), chemistry (OptiChem) and natural evolution (OptiEvo). Each theorem, upon satisfaction of its assumptions, implies that the associated control landscapes should be free from local suboptimal optima (traps) lurking to halt algorithms seeking to locate the optimal variables or controls. The scope of OptiSci, as well as its methodological structure, presents a novel and striking example of unification across the sciences for philosophical consideration.

In quantum mechanics, OptiQ has theoretical as well as practical implications. In particular, for the creation of quantum technologies, quantum processes must often be manipulated using optimally tailored electromagnetic fields to achieve a desired goal. In this picture, the landscape is the measure of how well the quantum mechanical goal is achieved as a function of the control field variables. Several examples of such quantum control objectives will be presented. Seeking to optimize quantum phenomena was initially thought to be extremely difficult, and possibly even intractable by any means. However, experiments have shown that discovering an effective control is often far easier than the apparent complexity of the task, given the large number of control field variables typically involved. The fundamental reason for this serendipitous behavior in quantum systems is encapsulated in an OptiQ theorem, which will be presented as a basis to understand the favorable control landscape topology. The most important practical consequence of this revelation concerns its implications for the automated discovery of effective controls for a broad variety of quantum technologies. Moreover, at a fundamental level OptiQ forms a new perspective about the nature of the world we inhabit.

Making Ecological Value Make Sense

Justin Donhauser
UNIVERSITY AT BUFFALO and SUNY: BUFFALO STATE
United States

Value claims about ecological populations, communities, and systems appear everywhere in the literature put out by leading environmental advisory institutions. In this talk, I will clarify the content of such normatively significant ecological value claims in two main steps. I will first outline the conception of ecological entities, functionality, and properties, operative in the background of modern ecology. I'll then assess the implications of that background theory for how the many policies and management strategy directives that refer to such entities, functionality, and properties, can be most reasonably interpreted and operationalized. Specifically, I will consider the implications of my account for directives in the United Nations Framework Convention on Climate Change's (UNFCCC) policy framework (as of COP21 in Paris 2015) and for existing pieces of so-called "biodiversity legislation" in the U.S. and U.K.

Measuring Inequality: Race and Gender in Science

Robin O. Andreasen
Cognitive Science, University of Delaware
United States

Heather Doty
Mechanical Engineering, University of Delaware
United States

Quantitative research on faculty diversity is often undertaken with the intention of documenting and addressing inequities. This talk discusses methodological difficulties that arise in analyzing and interpreting diversity data for small subsamples such as faculty of color and women faculty in science and engineering. We will discuss cases where traditional statistical analysis might lead to error or disadvantage members of highly underrepresented groups, owing to their small sample size. In these cases, inequities may be allowed to persist. We introduce ideas to address this problem and hope that discussion will help generate new ideas.

Mechanism Discovery and Design Explanation: Where Causal Role Meets Biological Advantage Function

Dingmar van Eck
Ghent University
Belgium

Julie Mennes
Ghent University
Belgium

In the recent mechanisms literature, analyses of mechanism discovery focus heavily on intervention experiments geared towards the discovery of causal roles of mechanistic components. In this contribution we elaborate another complementary line of research that is also relevant for the discovery of mechanisms and which has gone (largely) unnoticed in the literature: biological research on the biological advantages of organismal traits. We argue that this research, which connects mechanistic information to advantageous traits, provides useful heuristics for mechanism discovery across species.

The reasoning strategies we consider fall under the rubric of ‘design explanation’, a specific type of functional explanation frequently employed in biology and often constructed alongside mechanistic explanation (Wouters 2013).

Whereas mechanistic explanations are invoked to explain how mechanisms, i.e., organized collections of entities and activities, produce phenomena, design explanations are procured to explain why organisms have certain traits, e.g., specific mechanisms, rather than alternative ones. Design explanations hinge on spelling out the biological advantages that are conferred on organisms by the presence of specific traits by relating these traits to other ones and specific environmental conditions (Wouters 2013).

Detailing the biological advantage of a certain trait, often, amounts to showing that the performance of a causal or biological role by a mechanism or component is efficient in certain environmental contexts and not in others, or that it is the only feasible way to perform that role in particular conditions. For instance, the use of the ‘two voice system’ by emperor penguins, in which the two parts of the vocal organ are used to produce two different sounds, is shown to be advantageous in the harsh Antarctic circumstances in which these animals live (Aubin et al. 2000). In these circumstances, the role of the two voice system in the detection of mates and young can be performed efficiently, since the sounds produced are highly stereotyped within individual penguins, and highly variable across individuals and, moreover, propagate far enough through densely packed emperor penguin colonies to allow mate recognition. Performance of this role in this manner is more efficient than other ways of performing the role of mate detection, such as through visual recognition or one voice recognition (in fact, these other ways of performing this role would not be viable in the conditions in which emperor penguins live.

Assessments of the biological advantages of traits, such as the two-voice system, typically focus on other traits, like observable behavioral patterns, and physical features of the environment, and the latter often get systematically manipulated in order to assess in which contexts the performance of a role offers a biological advantage. This research is quite different from the intervention experiments emphasized in the mechanism literature, since it is more than ‘standard testing’ for roles. It explains why a role is performed the way it is and, importantly, as we will argue, it gives a heuristic for mechanism discovery in those contexts in which similar roles are performed by similar species in similar environmental conditions, in which it is not known which system/mechanism/component fulfills the role in question. In such cases, one may (plausibly) infer (and subsequently test further) the underlying mechanism in question based on information about the contexts in which the performance of a biological role is advantageous. We discuss a case study on prey perception of ‘trawling’ bats (Siemers et al. 2001) to illustrate this discovery heuristic.

Aubin, T., Jouventin, P., & Hildebrand, C. (2000). Penguins use the two-voice system to recognize each other. Proceedings Royal Society London, 267: 1081-1087.

Siemers, B., Stilz, P. & Schnitzler H. (2001). The acoustic advantage of hunting at low heights above water: behavioural experiments on the European ‘trawling’ bats Myotis capaccinii, M. dasycneme and M. daubentonii. The Journal of Experimental Biology, 204: 3843-3854.

Wouters, A. (2013). Biology’s functional perspective: roles, advantages, and organization. In: (Ed.: K. Kampourakis) The philosophy of biology: a companion for educators. Dordrecht: Springer: 455-486.

Mechanisms as Modal Patterns

Joseph Rouse
Wesleyan University
United States

Philosophical discussions of mechanisms and mechanistic explanation (e.g., Bechtel 2006; Bechtel and Abrahamsen 2005; Craver 2007; Craver and Darden 2014; Darden 2006) have often been framed by contrast to laws and deductive-nomological explanation. A more adequate conception of lawfulness and nomological necessity, emphasizing the role of modal considerations in scientific reasoning, circumvents such contrasts and enhances understanding of mechanisms and their scientific significance. The first part of the paper sketches this conception of lawfulness, drawing upon Haugeland (1998), Lange (2000, 2007), and Rouse (2015). This conception emphasizes the role of lawful stability under relevant counterfactual suppositions in scientific reasoning across the sciences, in place of traditional conceptions of law that are primarily confined to the physical sciences. It also extends lawful stability beyond verbally or mathematically expressed law-statements, to encompass other ways of conjoining patterns in the world with scientific pattern recognition. The remainder of this paper shows how and why mechanisms constructively exemplify this conception of lawfulness in scientific practice:

• Mechanisms are robust, counterfactually stable and inductively projectible patterns, even though they are not exceptionless "laws of nature".

• Mechanistic explanations often take non-verbal forms, which consequently resist philosophical inclinations to semantic ascent, but understanding lawfulness in terms of counterfactually stable pattern recognition accounts for these ways in which scientific understanding outruns the expressive capacities of natural languages;

• Mechanisms are sometimes characterized as real ("ontic") patterns in the world, and sometimes as epistemic representations; understanding mechanisms as modal patterns shows why both conceptions are needed, as mutually supportive.

• Mechanisms are typically open-ended, and only partially specified, in ways open to and directive toward further articulation and revision ("discovery"). Understanding mechanisms as modal patterns incorporates this aspect of mechanistic understanding within a broader conception of scientific understanding as embedded in research practice, rather than in bodies of knowledge extracted from it.

• Mechanistic explanation has often been placed on the causal side of an opposition between causal and nomological explanation, but understanding mechanisms as modal patterns helps overcome that opposition, and contributes to a pluralist conception of causal relations and their characteristic forms of counterfactual invariance.

• The recognition of mechanisms as modal patterns allows for a new way to think about the relations among distinct levels of a mechanistic hierarchy, and the broader scientific significance of mechanistic understanding.

REFERENCES
Bechtel, William 2006. Discovering Cell Mechanisms. Cambridge: Cambridge University Press.
_______________ and Abrahamsen, Adele 2005. Explanation: A Mechanistic Alternative. Studies in the History and Philosophy of Biological and Biomedical Sciences 36: 421-41.
Craver, Carl 2007. Explaining the Brain. Oxford: Oxford University Press.
___________ and Darden, Lindley 2013. In Search of Mechanisms. Chicago: University of Chicago Press.
Darden, Lindley 2006. Reasoning in Biological Discoveries. Cambridge: Cambridge University Press.
Haugeland, John 1998. Having Thought. Cambridge, MA: Harvard University Press.
Lange, Marc 2000. Natural Laws in Scientific Practice. Oxford: Oxford University Press.
___________ 2007. Laws and Theories. In A. Plutynski and S. Sarkar, eds., A Companion to Philosophy of Biology, 489-505. Malden, MA: Blackwell.
Rouse, Joseph 2015. Articulating the World. Chicago: University of Chicago Press.

Meta-Analyses in Neuroimaging: Integrating Data to Achieve Construct Stability

Jessey Wright
University of Western Ontario
Canada

Cognitive neuroscience, understood as the study of the relationship between the human brain and cognition, is presently faced with a lack of stability of cognitive constructs within and between laboratories. Constructs, such as 'working memory', are the concepts used to describe cognitive phenomena. They are specified by conceptual definitions that relate the construct to phenomena in the world, and the experimental protocols used to create instances of the construct in the lab. Construct instability occurs when the same construct, or concept, is used to refer to different phenomena. It can arise from variations in either conceptual or experimental practices. Instability is a problem because construct stability is a prerequisite for the integration of neuroscientific explanations (Sullivan 2015; in press). After all, results cannot be brought together to improve understanding of a specific phenomenon unless the results are actually about that phenomenon.

Neuroscientists have proposed using meta-analyses of neuroimaging data to stabilize cognitive constructs (e.g, Yarkoni et al 2011). A meta-analysis is an integrative practice that involves synthesizing and analyzing data produced in different research contexts. I assess how this is possible, given that construct stability is a prerequisite for integration, and implications of this for our understanding of constructs in cognitive neuroscience. I argue that (1) construct stability is not a prerequisite for the kind of data integration that meta-analyses are engaged in; and (2) the constructs resulting from these procedures are about different phenomena than the constructs from psychology that they are derived from. I support my argument with an analysis of the techniques and methods used by neuroscientists to stabilize cognitive constructs (e.g., Lenartowicz et al 2010).

Neuroimaging data has two parts: functional data, which captures metabolic changes in brain activity, and behavioural data, which captures the participant's performance at a cognitive task. Cognitive tasks are used to produced an instance of a construct, and so behavioural data are taken to be the relevant data for evaluating constructs. Meta-analyses proceed by collecting together functional data from different neuroimaging experiments, and labelling it according to the constructs the researchers who produced the data took it to be about. The constructs are evaluated on the basis of their discriminability with respect to the functional data they are correlated with. From this it follows that meta-analysis procedures redefine constructs by making functional data relevant to construct explication.

I conclude by considering this case in the context of data integration more broadly. The meta-analysis of neuroimaging data is different from efforts to share data via databases, which the discussion on data integration has been concerned with (e.g., Leonelli 2013). In the case of databases, concepts used to describe phenomena are often stabilized so data can be integrated and interpreted. The meta-analysis of neuroimaging data operates in the other direction. The data is integrated so that the concepts can be stabilized. As new kinds of data are made relevant to specifying the meaning of a concept, concepts are remade to fit the integrated data.

Multiscale Modeling in Nanoscience: Beyond Non-Mereological Relations

Julia Bursten
San Francisco State University
United States

Multiscale modeling is an increasingly common scientific practice. It involves investigating systems via complex, often simulation-driven models that represent a variety of behaviors. These behaviors occur at differing length, time, or energy scales, and multiscale approaches combine information about microscopic, mesoscopic, and macroscopic component behaviors into representative, predictive, or explanatory models of a given system. Recently (e.g. Batterman 2012), multiscale modeling has been proposed as a promising alternative approach to traditional reductive and/or emergent frameworks for understanding inter-theory and inter-model relations. Traditional accounts, exemplified by the literatures on reduction and emergence, disagree on the relative privilege of various component theories or models used to describe, predict, or explain systems of scientific interest. However, they all agree that some partial or total ordering of theories is both ontologically correct and scientifically useful. The multiscale approach instead shows that a greater degree of understanding—and thus greater descriptive, predictive, and explanatory power—can be gleaned not from ranking theories for fundamentality or autonomy, but instead by exploring how component theories and models, especially those that make apparently contradictory assumptions about the nature of the systems involved, are successfully linked up into multiscale-modeling frameworks.

Winsberg's (2010) "handshaking" account is a multiscale approach to inter-model relations. The account features Winsberg's argument that relations among the component models in a multiscale modeling system are not related mereologically, but rather by empirically determined algorithms. In this talk, I argue that while the handshaking account does demonstrate the existence of non-mereological relationships among component models, Winsberg does not attend to the different ways in which handshaking algorithms are developed. By overlooking the distinct strategies employed in different handshake models, Winsberg's account fails to capture the central feature of effective multiscale modeling practices, namely, how the dominant behaviors of the modeled systems vary across the different scales, and how this variation constrains the ways modelers can combine component models. I discuss a variety of modes of handshaking. I begin with the two distinct methods employed in Winsberg's example of nanoscale crack propagation, which I call boundary condition manipulation and fictionalization, and introduce additional examples from my work with synthetic nanoscientists that employ a third technique, namely embedding models. I show how the different modes arise from the scale-dependent physics involved in each component model.

Finally, I discuss the upshots of multiscale accounts of inter-theory relations for nanoscience. As a highly applied, methodologically interdisciplinary, and relatively young science, nanoscience is still building its own conceptual foundations. I show how nanoscience's practical, synthetic aims are better described by conceptual frameworks founded on multiscale approaches than on reductive or emergent ones.

References
Batterman (2012). "The Tyranny of Scales." in Batterman, ed., The Oxford Handbook of Philosophy of Physics. Oxford.
Winsberg (2010). Science in the Age of Computer Simulation. Chicago.

Natural Evolution Expressed in terms of Optimization (OptiEvo)

Xiaojiang Feng
Princeton University
United States

Jinhai Chen
Princeton University
United States

Mohammad Seyedsayamdost
Princeton University
United States

Herschel Rabitz
Princeton University
United States

The origin of life and its evolution to its current diverse forms have remained a challenge to explain. The common view by scientists is that primitive life forms originated from accidental chemical reactions that produced the precursor organic molecules necessary for self-replication, while the evolution of life forms occurred through complex interactions of random genetic changes and natural selection.

An overarching question here is the reproducibility of creating life through evolution, i.e., will we get the same life forms if we replay the "tape of life"? In principle, since both life's creation and evolution involve random chemical and biological processes, and since the interaction between life forms and the environment is highly complex, one would reasonably expect (1) the creation of life to be an extremely unlikely (fortunate) event and (2) the evolution of life to be highly divergent and non-repeatable.

Both topics have been studied using carefully designed experiments under controlled conditions, especially through observing the laboratory evolution of microorganisms. A common observation is the coexistence of genetic diversity and fitness convergence after sufficient time of evolution. This observation is surprising because evolution occurs on a “fitness landscape” characterizing fitness as a very high dimensional function of the genotype variables. Intuitively, such complex fitness landscapes would be rugged (i.e., containing many isolated fitness peaks and valleys), resulting in both genetic diversity and fitness divergence (not convergence). Various theoretical models have been proposed to explain this apparent disparity, but almost all assume a rugged fitness landscape and seek biological strategies to “jump over” fitness valleys.

The OptiEvo theorem developed as an application of OptiSci, which views the evolution of life as fitness optimization, may help explain these puzzling experimental findings. OptiEvo expresses evolution as an excursion of the species population over the fitness landscape where nature’s optimization variables are the nucleotides constituting an organism’s genome. A topological analysis of the landscape then shows that (a) the fitness landscape should be globally smooth, containing no isolated fitness peaks, and (b) the optimal genotypes can form a connected equivalent set of fitness values. These two conclusions are valid when the environment is constant or is changing slowly compared to the rate of genetic change.

The OptiEvo theorem rests on the satisfaction of basic physical assumptions and mathematical analysis whose conclusions naturally explain the coexistence of genetic diversity and fitness convergence. Interestingly, OptiEvo implies that globally smooth fitness landscapes are statistically more likely when the size of the genome is larger, i.e., complexity begets simplicity. Lastly, OptiEvo linked with OptiChem suggests that the creation of primitive life may also be less difficult than implied by its apparently complex avenues. These theorems likewise imply that life should exist throughout the universe in favorable conditions, while the detailed forms of organisms could differ considerably from those on earth. Thus, OptiEvo may provide new insights regarding the scope of evolution in the natural world while affording a nice example of the utility of formal methods in biological theorizing.

A New Comparative Model of the Historiography of Science: From Neutral Data Assessments to Comparisons through Epistemic Values

Jouni-Matti Kuukkanen
The University of Oulu
Finland

Philosophical or other general ideas of science should be not mere speculations but be based on some external substratum. Since the emergence of the historical philosophy of science the history of science has functioned in this role for philosophical theories of science. Further, it may be said that an evidential relationship between the philosophy of science and the history of science is essential for the disciplinary identity of History and the Philosophy of Science (HPS). For how could the history of science be otherwise relevant for the philosophy of science, and vice versa?

Arguably the most well-known explicit attempt to test philosophical theories was the Virginia Polytechnic Institute project for empirical testing of philosophical claims in the 1980s (Donovan 1988; Laudan 1986). However, recent decades have shown that the approach that treats the history of science as a testing ground for philosophical theories faces serious problems. The two most significant ones are the problem of theory-ladeness of historical data and the problem of circularity of theory-testing. Theory-ladeness means that there is no neutral data but that any reconstruction of history is laden by various kinds of theoretical and philosophical assumptions. This is serious because the traditional ‘confrontation model’ (Schickore, 2011) of the VPI kind requires data as a neutral arbiter between different philosophical theories and ideas. The circularity problem follows from theory-ladeness. If a theory to be tested already influences the reconstruction of a historical episode, the testing of that theory by reference to the data of that episode becomes circular. It is, as Hull remarked, that if the historian approaches his data, say, from the perspective of a Darwinian theory, one "should not be surprised when … [one's] observation support … [one's] theory" (Hull 1992, 471).

While a few believe that the confrontation model can be further developed (e.g. Laudan 1989), almost all commentators have taken the problems of theory-ladeness and circularity as their starting point. This current state of affairs has prompted several reactions. One reaction to is to fine-tune the view of evidential link and understand the evidential force of historical case studies as weaker than assumed earlier (e.g. Kinzel 2015). But then one implicitly accepts the confrontation model and faces the problem of sliding scale with the regard to evidential force, which leads to a non-evidential hermeneutical approach. Some have argued that most that can be done is to use the history of science to illustrate our philosophical ideas and concepts so that gradually a reflective equilibrium is achieved (e.g. Schickore 2011). Unfortunately, the historiography of science loses its critical evaluative function entirely and becomes ornamental.

A new comparative model of the historiography of science developed in this research project adopts a different approach to the relation between the history of science and the philosophy. The different models and their reconstructions, which are analogous to Lakatos' 'historiographical research programmes' (Lakatos 1979 vol 1, 103, 131-132, 137, 192), are to be compared to each other via both theory- and data-independent evaluative criteria, and not directly to historiographical data. The hypothesis is that epistemic values, such as consistency, coherence, empirical adequacy, the scope of explanation, the comprehensiveness of source material, fruitfulness, etc. can be used in comparisons even if alternative frameworks, such as scientific realism, the SSK and the actor-network theory, provide very different accounts with different explanatory factors and ontologies. It is not expected that they determine an absolutely correct framework but that they enable a comparative ranking of alternative reconstructions nevertheless.

Bibliography

Donovan, A., Laudan, R. & L. Laudan. 1988. Scrutinizing Science. Empirical Studies of Scientific Change. Dodrecht, Boston, London.

Kluwer. Hull, D. (1992). Testing Philosophical Claims about Science. PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, 2, 468-475.

Kinzel, K. (2015). "Narrative and Evidence. How Can Case Studies from the History of Science Support Claims in the Philosophy of Science?" Studies in History and Philosophy of Science 49, 48-57.

Lakatos, I.. 1979, Vol 1. The Methodology of Scientific Research Programmes. Philosophical Papers Volume 1. Edited by John Worrall and Gregory Currie. Cambridge: Cambridge University Press.

Laudan, L. (1986). "Scientific Change: Philosophical Models and Historical Research." Synthese 69, special issue on Testing Theories of Scientific Change, 141-223.

Laudan, L. (1989). "Thoughts on HPS: 20 Years Later." Studies in History and Philosophy of Science 20, 9-13.

Nickles, T. (1986). Remarks on the Use of History as Evidence. Synthese, 69, 253-266.

Schickore, J. (2011). "More Thoughts on HPS: Another 20 Years Later." Perspectives on Science 19, 453-81.

Solomon, Miriam. (2001). Social Empiricism. Cambridge, MA: MIT Press.

Natural Kinds in Practice

Beckett Sterner
Arizona State University
United States

The metaphysics of natural kinds has overshadowed the epistemology of kinds in practice. Philosophers have been searching for a theory to describe the underlying nature of kinds we discover through science or everyday experience. In pursuing this project for realist metaphysics, philosophers have understandably relied on cases, such as the chemical elements, where we already have highly reliable scientific knowledge. However, scientists make practical use of kind concepts even in the absence of such knowledge, including when they are uncertain about whether a kind even exists. To the extent that existing work on the metaphysics of natural kinds addresses the epistemology of kinds in practice, it tends to give a cursory story that misrepresents the philosophical content and complexity of scientific practice. “Metaphysical approaches to kind individuation lead to a flattening of representational reasoning in biology; i.e., they treat it as a relatively homogeneous endeavor, ignoring the particularities of disciplinary contexts where empirical inquiry occurs” (Love 2008, 60). As a result, there must be more to the epistemology of kinds in practice than what we can do with kinds we already know are real.

I describe a positive approach to studying natural kinds in this expanded arena by showing how making commitments to the existence of kinds helps scientists structure their research problems. For example, scientists may propose a new theory that depends on the existence of a hypothesized kind in nature to explain a class of phenomena. Making assumption allows the scientists to organize their research process in terms of testing for the existence of these proposed kinds. I look at a case study from systems biology where scientists proposed the existence of network motifs as evolved, causal patterns of interactions between genes in order to explain the behaviors and molecular organization of cells. The scientists used the idea of network motifs to articulate an iterative procedure for testing their proposed explanation by progressively narrowing the space of candidate types of network motifs based on whether they exhibited each of the proposed properties. I show how the temporal sequence of testing network motifs illustrates a range of epistemic roles for natural kinds that have been overlooked by the traditional approach.

Over time, scientists and philosophers have accumulated a bewildering array of kinds of kinds, including functional kinds, microstructural kinds, and historical kinds, along with Mill-kinds, Leibniz-kinds, and Peirce-kinds (Hacking 1991). On the original view of the natural kinds program, only one of these kind concepts could succeed as a theory of the single, true ontology of real kinds. Over time, however, different theories of natural kinds have ended up prioritizing different aims, leading to an overall incommensurability of standards for what an account should achieve (Hacking 2007). Where the realist project seems to founder on such a radical plurality, a practice-oriented approach can actually find strength: the distinctive value of Mill-kinds, functional kinds, or Peirce-kinds lies in the distinctive and often complementary ways they allow scientists to structure their problem-solving activities.

Objectivity and Cognitive Interests

Torsten Wilholt
Leibniz Universität Hannover
Germany

This paper explores the possibilities of using the concept of inductive risk to elucidate the ideal of scientific objectivity. In previous work (Wilholt 2009), I have proposed to regard biases and distortions as deviations from standards that exist within research communities by convention. The binding character of such standards arises from the fact that they enable epistemic trust between individual cognitive actors and hence facilitate the emergence of real epistemic communities. The standards place constraints on the permissible trade-offs between different kinds of inductive risk and thereby constitute a norm against which biases can be measured. In the present paper, I will explore the possibility of developing this analysis into a more comprehensive account of scientific objectivity. A decisive challenge of this approach is posed by cases in which a research community loses its independence as a whole, so that the community standards themselves seem no longer objective (in a pre-theoretical, intuitive sense). Such cases seem to presuppose that scientific communities are in some sense obliged to stipulate the “right” kinds of standards. But an assessment of the appropriateness of methodological standards can only make sense in relation to the values and interests at stake. I will argue that the relevant values and interests cannot be just any kinds that the researchers happen to hold. I will propose that standards of a community must be measured against a “cognitive interest” that is characteristic for the kind of inquiry that they are publicly perceived to be engaged in. A cognitive interest in the relevant sense is characterized by a set of objectives, typically a certain target reliability of positive results (“specificity”), a certain target reliability of negative results (“sensitivity”), and a certain target propensity to produce definitive results at all (“productivity”). Not just any interest qualifies as a cognitive interest. Cognitive interests must be constituted by objectives that represent dimensions of the search for truth. At the same time, the concept makes room for the insight that truth cannot be understood as one monolithic aim of research, but that truth-directedness comes in more than one dimension (most importantly, specificity, sensitivity and productivity). I will discuss an analysis of scientific objectivity as the match between methodology and characteristic cognitive interest. A good match in this respect underlies the trustworthiness of science as an epistemic enterprise. This would mean that the ubiquity and diversity of cognitive interests is not undermining the very possibility of scientific objectivity, as critical theory once suspected. Rightly understood, objectivity is a relation of appropriateness between a collective effort of inquiry and its underlying cognitive interest. Thus understood, objectivity designates the proper relation of an episode of inquiry not to an object, but to a complex kind of objective.

Wilholt, T. (2009), Bias and Values in Scientific Research, Studies in History and Philosophy of Science 40, 92-101.

On the Application of Science to Science Itself: Chemistry, Instruments, and the Scientific Labor Process

George Borg
University of Pittsburgh, Department of History and Philosophy of Science
United States

The period dubbed the "Instrumental Revolution" in the history of chemistry refers to a transitional period in the mid-20th century during which sophisticated instrumentation based on physical principles was introduced to solve chemical problems. Historical reflection on whether the revolution was a scientific one has been dominated by general models of scientific revolution, in particular those proposed by Thomas Kuhn, I. B. Cohen and Ian Hacking. Here I propose that the Industrial Revolution is a useful model for understanding the transformation wrought by the increasingly important role of machines in chemical research. Drawing on Marx's analysis of that event, I argue that the use of physical instrumentation was accompanied by a radical reorganization of the labor process in at least one kind of chemical research, the study of molecular structure. That reorganization permitted a rapid flourishing of methods in structural chemistry. Previous work on the Instrumental Revolution has focused on the institutional and individual pathways permitting the transfer of technology from physics to chemistry. In light of this case study, however, I argue that the dynamics of technical progress following the transfer of technology from one scientific field to another depend, in addition to the pathways connecting the fields, on the distribution of functions within the labor process of the importing field. I thus demonstrate the utility of the conception of science as a labor process for understanding its historical development. I begin by offering grounds for thinking that the resemblance between the Instrumental and Industrial Revolution is not fortuitous, but rather reflects a general pattern of development caused by the mechanization of the labor process, drawing largely on evidence from structural organic, but also occasionally analytic, chemistry. I then argue that the connection between the Instrumental Revolution and the Industrial Revolution helps us understand in what way the former was a scientific revolution. I propose that the Instrumental Revolution had a significant, progressive impact on the dynamics of methodological innovation in structural chemistry, somewhat analogously to the way in which the Industrial Revolution had an impact on the dynamics of technological innovation in capitalism, and that this rapid progress was due in part to mechanization. The Instrumental Revolution resulted in a sharp increase in the rate of development of laboratory tools and techniques. This afforded a huge increase in analytic productivity. To preview an example discussed in greater detail in the paper: the basic principle of NMR spectroscopy was discovered in 1946, and 35 years later, the method had been developed to the point that the complete carbon skeleton of a molecule could be established in a single experiment, representing a level of productivity that was unattainable using classical methods. My contention is that this increase in productivity is partially explained by the specific characteristics of machines, in particular by the fact that they allow advances from a broad range of scientific and technological fields to be concentrated in the instrument of labor. In my view, therefore, the revolutionary character of the Instrumental Revolution should be located at the level of the labor process, not at the level of theories, mental events or institutions.

On the Distinction Between 'Models of' and 'Models for' in Molecular Biology

Emanuele Ratti
University of Notre Dame, Center for Theology, Science and Human Flourishing
United States

Evelyn Fox Keller's distinction between 'models of' and 'models for' seems to capture, at least intuitively, the dynamics and the intersections between practical and theoretical aspects of molecular biology. However, the distinction deserves more attention. In molecular biology, 'models of (phenomena)' should be understood as models representing biological phenomena in a mechanistic fashion, while 'models for (manipulating phenomena)' are models suggesting new types of material manipulations and experimental strategies ("tools for material change"). What is the relationship between these two types of models? Which are exactly their differences? What kind of virtues do these different types of models have to embed? I will show the importance of both types of models in molecular biology by illustrating how they shape the understanding of scientific progress in molecular biology. In this field, the understanding is dual because molecular biology seems to progress both by accumulating 'pragmatically' complete mechanistic models, and by elaborating new experimental strategies that are suggested by molecular discoveries. Therefore, both types of models are needed and they play specific roles. I will illustrate such 'duality' in particular in the episode of the landmark discovery and characterization of restriction-modification (RM) systems, and their exploitations for DNA mapping and sequencing technologies. However, this case study also suggest that, while in molecular biology the distinction is in principle very sharp, in scientific practice it is not. In particular, the model of RM-systems constantly shifts from being a 'model of' and a 'model for', thereby undermining the distinction itself. Nonetheless, I will argue that the difference between 'models of' and 'models for' still hold, and it lies in two specific types of (cognitive) dispositions of biologists. These dispositions are character-traits, (not innate) habits or tendencies that practitioners of molecular biology develop in the course of their scientific education. In molecular biology, such tendencies profoundly influence the practice of modeling and it is through these that biologists treat models either as 'models of' or as 'models for'. I will propose that each disposition affect the choice of a different set of (what have been called) 'cognitive values' or 'representational ideals', thereby influencing the development of models in different directions according to different desiderata.

On the Elusiveness of Intentional Gaps in Mathematical Proofs

Line E. Andersen
Centre for Science Studies, Aarhus University
Denmark

In a paper from 2003, Don Fallis distinguishes between three types of gaps in mathematical proofs: inferential gaps, untraversed gaps, and enthymematic gaps. A mathematician has overlooked an inferential gap when the sequence of propositions she has in mind as being a proof is not a proof. She has left an untraversed gap when she has not gone through all the details of the sequence she has in mind as being a proof. In the case where nobody else has gone through the details either, the gap is universally untraversed. Finally, a mathematician has left an enthymematic gap in the presentation of her proof when she has not presented the entire sequence of propositions she has in mind as being a proof. Untraversed gaps and enthymematic gaps are intentional gaps.

In this paper, I argue that Fallis' way of thinking about gaps in proofs is based on a flawed conception of proof. It seems to me that it is seldom appropriate to speak of intentional gaps (as well as leaps, contractions, and so on) in proofs. According to Fallis, a proof is essentially a sequence of basic mathematical inferences where 'a basic mathematical inference' is an inference that is "accepted by the mathematical community as usable in proof without any further need of argument" (49f.). He writes that the set of basic mathematical inferences varies across time and sub-

disciplines, but that it appears to be fixed in any given context and that “mathematicians know exactly what the set contains” (49).

If it is clear what a complete proof is, it is clear what a gap is: A proof has a gap wherever it deviates from being a complete proof. I argue, however, that it is not clear what a complete proof is, even within a given context. A proof can be thought through or presented in more or less detail, but it is not clear when the level of detail is high enough for the proof to be “complete” or gapless. There is no set standard to speak of gaps (or contractions) in relation to. Instead of speaking of intentional gaps in proofs, I propose that we speak of their levels of detail. Saying that some proof lacks detail does not imply that there is a clear and unambiguous standard by which this is determined. This is not to suggest that we should never speak of intentional gaps in proofs, but that we should only do so in limited cases, for example when referring to statements such as, “It is easy to show that q follows from p, so I skip this step.” Instead of saying that mathematicians often accept proofs that have gaps in them, as Fallis does (59f.), we should say that they often accept proofs that aren’t very detailed. What is up for negotiation in the mathematical community is usually not whether some proof has too many gaps to be accepted, but whether it is detailed enough to be so.

Fallis, D. (2003). Intentional Gaps in Mathematical Proofs. Synthese 134: 45-69.

On the General Concept of Mental Disorder and the Subordinate Concepts of Individual Disorders in DSM-V

Maria Cristina Amoretti
University of Genoa
Italy

Elisabetta Lalumera
Bicocca University, Milan
Italy

The general concept of mental disorder given by DSM-5 is definitional in character: a mental disorder seems to be identified with a harmful dysfunction. The aim of this paper is twofold. First, we analyze the general definition of mental disorder to understand what necessary (and maybe sufficient) conditions do actually characterize DSM-5 general concept of mental disorder. Second, we consider the subordinate concepts of individual disorders, focusing on major depression and narcissism as case studies. To conclude, we underline some contrasts between the general definition of mental disorder and the concepts of individual disorders, and propose some possible amendments.

First, we compare the definition of mental disorder stated in DSM-5 (2013) with those listed in DSM-III (1980), DSM-III-R (1987), and DSM-IV (1994) to see how it has changed over the years. This analysis brings us to appreciate the following characteristics of current definition. (1) DSM-5 identifies mental disorders with syndromes, that is with symptoms and signs, not with their proximal cause. As it is often unclear whether to regard symptoms as physical or mental, the distinction between somatic and mental disorders does not have clear cut boundaries and can be a pragmatic choice. (2) It gives primacy to the dysfunction criterion. The dysfunction is a necessary condition: no mental disorder can be correctly diagnosed as such without any dysfunction. Despite this key role, DSM-5 never defines what a function or a dysfunction are. (3) The harmful criterion is not only secondary, but also—in contrast to DSM-III-R and DSM-IV—unnecessary: DSM-5 just requires that mental disorders would be usually associated with significant distress or disability. Thus a mental disorder can be correctly diagnosed as such when there is no harm.

Second, we examine what are the main features of the various subordinate concepts of individual disorders. This analysis brings with it a heterogeneous picture. All subordinate concepts of individual disorders are criterial concepts (that is, they are not definitional concepts, but they are not prototypical concepts as well—at least in the current psychological sense). However, the comparison between major depression and narcissism shows that: (1) the dysfunction criterion in clearly present in the first case only, not in the second one; (2) provided that the subordinate concept of narcissism could be redescribed as having a dysfunctional criterion, no account of dysfunction conceived in evolutionary terms would convincingly fit it; (3) pharmacological research provides cues for setting the boundaries of the subordinate concept of major depression, but not of narcissism.

To conclude, we claim that a better definition of the general concept of mental disorder should make explicit what a function and a dysfunction are, and that defining functions in biostatistical rather than in evolutionary terms might be more effective in accommodating the various subordinate concepts of individual mental disorders. Moreover, we claim that the harmful criterion is rightly regarded as unnecessary in the general definition of mental disorder, while its role in the characterization of individual mental disorders should be better clarified.

Ontological Investigations and Social Scientific Practice

Simon Lohse
Leibniz Universitaet Hannover
Germany

This talk aims to contribute to the debate revolving around the relevance of ontological projects in the philosophy of the social sciences. More precisely, the talk is an attempt to respond to neo-pragmatist philosophers who contest the usefulness of ontological investigations for the social sciences tout court and, hence, propose that we should de-ontologize the philosophy of the social sciences in favour of epistemological and methodological investigations (Kivinen and Piiroinen 2006, 2007; van Bouwel and Weber 2008; Tsilipakos 2012). My goal here is to defend the view that ontological investigations—of a certain kind—can indeed be useful for the social sciences. I will go about this as follows. After some clarifying remarks on my terminology, I will discuss two prominent views on the relevance and irrelevance of ontological investigations for the social sciences respectively. (1) Ontological foundationalism consists in the view that ontological investigations play a central role for the social sciences, as they are the foundation for the explanation of social phenomena. (2) Anti-ontological pragmatism, by contrast, states that doing ontology is irrelevant or fruitless for explanatory practices in the social sciences. I will argue that both views are unsatisfactory. The subsequent part of the talk will introduce an alternative role for ontological projects in the philosophy of the social sciences that fares better in this respect by paying attention to actual social scientific practices. I will illustrate and support this alternative through discussion of two examples: (a) the investigation and clarification of ontological assumptions in mechanistic explanations, and (b) the illumination of ontological relationships between different explanatory frameworks in the social sciences. In the conclusion I will wrap up the main points of my discussion and draw out some general conclusions for the philosophy of the social sciences and its connection to the social sciences.

Optimization in the Chemical Sciences (OptiChem)

Katharine Tibbetts
Virginia Commonwealth University
United States

Herschel Rabitz
Princeton University
United States

For hundreds of years, chemists have sought to maximize the yields of chemical reactions and tailor the properties of the reaction products for applications. The wide success of these optimization goals is obvious in the proliferation of everything from pharmaceuticals to solid state materials in electronics. However, there is no a priori reason to expect that optimization in chemistry should be easy, or even practically feasible. In fact, simple reasoning suggests that optimization in chemistry should be exceedingly difficult. For example, the potential number of combinations of reagents and reaction conditions to choose from when optimizing the yield of a chemical reaction is unbounded in principle and enormous in practice, creating the expectation of a daunting challenge akin to finding a needle in a haystack. Yet, despite the vast number of choices, typical investigations need to sample only a very small selected fraction of the possibilities in order to accomplish virtually any well-posed synthesis goal. This surprising circumstance raises the question of why such favorable behavior is widely observed across a variety of optimization objectives in the chemical sciences.

We have recently introduced a mathematically rigorous theorem encompassed in OptiChem to explain the success of optimization across the chemical sciences without regard to optimizing any specific synthesis target or molecular/material property. The theorem, whose roots are tied to an analogous theorem describing optimization in quantum mechanics (OptiQ), rests on the concept of a fitness landscape, or the quantitative expression of the optimization objective (e.g., fractional yield of a chemical reaction) as a function of the accessible variables (e.g., the concentration of each reagent). The OptiChem theorem predicts that, upon satisfaction of physically reasonable assumptions, the fitness landscape for any optimization objective in the chemical sciences will possess specific properties. We will present broad evidence that fitness landscapes reported in or extracted from the chemical literature do indeed satisfy the properties predicted by the OptiChem theorem.

The broad evidence for the validity of the OptiChem theorem raises important implications when considering past, present, and future research practices across the chemical sciences. For example, for decades chemists have used empirical "rules" organizing chemical substances to predict reaction outcomes. The OptiChem theorem not only offers a physical explanation for the existence of such rules, but also provides a basis for systematically identifying new rules. Assuming the validity of the assumptions underlying the OptiChem theorem also offers ways to significantly improve upon modern methodological practices such as factorial design and genetic algorithm optimization. This work may inform a variety of current philosophical projects, including (1) the utility of conceptualiz- ing optimization problems in terms of fitness landscapes, an issue seldom discussed by philosophers outside the context of biological evolution, (2) the methodological role of formal methods to supple- ment or even supplant traditional (and more informal) empirical methods, and (3) the application of automated computational algorithms to facilitate practical optimization goals in the chemical sciences.

Organizing Principles in Neuroanatomical and Physiological Practice

Philipp Haueis
Berlin School of Mind and Brain
Germany

Philosophers of science commonly assume that neuroscientists primarily aim to uncover mechanisms of biological or cognitive phenomena (Craver 2007). In recent years, however, data-driven and graph-theoretical approaches to brain connectivity attempt to find organizing principles (Biswal 2010, Sporns 2011). While organizing principles have gained renewed attention in the philosophy of biology (Green 2015), an analysis of this notion in the neurosciences is missing so far.

In this paper, I explicate two senses of "organizing principle" in neuroscientific practice. In the pragmatic sense, such principles help researchers to link experimental results from various systems and species. The pragmatic sense is exemplified in research on central pattern generators (CPGs). CPGs are oscillatory circuits that are found in a wide variety of species (lobsters, lampreys, cats, humans). The presence of certain general features (e.g., rhythmic output produced by pacemaker and follower neurons) is taken as evidence that CPGs are exemplars of organizing principles (Striedter et al. 2014). I propose that the generality of CPGs is a case of discovering "modular subassemblies" (Darden 2006). CPGs are evolutionarily repeated configurations that implement different survival functions (e.g. digestion, respiration, movement), depending on the species and system at hand. The pragmatic interpretation of CPGs suggests that they provide principles to organize the discovery of similar circuits in other systems, such as the cerebral cortex (Yuste et al. 2005). Whereas exploration can rely on general features of CPGs to describe unknown circuits, explanation will require the specifics of the survival function and spatiotemporal organization of the underlying mechanism in the particular species.

In the second, “ontic” sense, however, organizing principles do not only guide the search for, but supposedly also constrain the operation of species-specific mechanisms (cf. Wouters 2007). This sense is most clearly exemplified by the quantitative neuroanatomical studies of Herculano-Houzel and colleagues. Facilitated by the technological possibility of measuring total neuron numbers via isotropic fractionation (Herculano-Houzel and Lent 2005), these researchers determined a series of species-invariant relationships between structural or functional measures. By discussing the degree of folding and fixed energy budget as examples of this research strategy (Mota and Herculano-Houzel 2015, Herculano-Houzel 2011), I show how these invariant relationships constrain the space of possible mechanisms that a given brain organization can realize. Because principles only constrain, rather than determine “how-actually” explanations, I also argue that it is an empirical question whether they stand in a one-to-one or one-to-many relation to species-specific mechanisms being covered by the generalization.

I end my paper by showing how the commonalities between both senses improve our philosophical understanding of organizing principles. While previous authors stressed mathematical analysis and abstraction from causal detail (Huneman 2010, Green 2015), my examples highlight the importance of interspecies comparison to find invariant relationships between submodular assemblies or local mechanisms. Interspecies comparison finally points out that research of “ontic” organizing principles cannot proceed independently from research into mechanisms: Without the mechanistic knowledge about causal details of how a principle is implemented, similarities that are invariant across systems can be hardly identified (Green et al. 2015).

References

Biswal, B. et al. (2010). Toward a discovery science of human brain function. PNAS 9, 4734-39.

Craver, C. (2007). Explaining the brain. Oxford: Oxford University Press.

Darden, L. (2006). Reasoning in Biological Discoveries: Essays on Mechanisms, Interfield Relations, and Anomaly Resolution. Cambridge: Cambridge University Press.

Green, S. et al. (2015). Explanatory integration challenges in evolutionary systems biology. Biological Theory 10(1), 18-35.

Green, S. (2015). Revisiting generality in biology: systems biology and the Quest for Design Principles. Biology and Philosophy 30(5), 629-652.

Herculano-Houzel, S. (2011) Scaling of Brain Metabolism with a Fixed Energy Budget per Neuron: Implications for Neuronal Activity, Plasticity and Evolution. PLoS ONE 6(3), pp. e17514. doi:10.1371/journal.pone.0017514

Herculano-Houzel, S. and Lent, R. (2005). Isotropic fractionator: a simple, rapid method for the quantification of total cell and neuron numbers in the brain. Journal of Neuroscience, 25(10), 2518-2521.

Huneman, P. (2010). Topological explanations and robustness in biological sciences. Synthese 177, 213-45

Mota, B., and Herculano-Houzel, S. (2015) Cortical folding scales universally with surface area and thickness, not number of neurons. Science 349(6243) 74-77.

Sporns, O. (2011). Networks of the brain. Cambridge, MA: MIT Press.

Striedter, G. et al. (2014). NSF workshop report: discovering general principles of nervous system organization by comparing brain maps across species, Brain Behavior Evolution 83, 1-8.

Wouters, A. (2007). Design explanation: determining the constraints on what can be alive. Erkenntnis 67, 65-80.

Yuste R., et al. (2005). The Cortex as a Central Pattern Generator. Nature Reviews Neuroscience 6, 477-483.

Patterns, Mechanisms and the Ontic/Epistemic Distinction

Lena Kästner
Berlin School of Mind and Brain
Germany

Philipp Haueis
Berlin School of Mind and Brain
Germany

In the sciences, we find that pattern recognition techniques are successfully employed, e.g. in the analysis of neuroimaging data. However, the philosophical significance of this practice remains unexplored. In fact, the concept of pattern has received only very limited philosophical attention. Among the best-known treatments are Dennett's (1991) "Real Patterns" and Haugeland's (1998) "Pattern and Being". Building on Dennett's work, Haugeland suggests that every pattern is both an orderly arrangement in the world and a candidate for recognition by creatures with the right kind of recognitive capacities. We argue that this analysis extends to many phenomena investigated by contemporary scientists; even if pattern recognition techniques are not explicitly invoked. Applying Haugeland's notion of pattern in the context of scientific explanation provides a striking benefit: it allows us to dispense with the dispute between ontic and epistemic conceptions of scientific explanation. This dispute has been especially intense among proponents of mechanistic views.

Mechanistic philosophers widely agree that physiological sciences (such as neuroscience) seek explanations by describing mechanisms, i.e. entities and activities that are spatiotemporally organized such that they exhibit the phenomenon to be explained (Illari and Williamson 2012). Proponents of an ontic view hold that mechanistic explanations situate phenomena in the causal structure of the world (Craver 2012). Proponents of an epistemic view, by contrast, emphasize the utility of mechanistic models in rendering phenomena intelligible or showing that they were to be expected given what we know about a mechanism's entities and activities (Wright and Bechtel 2007, Wright 2012). Our contention is that if mechanisms and the phenomena they serve to explain can be conceived of as patterns in Haugeland's sense, we do not have to choose between the ontic and epistemic views; they go together.

To demonstrate this, we will first show how Haugeland's conception of patterns applies to mechanisms: they are both orderly arrangements and candidates for recognition. While proponents of the ontic view emphasize the former, proponents of the epistemic view focus on the latter. However, Haugeland makes it quite clear that being an orderly arrangement goes hand in hand with being a candidate for recognition. Looking examples from scientific practice (e.g. the Hodgkin and Huxley model), we illustrate this point in the context of scientific discovery and explanation. We conclude that it is the combination of ontic and epistemic aspects that facilitates scientific progress; they are mutually implicating and there is no need to choose between them. This result is nicely in tune with Illari's (2013) recent suggestion that good mechanistic explanations should incorporate both ontic and epistemic constraints.

References

Craver, C. (2012). Scientific Explanation: The Ontic Conception. In Hutteman, A. and Kaiser, M., (ed.), Explanation in the Biological and Historical sciences. Springer.

Dennett, D. (1991). Real Patterns. Journal of Philosophy, 88, 27-51.

Haugeland, J. (1998). Having Thought. Essays in the Metaphysics of Mind. Cambridge, MA: Harvard University Press.

Illari, P. (2013). Mechanistic Explanation: Integrating the Ontic and Epistemic. Erkenntnis, 78, 237-255.

Illari, P. M. and Williamson, J. (2012). What is a mechanism? Thinking about mechanisms across the sciences. European Journal of the Philosophy of Science, 2:119-135.

Wright, C. and Bechtel, W. (2007). Mechanisms and psychological explanation. In Thagard, P. (ed.), Philosophy of Psychology and Cognitive science. Elsevier.

Wright, C. D. (2012). Mechanistic Explanation without the Ontic Conception. European Journal for Philosophy of Science, 2:375-394.

Personalised Medicine: A Negotiable Ideal Without Precise Definition

Giovanni De Grandis
NTNU
Norway

Vidar Halgunset
NTNU
Norway

The idea of personalised medicine (PM) has gathered momentum recently, attracting funding and generating hopes as well as scepticism. As PM gives rise to differing interpretations, several attempts to clarify the concepts have appeared. In an influential article published in BioMedCentral Medical Ethics in 2013, Sebastian Schleidgen and colleagues have proposed a precise definition that demarcates clearly what PM is and what is not. The paper discusses this proposal, and points out four limitations. 1) The authors overlook the fact that definitions depend on the context of use. 2) It is questionable that an inferential definition drawn from actual use can be first reduced to its core meaning and then used normatively as a demarcation criterion. 3) The pool from which they have drawn their empirical data is too limited for providing an exhaustive sample of the uses of the concept. 4) It argues that disciplining the use of the concept through normative semantics is both hopeless (because ineffective) and wrong (because it silences some voices). As an alternative to the shortcomings of the semantic strategy we propose a pragmatic approach. Rather than considering PM a scientific concept in need of precise demarcation, we look at it as an open and negotiable concept whose primary use is to orient research goals and policy objectives. The drive towards personalisation is rooted in dissatisfactions with some aspects of healthcare: a demand for more personalised services and an aspiration for a more personalised knowledge base. These basic aspirations generate different visions of PM, all of which voice genuine and important concerns. We believe that accepting the open and contestable nature of PM allows a discursive space where different instances can be articulated and engage with each other in the attempt to integrate at least some of them into a coherent and viable vision of personalised medicine. Seeing PM as a negotiable ideal acknowledges the fact that its content is not a matter of authoritative stipulation, that the nature of the ideal cannot be separated from the coalition who supports it: participants contribute to define the goal and conversely how the goal is framed affects who will be willing to participate. What PM is cannot be separated from who contributes to it and how the practices of PM are implemented. However the negotiable ideal has to be negotiated in a way that is disclosive and inclusive. We want the negotiation to be inclusive because we want to take away the power of excluding from those who are setting the agenda, and the only way of achieving this is to leave the door open to dissenting voices and interpretations. Nobody should be given the power to define concepts beforehand and hence to exclude dissenters. We want the negotiation to be disclosive because disagreements and conflicts need to be made explicit and negotiated, otherwise they become ideological screens used to hide tensions and to evade potentially divisive issues that instead need to be brought into the open and tackled as transparently and fairly as possible. Seeing PM as a negotiable ideal is both more realistic than any semantic strategy and more helpful in addressing the most relevant ethical issues.

Personalizing Medicine in Silico and in Socio

Sara Green
University of Copenhagen
Denmark

The personalization of medicine has historically emphasized the capabilities of the doctor-patient relationship in accounting for the uniqueness of the personal and social experiences of individuals. Today, personalized medicine (also called precision medicine or P4 medicine) is rearticulated in the technological context of genomics, big-data and systems medicine (the medical application of systems biology). Understanding the intertwined developments of technology, society and the life sciences requires a multi-faceted analysis that combines philosophical, scientific, clinical as well as social aspects. This paper will be presented by a philosopher of science but draws on interactions with practicing systems biologists, clinicians, and scholars within public health. I examine the prospects and challenges for using strategies from personalized medicine to improve health care, specifically through detection of risk factors for future diseases among healthy patients.

Proponents of personalized medicine argue that computational integration and analysis of patient-specific data will revolutionize our health care systems through improved techniques for disease prediction and prevention. While the ambitions of so-called 'digital patient models' remain visionary, steps to personalize medicine are already taken via personalized genomics and mobile health technologies. Funding bodies in several countries have invested in large-scale projects that create repositories of patient-specific data and "national reference genomes", such as the "Precision medicine initiative" (US), the "100,000 genomes" project (UK). The Hundred Person Wellness Project was launched as a pilot project in 2014 by the Institute for Systems Biology in Seattle and provides a window to how personalized health care may look in the future. The prospects of such strategies are, however, a matter of controversy. Some expect that personalized medicine ultimately can solve some of the pressing challenges in society, such as the increasing costs of health care and treatment of lifestyle diseases. Others have expressed concerns about negative side-effects resulting from methodological uncertainties, such as overdiagnosis and medicalization, and ethical concerns about stigmatization of patient groups. Given the ambitions to implement the new preventive strategies in health care systems, it is increasingly relevant to discuss the scientific, philosophical and social implications of the new health technologies.

In this paper, I aim to balance the focus in the scientific literature on the predictive potential of new in silico models with an analysis of what "actionability" in socio could mean from a philosophical and public health perspective. I critically analyze the epistemic and social norms in the data-intensive health strategies, and draw on these to highlight challenges for translation of statistical measures for disease risk into meaningful disease-preventive actions. Considering concrete examples of risk detection for obesity and cancer, I call for more attention to the gap between the quantitative measures of disease risk in P4 medicine, and the social and societal complexity of the patient's life. Moreover, I examine the broader social implications of the ways in which individualized preventive strategies reframe the notions of health care and identity through a re-articulation of philosophical assumptions about the human body and social norms about patient responsibility.

Philosophical and Historical Perspectives on the Diagnosis of Non-Accidental Head Injury in Infants

Nicholas Binney
University of Exeter
United Kingdom

In this paper I will highlight problems encountered with the medical and legal diagnosis of non-accidental head injury in infants, and discuss ways that philosophy and history of medicine can be done in practice to help address these problems.

On the 7th September 2000 at the Nottingham Crown Court in the UK, Lorraine Harris was convicted of the manslaughter of her one year old son. A jury was convinced that Mrs Harris had shaken her son to death. This conviction was based on the pathological findings in this case, in particular on the findings of subdural haemorrhage (SDH, bleeding under one of the membranes that enclose the brain) and retinal haemorrhage (RH, bleeding in the back of the eye). On the 21st July 2005 Mrs. Harris' conviction was quashed by the court of appeal because the court was satisfied that the presence of the pathological findings used to convict Mrs Harris were not on their own sufficient to establish a diagnosis of non-accidental head injury. This case begs the question of how strong the link between SDH, RH and non-accidental head injury really is, as a misdiagnosis in cases like this have such serious consequences.

Since April 2015 I have been working with Dr Waney Squier, a pathologist who was involved both with this trial and with the subsequent appeal. Based on my work with Dr Squier, I will argue that the medical discussion about how to diagnose non-accidental head injury stands to benefit significantly from skills and perspectives that are commonplace in academic philosophy and history. One important philosophical discussion in this context is of the theory-ladenness of observation. Using a Fleckian framework of active and passive associations, I will argue that many supposedly "raw" observations of non-accidental head injury are laden with the assumption that SDH and RH are strong evidence of abuse. Philosophical work done to identify and problematize these assumptions is therefore of great value to this medical discussion. Furthermore, highly problematic forms of argument (including examples of circular arguments and formal fallacies like affirming the consequent) are often found in the medical literature on the diagnosis of non-accidental head injury in infants. I will argue that formal training in the reconstruction and critical analysis of arguments should be made available to medical students, just as it is for undergraduate philosophy students. I will report on my experiences of providing such training to medical students in a special study unit at the University of Exeter medical school. I would also like to draw attention to the practice of medical researchers reconstructing the historical development of their knowledge of non-accidental head injury, and to their use of these histories in their arguments over how best to make a diagnosis. Many of these histories are anachronistic, and therefore often do little more than provide propaganda for a particular point of view. Nevertheless, I will argue that properly historical work into the development of this knowledge productively shape the interpretation of evidence on how to diagnose non-accidental head injury.

Philosophical Implications For Optimal Control in the Sciences Over Vast Length and Time Scales (Symposium)

Herschel Rabitz
Princeton University
United States

Andrea Woody
University of Washington
United States

The desire to optimize extends across all of the sciences. In the laboratory, these activities may range from seeking optimal performance of a quantum-operating device to maximizing the properties of a new material. Similarly, from certain perspectives nature may be recognized as optimizing through evolutionary processes. We refer to these collective efforts, whether by the hand of a scientist or by nature, as “OptiSci”—Optimal Control in the Sciences. A large body of evidence exists that optimization in the sciences is often considerably easier than a complexity assessment would suggest. This observation has led to characterizing OptiSci formally, by articulating a basic principle, with underlying assumptions, whose satisfaction explains this striking experimentally-observed control behavior across a broad range of physical and biological systems. The session will present the physical foundations for OptiSci and summarize associated supporting experimental evidence from quantum mechanics, chemistry and materials science, and natural evolution with micro-organisms. The link between optimization in various sciences as expressed by OptiSci has not been appreciated before, especially the link between the physical sciences and natural evolution. Following presentations on the three primary components of OptiSci research, a philosopher associated with this project will give a brief commentary to frame what we hope will be a wide-ranging, open-ended discussion with the audience. The research makes contact with a variety of issues of current interest to philosophers of science, both methodological and broadly metaphysical. These include the use of optimality landscape representations, algorithmic techniques for directed search of large, and complex data sets, the application of formal methods in empirical domains, and issues of unification in the context of inter-disciplinarity. We have three aims in presenting this work to a philosophical audience: (1) to explore the philosophical implications of OptiSci research generally by getting input from philosophers of science regarding both what the philosophical implications are and how best to grapple with them, (2) to consider OptiSci are a potentially unifying framework for scientific efforts across the sciences, one that may help us understand the potential for integration across highly specialized research methods rooted in distinct disciplinary traditions, and (3) to provide an example of how (as a practical matter) scientists and philosophers can join forces to explore scientific research projects as they unfold. Because of the atypical nature of this session (presenting new scientific research and exploring together with the audience both the philosophical implications of the work and the potential for interaction between science practitioners and philosophers of science that it affords), ample time for discussion will be reserved for the final portion of the session.

Presentations:

1. “An Introduction to OptiSci and Optimization of Quantum (OptiQ) Phenomena”, Benjamin Russell and Herschel Rabitz (Princeton University)

2. “Optimization in the Chemical Sciences (OptiChem)”, Katharine Moore Tibbetts (Virginia Commonwealth University) and Herschel Rabitz (Princeton University)

3. “Natural Evolution Expressed in terms of Optimization (OptiEvo)”, XiaoJiang Feng, Jinhai Chen, Mohammed Seyedsayamdost and Herschel Rabitz (Princeton University)

Commentary: Andrea I. Woody (University of Washington)

Philosophically Understanding Models in Scientific Practice: The London & London Case Study

Jan Potters
Universiteit Antwerpen
Belgium

Scientific models play a central role in many contemporary philosophical accounts of science. Different reasons have been given for this: focusing on models allows us to overcome some of the problems of the syntactic approach to theories [5, p. 35]; or it enables us to capture diachronical and dynamic aspects of science, such as theory change [7, p. 118] or the construction of e.g. experiments [8, p. 21]. Moreover, this interest has given rise to new philosophical topics and approaches, such as 'structuralism' or 'scientific understanding'.

While this focus on models has opened new directions in the philosophy of science, there is little agreement on how to approach scientific models philosophically. My aim is to clarify some of the issues that are at stake. I start with a specific case: the long-lasting philosophical debate on the London & London model of superconductivity [e.g. 1, 3-6]. All participants, at first sight, seem to have the same aim: understanding the role of scientific models in scientific practice. I will argue, however, that they are in fact partially talking past each other: when referring to scientific practice, they are in fact talking about different kinds of scientific practice. Their accounts of what constitutes scientific practice—theories, models, scientists, etc.—and of what it means for scientific practice to function successfully differ fundamentally. Explicating these differences will allow me to clarify not only the misunderstanding about the London & London model, but also more general debates about e.g. the autonomy of scientific models [8].

The dispute, however, does not merely concern philosophical accounts of scientific models. In the second part I will argue that these different notions of scientific practice go together with different conceptions of what it means to do philosophy on the basis of science, as exemplified in the London & London dispute but also in e.g. [2] and [7]. By this I mean that there are specific differences with respect to e.g. the use of scientific case studies in philosophy, the way in which to compare and evaluate philosophical claims or the goal of philosophy of science. Conceiving the debate in this way, I will argue, can further clarify some of the issues in present disputes on scientific models.

Sources

[1] Bueno, French, & Ladyman. (2012). Models and Structures. STUD HIST M P, 43(1): 43-46.
[2] Cartwright. (1999). The Dappled World. CUP.
[3] Cartwright, Showmar & Suárez. (1995). The Tool Box of Science. Poznan Studies in the Philosophy of Science and the Humanities, 44: 137-149.
[4] Cartwright & Suárez. (2008). Theories: Tools versus Models. STUD HIST M P, 39(1): 62-81.
[5] French. (1997). Partiality, Pursuit, and Practice. Structures and Norms in Science. Kluwer Academic: 35-52.
[6] French & Ladyman. (1997). Superconductivity and Structures. STUD HIST M P, 28(3): 363-393.
[7] Ladyman & Ross. (2007). Every Thing Must Go. OUP.
[8] Morgan & Morrison. (1999). Models as Mediators. CUP.

Philosophy of Autism (Symposium)

Ruth J. Sample
University of New Hampshire
United States

Susan V. H. Castro
Wichita State University
United States

Autism Spectrum Disorder (ASD) is a rich research target for empirically informed philosophy in areas ranging from basic philosophy of science to gender studies. As a phenomenon that has only recently been characterized, ASD provides an opportunity to revisit longstanding issues from a fresh perspective and new data by which to evaluate our theories. As a range of human diversity that will continue to shape our lives indefinitely, ASD makes moral and sociopolitical demands that require philosophical attention. Misunderstanding and abuse have been prevalent. Despite the recent publication of the anthology The Philosophy of Autism, ASD has received little philosophical attention. This session on autism illustrates the range and depth of philosophical issues raised by ASD. In the first talk, Jami Anderson raises several philosophical issues surrounding diagnosis. ASD is explicitly a spectrum, one that is continuous with the neurotypical (NT) spectrum, whereas the practice of diagnosis presupposes a non-arbitrary objective boundary, challenging the very coherence of ASD diagnosis (cf. Sorites paradox). The boundary between ASD and NT is currently determined by clinical observation of phenomena that stretch the notion of scientific observability, Anderson argues, and standard diagnostic practice devalues both the first person authority and the agency of patients. The implications of these issues runs deep, ultimately challenging the metaphysical reality of autism. In the second talk, Ruth Sample turns to the scientific and sociopolitical implications of characterizing ASD as an “extreme male brain” disorder. ASD is far more commonly diagnosed in males than females, and gender stereotypes tempt some to understand ASD in gender-based terms, but what could it mean to claim that ASD or the ASD brain is “male”? Sample considers the alternatives, what criteria might be offered, and what functions (legitimate or otherwise) gendering might serve. As Sample demonstrates through contrast cases, the terms in which we theorize can surreptitiously perpetuate and reinforce tangential socially constructed norms. In the final talk, Susan Castro offers a context from the history of philosophy from which autism may be comprehensively theorized without gendering, i.e. Immanuel Kant’s theory of imagination. Imagination includes visualization and fantasy, but the Kantian imagination is fundamentally the faculty that enables us to transform intuition, ranging in its uses from basic sensory integration to consciously guided episodic foresight. The wide range of how autism presents and how it is experienced can, on a Kantian view, be understood in terms of the atypical development of specific functions of imagination. This theory usefully supports the prevalent view that autism has a kind of underlying unity, at least at the level of clinical psychology, and it provides a basis for understanding distinctive individuals within the human spectrum.

Philosophy of Scientific Integrity: Distinguishing Error, Fraud, and Malpractice (Symposium)

Hanne Andersen
University of Copenhagen
Denmark

Douglas Allchin
University of Minnesota
United States

David Budtz Petersen
University of Copenhagen/Aalborg University
Denmark

Mikkel Willum Johansen
University of Copenhagen
Denmark

Frederik Voetman Christiansen
University of Copenhagen
Denmark

Misconduct and questionable research practices are topics of major concern in contemporary science. Empirical studies show that questionable practices are widespread, and the steady occurrence of spectacular misconduct cases reveal that the scientific community still lacks adequate tools for early detection, consistent intervention and efficient prevention. It is therefore an important undertaking for philosophy of science in practice to investigate how these various malpractices diverge from normal scientific practice. To begin filling this important gap, this session will:

• analyze key concepts used in the misconduct debate and argue that we need to distinguish more fully between the epistemic, ethical, and professional dimensions of science and develop deeper epistemic understanding of both error and expertise

• analyze changing experimental, statistical and publication practices and how these changes affect the quality of reported results, and discuss incentives towards 'grey zone' practices can be countered

• examine empirically how students during their education are trained to handle experimental data and how the motivation to engage in 'grey zone' behavior may arise.

Together, the talks in this session will show how a philosophy of scientific malpractice will break important new ground within:

• philosophy of science in practice—by making malpractice a new object of investigation,

• research integrity—by complementing standard ethical analysis of misconduct and questionable practices with epistemological analyses, arguing that key challenges in current misconduct debates can only be resolved by combining ethical and social epistemological perspectives, and

• RCR instruction—by identifying the need for explicit standards of professional conduct and levels of expertise

• Research policy—by articulating the distinction of error and malpractice, and clarifying those professional standards of practice

The session includes the following presentations

Philosophy of Scientific Integrity: Distinguishing Error, Fraud, and Malpractice Hanne Andersen, University of Copenhagen & Douglas Allchin, University of Minnesota

Irreproducibility: Experimenter's regress or scientific misconduct? David Budtz Pedersen, University of Copenhagen When students go to the lab: A quantitative study of the scientific conduct of students' relations to experimental data Mikkel Willum Johansen & Frederik Voetman Christiansen, University of Copenhagen

Philosophy of Science Undisciplined: Recovering Ethics and Politics with STS

Matthew Sample
University of Washington
United States

Scholarship in science and technology studies (STS), especially as approximated by Jasanoff's "idiom of co-production", suggest that knowledge practices are co-produced along with our ethical and political choices. This relationship reveals itself in many of the particularities of scientific practices: in the hopeful framings of cure or benefit that begin journal articles, in the distilled expert publics of institutional bioethics, and the entrepreneurial identity of many scientists. Truth, as an ideal, fails to capture many problematic cases arising from these entanglements that define present day science. In response, I suggest that philosophers of science (in practice) should reorient themselves away from a focus on knowledge or reasoning and towards a focus on specific features of practice. In this paper, I suggest and provide examples of three such foci from the STS literature: imaginaries, boundaries, and translation.

Imaginaries, as used by Marcus ("technoscientific imaginaries") and Jasanoff and Kim ("sociotechnical imaginaries"), are not mere fantasies or mental entities. Discoverable in practice, they encode not only what is valuable but also how, materially and institutionally, the desirable future is to be achieved. Once revealed by the scholar-analyst, they are suitable for normative evaluation in political, epistemic, and ethical terms. Boundaries provide a similar opportunity. Since Gieryn's introduction of "boundary work" as a concept, we know that scientists' choices in differentiating science from non-science have simultaneous political and epistemic consequences, allocating authority and prioritizing different forms of reasoning. Philosophers of science should develop a sensitivity to local "boundary work" and its strategic function.

Callon's "translation", lastly, might be seen as analogous to truth for philosophers of science. Successful science, in practice, is often defined in terms of a vast, distributed network. It is not the god's-eye view or the correspondence with reality that makes research a success, but rather its ability to transcend the lab, to be "translated" for the host university, the funding agency, top journals, peers, graduate students, and industry partners. Philosophers will see truth lurking, of course, in translational processes. But to see truth alone would be to deliberately avoid features of scientific practice and, thus, miss other dimensions of normativity.

The practical cost of these new tools is not negligible. Many philosophers of science will have to dust off their long-forgotten political and ethical theories. Others will be compelled to leave the office and talk with scientists and engineers or conduct qualitative research. But these should be non-issues for philosophers of science in practice. By presenting select examples from my dissertation on neural engineering, I hope to show that the perspective provided by imaginaries, boundaries, and translation both provides intellectual benefits and makes obsolete the distance between philosophy of science and (perennially misunderstood) "social construction." By adopting some of the objects of STS, philosophers of science can prove to themselves the normative potential of these concepts and restore our ability to critique science as it actually exists, rather than merely replicating the boundary work characteristic of science itself.

Pragmatist Coherence as the Source of Truth and Reality

Hasok Chang
University of Cambridge
United Kingdom

In the philosophical analysis of scientific practice it is difficult to know how to handle the concepts of truth and reality, as there are no actual scientific methods for producing statements that are true in the sense of corresponding to reality. But how can we do any respectable epistemology without thinking about truth, and metaphysics without reality?

A way out of this philosophical predicament can be found in a new coherence theory of truth. The core of this theory is a revitalized pragmatist notion of "coherence" (implicit in Dewey's theory of knowledge), irreducible to logical consistency between statements. I mean by coherence a harmonious fitting-together of actions that is conducive to the successful achievement of particular aims. Coherence may be exhibited in something as simple as the correct context-dependent coordination of bodily movements needed in lighting a match, or something as complex as the successful integration of a range of material technologies and various abstract theories in the operation of the global positioning system.

According to this new coherence theory of truth (ultimately indistinguishable from the pragmatist theory of truth), if our use of a theory has led to successful outcomes, and not as a result of any strange accident as far as we can see, then we can and should say, modestly and provisionally, that the relevant statements made in this theory are "true", in the same sense as we say that it is true that rabbits have whiskers and live in underground burrows. This "truth" is operational and verifiable. It is the same thing as empirical confirmation, taken in a broad sense. It is achievable, to various degrees, and its pursuit is clearly useful.

It is crucial to note that pragmatist coherence carries within it the constraint by nature. This gives coherentist truth the mind-independence that realists value most in correspondence truth, while it is an "internal" notion meaningful within a system of practice, not without it. Such a conception of truth easily allows plurality while not allowing arbitrariness.

Concerning reality, I propose that we should, and usually do, consider as "real" the presumed referents of concepts that that play a significant role in a coherent system of practice. My position can be considered a generalization of Hacking's experimental realism. Reality in this sense is context-dependent, or rather, system-dependent. For example, constellations are real within traditional systems of positional astronomy, but not in modern cosmology; the unique atomic weight of a chemical element is real in most chemical contexts, but not in nuclear physics. Like truth, reality comes in degrees, it is defeasible, and it is continuous with everyday usage such as "Ghosts aren't real."

With this new coherentism we realistic people, following Austin, should re-claim the notions of truth and reality, which are actually useful terms in ordinary language. This will certainly help us in the task urged by the SPSP manifesto, namely a "detailed and systematic study of scientific practice that does not dispense with concerns about truth and rationality".

Presume It Not: True Causes in the Search for the Basis Of Heredity

Aaron Novick
University of Pittsburgh
United States

Raphael Scholl
University of Cambridge
United Kingdom

Kyle Stanford (2006) has recently articulated and defended the problem of unconceived alternatives, which challenges the reliability of inference to the best explanation (IBE) in "remote" domains of nature. Because IBE is eliminative, it can only choose the best of the alternatives conceived at the time of its application. But, Stanford argues, on the basis of several historical case studies, we have good reason to believe that, when it comes to the very small, the very distant in time and space, and similarly remote domains, scientists systematically fail to exhaust all the well-confirmed alternative explanations. In such cases, IBE is unreliable. Conjoined with the claim that IBE is the central inferential tool at our disposal in investigating these domains, the problem of unconceived alternatives leads to a form of scientific anti-realism.

We pursue Stanford's case studies further, showing that the biological community has long been aware of the dangers of IBE. In particular, we reconsider the case of pangenesis, showing how the biological community reacted against Darwin's theory on the grounds that it did not satisfy an important methodological standard, the vera causa ideal. The core element of this methodological standard is the insistence that causes invoked in explanations should be shown to exist independently of their explanatory power. We then show how, in the 20th century, the study of heredity gradually came to meet this ideal (we also show that the biologists responsible for this accepted the ideal). We argue that this marks a qualitative shift from speculative, IBE-driven theorizing about heredity to the piecemeal elucidation of particular causes of heredity.

On the basis of this case study, we defend a wide-ranging, prospective scientific realism, applicable to the biological sciences. We do so by accepting the potency of Stanford's problem of unconceived alternatives, but dramatically limiting its scope. Stanford suggests that his problem applies in "remote" domains, and we agree, but contend that remoteness is relative to the experimental techniques available at a given time. As these improve, more and more domains are rendered accessible to forms of reasoning other than IBE, and so escape the problem of unconceived alternatives. We suggest that, once this occurs within a particular domain, knowledge of the causal entities and processes within that domain grows fairly cumulatively. We argue, further, that it can be recognized (fallibly, of course) when this shift occurs, for making such judgments is part and parcel of the evaluation of the quality of evidence in biology. Our realism is thus prospective, which Vickers (2013) has called the "holy grail" of scientific realism: it tells what of our current biological science we ought to accept.

References:

Stanford, P. K. (2006). Exceeding our Grasp: Science, History, and the Problem of Unconceived Alternatives. Oxford University Press.

Vickers, P. (2013). A confrontation of convergent realism. Philosophy of Science, 80(2): 189-211.

A Pragmatist Interpretation of Schizophrenia

Abigail Gosselin
Regis University
United States

For decades, scientists and mental health practitioners have treated schizophrenia as a brain disease. Neuroscientists have identified abnormal neurotransmitter activity and anatomical irregularities associated with schizophrenia, with the goal of targeting these for the treatment or prevention of schizophrenia. The standard treatment has been high doses of antipsychotic medicine which decrease dopamine and other neurotransmitter activity. A recent study, however, demonstrates that a low dose of antipsychotic drugs along with psychotherapy is more effective. This study indicates that the traditional disease model of schizophrenia misunderstands the nature of the condition.

In this paper, I offer a pragmatist interpretation of the significance of this study. I begin by giving an overview of scientific explanations of schizophrenia from the past seventy years, showing how they endorse a disease model. In this disease model, mental disorders are typically assumed to be natural kinds, the nature of which can be discovered through scientific inquiry according to a correspondence theory of truth. Using concepts from William James and John Dewey, I argue that the recent study on schizophrenia treatment discredits this assumption. Instead, this study supports James' idea of truth as "agreeable lending" and Dewey's conception of knowledge as "plastic" and "generative," constructed through lived experience of what "works."

The fact that psychotherapy "works" to help reduce symptoms of schizophrenia requires us to reject the straightforward disease model of schizophrenia in favor of a more complicated bio-psycho-social model. In the bio-psycho-social model, schizophrenia arises from interplay between biological mechanisms and how a person interacts with her environment; treatment, therefore, should address both. The bio-psycho-social model harkens back to Adolf Meyer's theory of schizophrenia as a reactive type: a set of maladaptive reactions developed in response to environmental stress. Writing in the 1950s, at the beginning of the drug revolution that spawned the disease model of schizophrenia, Mayer explicitly rejected the biological essentialism of drug researchers, arguing that human experience and behavior is the expression of continuous reorganization of meaning in response to environmental circumstances. Meyer's pragmatist understanding of human behavior is supported by the recent study.

A bio-psycho-social model that regards environmental influence as important as biological abnormalities in explaining schizophrenia is able to account for sociological research more easily than a disease model can. Current research shows higher rates of schizophrenia in certain populations, including the poor, migrants, and urban-dwellers. While schizophrenia is not necessarily caused by any of these factors, it is no doubt exacerbated by them. Treating schizophrenia thus requires addressing these factors just as much as it requires addressing brain chemistry, a point that is supported by the recent study demonstrating the effectiveness of psychotherapy. The bio-psycho-social model of schizophrenia "works" better than the disease model in accounting for the range of current research, making it "truer" according to a pragmatist theory of truth.

In sum, a pragmatist interpretation of schizophrenia helps us understand both the advancement of scientific research, in terms of what theory "works" better to explain current evidence, and the nature of schizophrenia as a bio-psycho-social disease.

Refutation in historical sciences: Cavalier-Smith's 'Archezoa' hypothesis

Thomas Bonnin
University of Exeter
United Kingdom

Historical sciences, which include disciplines interested in describing and explaining phenomenon of the past, have received substantial interests from philosophers of science. Among the issues treated, scholars were interested in explaining the (temporary or not) validation and refutation of historical hypotheses.

In this talk I will try to study how hypotheses can be refuted in historical science. More precisely, my case study concerns evolutionary biology: I will focus on the 'archezoa' hypothesis, as originally formulated by Tom Cavalier-Smith. This hypothesis is part of an evolutionary scenario to explain the emergence of eukaryotic cells in the history of life. It postulates the existence of a kingdom for eukaryotes that diverged before the origin of mitochondria, another hallmark of eukaryotic cells.

This hypothesis has since been collectively rejected, including by his initial formulator, apparently in the face of solid contrary evidence. It could therefore be seen as a textbook illustration of falsificationism, in which a boldly stated hypothesis has been falsified by further evidence. There are other frameworks in which this refutation could be interpreted, such as Cleland's 'smoking gun' model of historical science, insisting on the discovery of new traces that unambiguously discriminates between the merits of competing scenario. Another one is outlined in Alison Wylie's work, who emphasized, in a study of archaeology, the multiple dimensions of independence in evidential reasoning, and how they can be exploited to give weight or undermine historical hypotheses.

In other parts of my case study that focus on bits of evolutionary scenario which are still under debate, I have argued that these mechanisms for confirmation and refutation were not operative in the way they were described. Sustained disagreement in historical science can be seen as reflecting dissensions over what counts as data (something which Cleland, for instance, does not seem to include in her account). Studying this time an example of clear 'theory choice', this will provide a different type of case in which accounts of the fate of hypotheses, formulated by philosophers of historical science, are put under assessment.

Going beyond the mere explanation of this refutation, this presentation will also explore the fate of failures in the scientific community. To achieve this, I will describe how the rejected 'Archezoa' has become, in some instances, a rhetorical item. A staunch antagonist to Cavalier-Smith, William Martin has in various published contexts utilized the example of the demised kingdom to undermine the credibility of the evolutionary scenario of his opponent as a whole. Once accepted as a failed hypothesis, it seems that the 'Archezoa' hypothesis acquired a new life of its own, somewhat independent from its early context of formulation and its use before its failure.

This talk will therefore address the following questions: what does it take to refute an evolutionary hypothesis? What role does refuted hypotheses plays in scientific practice?

Repertoires: Dynamics of Scientific Collaboration in the Life Sciences (Symposium)

Sabina Leonelli
University of Exeter
United Kingdom

Rachel Ankeny
University of Adelaide
Australia

How effectively communities of scientists come together and co-operate is crucial both to the quality of research outputs and to the extent to which such outputs integrate insights, data and methods from a variety of fields, laboratories and locations around the globe. This symposium brings together scholars working in the philosophy, history and social studies of the life sciences, to focuses on ways to conceptualize practices of scientific collaboration within contemporary research. We examine in particular the notions of repertoire, platform and regime, which have been discussed at length within social science literature but have yet to be discussed and incorporated within philosophical research on the epistemic role and implications of scientific collaborations. We focus on the meaning, potential and limits of the concepts of repertoire, platform and regime, and particularly on (1) the extent to which they capture performative and institutional elements of research activities, and (2) the ways in which they can help philosophers make sense of the dynamics, commitments, constraints and structures involved in the production of scientific knowledge.

Repertoires Integrate Platforms and Research Programs

Elihu Gerson
Tremont Research Institute
United States

Alok Srivastava
Tremont Research Institute
United States

We usually think of research as organized into research programs: linked clusters of work on families of related problems. In recent years, other ways of organizing research have appeared, and it is of interest to consider how they interact with the traditional organization of research programs. Platforms are one such kind of research organization that allows many independent actors to address different problems with similar methods, concepts, and/or models. Platforms differ from specialties by focusing on only a few of the many tasks that must be integrated in a successful research program. Usually, they aren't supported by degree-granting programs. Examples of research platforms include: (1) Model organisms, i.e., captive populations of standardized organisms such as Drosophila. (2) Instrument systems, i.e., techniques and equipment for collecting and analyzing certain kinds of data. These include, e.g., many kinds of microscopy and spectroscopy, as well as computer applications such as Matlab and R. (3) Modeling systems, i.e., models that address a family of problems, such as predator-prey and prisoner's dilemma models, the "central dogma" of molecular genetics, variants of the "tragedy of the commons" in ecology and the "thermodynamic box" used in multiple branches of chemistry. Research platforms are institutions, i.e., collective capacities to perform a set of related tasks, organized as a system of conventions composed, in part, of protocols and "best practices". Such arrangements serve to coordinate activities with much lower costs of coordination. Platforms thus provide researchers with a standardized group of services, materials and other epistemic and material resources that facilitate work in multiple research programs. These services do not simply "plug in" to a program's efforts. Rather, researchers must learn and accommodate the platform's assumptions and requirements, incorporating them into the local repertoires that realize research institutions in particular settings. Platforms and research programs shape one another by constantly recreating and modifying each other via changes to local repertoires. In this view, platforms and programs are both clusters of coordinated institutions realized in the repertoires of participating actors. Because platforms cut across multiple research programs, they enable and bind programs in complex ways. Since changes in the repertoires associated with a platform are constrained by the organization of the platform, platforms limit what individual projects can do. On the other hand, refinements of local repertoires can transform and extend the research programs supported by the platform. In either case, local repertoires form the necessary epistemic and organizational links between research programs and platforms. Such refinements serve as the basis of institutional change in both platforms and research programs. Analysis of the epistemic and organizational interfaces between research programs and platforms, as embodied in the development and organization of repertoires, provides an opportunity for richer understanding of the ways in which practices interact to shape the course of research. Our analysis is fleshed out with examples drawn from genetics, cell biology, and developmental biology.

Replicate that…

Stuart Firestein
Columbia U
United States

Physicist, author and Nobel Laureate Richard Feynman said, "The purpose of science is not to fool yourself—and you are the easiest person to fool." Science has many rules and procedures designed to keep you from fooling yourself, and others. One of them is replication.

According to many sources in the press and the scientific establishment, science is in the midst of a replication crisis, with many published papers containing results that cannot be replicated by other laboratories. Is this really a crisis? Is science in some sort of trouble? Is there rampant fraud? Can science continue to be trusted to provide reliable facts? Are we wasting money on scientific ventures that don't hold up?

The simple answer is none of the above. Science is doing what it always has done—failing with regularity. Replication should never be 100%. Working beyond the edge of what is known, using techniques that are often newly invented, very sophisticated and still not well tested, it should not be surprising that things will occasionally come out wrong, even while they look correct or are only partly correct. Avoiding failure is not only impossible, even attempting to avoid it would be unproductive and, perhaps unintuitively, expensive and time consuming.

How then should we respond to replication failures? They should be published without prejudice. In science revision is a victory. The failure to replicate some result could occur for many reasons and getting to the bottom of it is the job of the scientific community, because getting to the bottom of any failure is how we learn about what we didn't know we didn't know, the very pinnacle of our ignorance. It is ridiculous to say, as some have recently claimed, that 75% of all science is probably wrong. Almost surely 99% is the more correct estimate. Every scientific advance brings up new questions, new conundrums that need to be investigated.

Replication failure is more common in younger sciences than in the mature fields. It is now less common in astronomy, physics and many branches of chemistry, while it seems to plague organismic or systems biology, psychology and social psychology in particular. I suggest that this is because the younger a field the less we know about the variables that are important and that can fool us when we don't control for them. For example, there were initially many failures associated with determining the temperature at which water boils. Understanding that altitude was a critical variable revealed the all important relationship between temperature and pressure—one of the underpinnings of thermodynamics. But initially it just led to puzzling replication failures.

Science would be in a crisis if it weren't failing most of the time. Replication failure is not different than other sorts of important failures in science. Science is a process, a verb not a noun. The mistake is to think that a published paper is the end of the story and a statement of incontrovertible truth. It is a progress report. Don't be fooled.

Replication and Evidence: A Tenuous Relationship (Symposium)

Brian D. Earp
The Hastings Center Bioethics Research Institute
United States

Veronica J. Vieland
The Research Institute at Nationwide Children's Hospital
United States

Stuart Firestein
Columbia U
United States

The past several years have given rise to a cottage industry within the biological and social sciences aimed at documenting and redressing a perceived crisis of non-replication of published findings. Top scientific journals (Science, Nature, Nature Genetics) have published multiple papers on the topic in addition to editorials and special issues; Science News picked "the irreproducibility problem" as one of the top 10 problems of 2015; and many news media have picked up on this theme, including a recent New York Times front page story under the headline "Psychology's fears confirmed: rechecked studies don't hold up."

The premise of all of this activity is that, to quote the editors of Science, "Replication—The confirmation of results and conclusions from one study obtained independently in another—is considered the scientific gold standard." If that is true, and if attempts at replication are truly failing, then perhaps indeed science is in crisis. But is the premise true, and are replication attempts actually failing?

In this session we will consider what constitutes a non-replication, and whether non-replication is necessarily a bad thing. We will argue that what is really of interest is the evidence, but that non-replication in the narrow statistical sense most often invoked tells us nothing about whether the evidence has gone up or down; and that furthermore, even setting aside statistical issues, the formulaic interpretation of non-replication as diminished evidence is scientifically detrimental. Indeed, even the use of successful replication as a method of evidential confirmation is problematic. Drawing on the work of the three speakers in statistical genetics, neuroscience, and psychology, we will consider the relationship between replication and evidence from both philosophical and practical perspectives, contrasting the current state of biology and the social sciences with historical examples from the physical sciences. We will discuss the proper roles of replication in the scientific process, and begin to develop corrective strategies for assessing what we really care about—the evidence—in our home disciplines. We recognize that successful strategies for the future will benefit from careful philosophical and historical analysis of the problem, to complement the ongoing development of appropriate practical scientific investigative procedures.

Replication, Evidence, and Statistical Practice

Veronica J. Vieland
The Research Institute at Nationwide Children's Hospital
United States

Picking up themes from Brian Earp's talk, I will revisit the Reproducibility Project paper in Science, which has galvanized pressure on scientists to demonstrate replicability of results. In genetics, replication is now generally required prior to initial publication in an effort to eliminate sources of bias and error in the literature. But is this a solution, and if so, to what problem?

At issue in current discussions is the notion of replication in only one narrowly construed statistical sense: repetition of small p-values. The assumption is that if a finding, as indicated by a significant p-value in one study, is true, then an independent replication of that study should also yield a small p-value; whereas, if the initial finding is a false-positive, the replication study should yield a larger (non-significant) p-value. Under this assumption we use the second p-value as information about whether the first p-value was a true or false positive. However, I will show in connection with the Reproducibility Project that neither assumption is warranted. The nature of the statistical test ensures that initial findings will usually be followed by non-replication, whether they are true or false (due to a phenomenon widely known as "the winner's curse," a form of regression to the mean, as I will explain).

Moreover, it is even problematic to interpret successful statistical replication as confirming a finding, as this involves a form of magical thinking in which the product of two independent p-values carries more evidential weight than a single p-value of the same magnitude. And while we may reasonably expect a false-positive initial finding to be followed by a negative replication result, care must be taken to distinguish statistical false positives from scientific red herrings: a statistically significant correlation may be real, yet spurious in the sense that the two correlated things are not causally related. Indeed, fields like genetics that require statistical replication may be "succeeding" by weeding out truly interesting findings, which are more likely to fall to the winner's curse, in favor of replicable yet causally spurious correlations.

The fundamental problem is that what we are really interested in is the evidence, but the statistical framework being used in lieu of direct consideration of the evidence ties us in logical knots and obscures the evidence itself. I will argue that, far from being a gold standard for experimental science in general, replication is primarily used in the physical sciences for the calibration of instrumentation and protocols, and only rarely to verify a finding obtained using established calibrated techniques. In the biological and social sciences, we have conflated this use of replication with the unrelated requirement of repeated small p-values. But we have a pressing calibration problem of our own: How do we measure the strength of statistical evidence on a properly calibrated measurement scale? Only once that problem is solved will we be able to tell whether the evidence is going up or down with the accumulation of new data, and whether science is in crisis or not.

Representation and the Analogy between Scientific Models and Maps: Insights from Cartographic Epistemology and Practice

Gui Oliveira
University of Cincinnati
United States

A common view in the recent philosophy of science literature is that the representational relationship between a model and the target phenomenon it simulates is what secures the epistemic value of model-based scientific research. This representationalist assumption is often supplemented by implicit or explicit analogies between scientific models and maps. In this paper I draw from the history and philosophy of geography to argue that a better understanding of maps and map-making motivates either rejecting the analogy between models and maps or rejecting representationalism about models.

Theories of scientific representation abound: there are views based on isomorphism (van Fraassen 2008) and similarity (Giere 2004, 2006, 2010), accounts dubbed "inferential" (Suarez 2003, 2004), "interpretational" (Contessa 2007), and "semiotic" (Knuuttila 2010), as well as deflationary approaches (Suarez 2015; Morrison 2015). Despite the variety, most in the literature frame the epistemic value of model-based science in terms of how models represent their target: models enable indirect investigation of some phenomenon insofar as they represent (more or less accurately) the phenomenon they simulate. This representationalist view has included analogies with maps and map-making. Because of the known distortions and inaccuracies contained in models, it is necessary to determine what the "representational mapping" is, i.e. what aspects of a model represent what aspects of the world and in what ways (Weisberg 2013; Van Fraassen 2008; Giere 2010). Further, some authors have explicitly compared scientific models to maps, suggesting that models are like maps in that their quality depends on how well they represent some existing particular (see e.g. Bailer-Jones 2009 after Giere 1999).

Despite the popularity of representationalism and the seeming plausibility of the model-map analogy, serious consideration of the history and philosophy of geography motivates a different perspective. Traditional views in cartographic epistemology have tended to frame the history of cartography as "progressive, cumulative, objective," with maps "continually evolving in their accuracy" (Martin 2011, p. 148). In contrast, post-representational cartographic epistemology emphasizes how "mappings" are used for addressing "relational problems" (Kitchin et al 2013, p. 482) and therefore should be treated as "collaborative artefacts" (p. 483). Rather than the ontology of maps (i.e., how maps attempt to describe what exists), in this view the essential feature of maps is their ontogenic role (i.e., how maps play a dynamic role in problem solving). I discuss the main ideas of cartographic post-representationalism, elaborating on this view by examining the "artifactual turn" in medieval cartography that led to the rise of modern maps. Moving away from richly adorned scholastic maps which had clear "didactic and moralizing" objectives, the late 1200s sees the invention of portolan charts "made by seamen for seamen" (Livingstone 1992, p. 50). These were geometrical versions of written records and instructions (Hofmann et al 2013), designed as tools to be used alongside other nautical instruments (Crone 1978; Unger 2010). I argue that the historical evidence and the post-representationalist perspective in cartographic epistemology motivates either rejecting the model-map analogy or taking seriously the idea that scientific models are tools or artifacts, not truth-bearing representations.

Research Ethics, Epistemic Trust and Bad Actors in the Retraction Watch Era

Barton Moffatt
Mississippi State University
United States

There has been growing awareness of the interconnections between the ethical and epistemic aspects of scientific practice. Philosophers of science are increasingly interested in forging pathways between methodological and ethical issues. In this paper, I argue that one source of ethical obligations in research stems from peoples' responsibilities as members of a trust-based, epistemic community and that, if so, we need to think about community solutions to research misbehavior from an epistemic perspective. Research communities are organized around the pursuit of scientific knowledge; there are thus research ethics obligations due both to being part of a community and to being organized in pursuit of knowledge.

Thinking of research ethics from the perspective of epistemic communities allows us to consider whether research communities have adequate mechanisms in place to keep the community healthy by identifying untrustworthy research. Recent high profile cases of research misconduct reveal some of the ways in which researchers can abuse the trust-based nature of scientific research. For example, the Dutch social psychologist Dietrich Stapel recently admitted to publishing 58 fraudulent papers by the wholesale fabrication of data. Peer review assumes that data is produced honestly. People who cheat can produce excellent results that hold up to peer review because the peer review system is built on a foundation of trust. Some people argue that the answer is to move to an accounting model in which audits ensure that people act appropriately. I argue that a better alternative is to create better avenues to identify and punish research misconduct.

Ironically, most research misconduct is never publically acknowledged because people who have knowledge of bad behavior have legal, institutional or personal reasons for not informing the relevant epistemic communities of untrustworthy research. The recent emergence of the blog Retraction Watch (<retractionwatch.com>) has remedied this situation by opening up a window on bad behavior. The blog collects retraction notices in research journals and presses journal editors to be more transparent in situations in which retraction notices are vague or lacking details. As a result, it has become the go-to location for information on research misconduct. However, it is a bit unfortunate that this is emerging as the only avenue for disclosures of unethical behavior since journal retractions also serve an important role in ethically identifying honest mistakes and incorrect results. I argue that, ideally, we would have one system that identifies people who have proven themselves to be unworthy of trust and another for trustworthy people who need to communicate mistakes. However, it is not clear whether another pathway exists given the various institutional pressures on the reporting of unethical acts.

Residual Phenomena and the Mechanism of Deep Earthquakes

Teru Miyake
Nanyang Technological University
Singapore

Detailed investigation of residual phenomena, or systematic discrepancies between theoretical predictions and actual observations, have sometimes led to the discovery of novel phenomena or causes that were previously unknown or unaccounted for. Residual phenomena have played an important role in the history of astronomy, for example, as shown in George Smith's work on gravity theory after Newton. A natural question that arises with regard to residual phenomena is: When should we take a residual phenomenon to be indicative of a real and significant phenomenon?

This paper will engage this question through the examination of a contemporary example of a residual phenomenon, related to the question of what the causal mechanism is of deep earthquakes. Most seismic sources are within 60 km of the earth's surface, but some are known to occur at much deeper depths, sometimes in excess of 600 kilometers. These deep earthquakes are something of a mystery, because there is some question as to whether rock at that depth is strong enough to support brittle fracture, the mechanism that is taken to be the cause of shallower earthquakes. Some seismologists have suggested a different mechanism as the cause of deep earthquakes, such as sudden phase changes deep within the earth. But because we do not have direct access to these sources, all of our knowledge about them must be inferred from observations of seismic waves and any other knowledge (e.g., the chemistry and physics of rocks, thermodynamics) that we can bring to bear.

Information about seismic sources can be obtained through an examination of the radiated pattern of seismic waves that can be observed on the earth's surface. Theoretical results in the 1960's showed that the radiation pattern from slip along a fault, which is the expected mechanism of shallow earthquakes, would be equivalent to that from a double couple, a particular kind of idealized point source of elastic radiation. Radiation patterns from some deep earthquakes, however, have been found to have significant departures from those of a double couple (more specifically, they have a significant CLVD component in the seismic moment tensor). Such residual phenomena might be taken to be indicative of a different mechanism at work in deep earthquakes. The question is whether these departures are systematic and robust enough to indicate the existence of another, as yet unknown, causal mechanism. Frohlich (2006), the most comprehensive book written on the subject of deep earthquakes, says "no". This paper will examine the reasons for this negative assessment, and compare it to cases of in the history of astronomy where residual phenomena were successfully used for the discovery of previously unknown causal factors at work in the solar system.

Risk, Judgments, and Objectivity In Medical Research—Evaluating the Strengths and Weaknesses of Meta-Analysis

Saana Jukola
Bielefeld University
Germany

In evidence-based medicine, evidence hierarchies represent the assumed strength of different types of evidence. Meta-analysis, a method of synthesizing information from numerous studies by using statistical techniques, is typically considered to provide the strongest evidence. This is because the prespecified formal rules for conducting analyses are thought to constrain individual judgments on the part of analysts and, thus, minimize bias. Stegenga (2011) has argued that the high evidential status of the method is not warranted because conducting a meta-analysis necessarily involves judgments, which means that biases may enter the process. He also states that relying on meta-analyses "comes with possibly significant epistemic risk" (Stegenga 2011). In this paper, I show that Stegenga's argument, despite correctly identifying some problematic practices related to meta-analyses, is based on an ideal of objectivity that is both unattainable in practice and insufficient in principle. I suggest that the strengths and weaknesses of meta-analysis need be analyzed by using a conception of objectivity that makes it possible to evaluate the judgments that are made with regards to different risks involved in the production of medical knowledge. Despite the simplicity of the general principles of performing a meta-analysis, there are multiple stages of the process that force an analyst to make decisions on how to proceed. Because these decisions require "[—] judgment and expertise [—]", Stegenga holds that meta-analysis fails to be an objective method (Stegenga 2011, 505). This criticism is based on a conception that Douglas (2004) has called procedural objectivity. This understanding of objectivity bans all judgments and does not make distinctions between acceptable and unacceptable judgments. I suggest that instead of adopting this conception of what objectivity is, we should evaluate the methods of producing medical knowledge by assessing the decisions that researchers make by taking into account the epistemic and non-epistemic goals of the inquiry. As mentioned, Stegenga believes that relying on meta-analyses comes with significant epistemic risks. This is because meta-analyses are successful in testing certain types of hypotheses but may be less strong in producing some types of relevant evidence, including evidence on the possible side effects of drugs. I argue that aiming at procedural objectivity does not take into account that avoiding errors with severe consequences may require expert judgment and legitimize setting the criteria of sufficient evidence differently in different contexts. Consequently, using this conception of objectivity for evaluating practices in medical research is not fruitful. Moreover, adopting the procedural conception of objectivity does not make it possible to analyze why placing meta-analysis on top of the evidence hierarchies can be problematic.

Douglas, H. (2004). The Irreducible Complexity of Objectivity. Synthese, 138, 453-473.
Stegenga, J. (2011). Is meta-analysis the platinum standard of evidence? Studies in History and Philosophy of Biological and Biomedical Sciences, 42, 497-507.

The Search for Kuhn-loss: A New Strategy for HPS

Jamie Shaw
Western University
Canada

The notion of 'Kuhn-loss', or the loss of puzzle-solving ability in a successive paradigm, has received extremely little attention in the secondary literature (cf. Chang 2012). This is surprising since it is one of the key points against the thesis that the history of science is linearly cumulative. My paper makes three contributions in this area: (i) I articulate a clearer conception of Kuhn-loss, (ii) demonstrate its theoretical and practical importance using two historical examples, and (iii) show the advantages the search for Kuhn-loss possesses over other strategies in HPS. Kuhn-loss is an extremely ambiguous notion. For instance, it is unclear whether Kuhn-loss means successive paradigms are initially or permanently unable to solve the puzzles of its predecessor. Kuhn's example of the recovery of phlogiston theory's explanation of the qualities of chemical substances suggests that Kuhn-loss can be recovered by succeeding paradigms but not that it is always recovered. This provides a distinction between recovered and unrecovered Kuhn-loss. Additionally, while Kuhn thought puzzle-solving was the primary virtue of a paradigm (and a marker of progress (cf. Laudan 1978)), it is unclear why we should conceive of Kuhn-loss in these terms rather than other epistemic virtues (e.g., explanation, prediction, etc.). Some puzzles are not worth solving from a contemporary standpoint (i.e. what the balance of the four humors is in a patient). Because of this, I argue that we should reformulate Kuhn-loss with other epistemic virtues to give us a notion of Kuhn-loss that is worth regaining. Several historical examples help develop the notion of Kuhn-loss and its contemporary importance. For example, consider the revival of the cosmological constant which was introduced to relieve the tension between Einstein's view of gravity and the static universe model, but has since been successfully revived in the study of dark matter. Or, consider Priestley's electrochemistry which was theoretically reconstituted in Lavoisier's combustion theory to expland its explanatory power (Chang 2012). These examples elucidate how regaining Kuhn-loss has been historically fruitful and contributed to the development of science. Thus, we have strong inductive reasons to continue this activity. For (ii), Kuhn-loss not only has important implications for how we conceive of the history of science, (i.e. as non-linearly progressive), but also suggests a new avenue for engaging with science. This provides a new set of tasks for historians and philosophers: to find instances of genuine Kuhn-loss, recover them, and apply them to contemporary frameworks. I go on to make sense of this task and motivate its importance within Feyerabend's account of pluralism (1970, 1975, and 1978). For (iii), I argue the search for genuine Kuhn-loss provides a better way of conceiving of the relationship between the history and philosophy of science. Rather than using historical examples to confirm or disconfirm philosophical theories (Laudan 1981; Worrall 1989; Psillos 1999) or using historical examples to illustrate philosophical theories (Heidelberg and Stadler 2001 and DeWitt 2011), the search for Kuhn-loss provides a method for directly engaging with scientific practices and aiding in the development of theories.

Securing Science in a Protean World (Symposium)

Nora Boyd
University of Pittsburgh
United States

David Colaço
University of Pittsburgh
United States

Aaron Novick
University of Pittsburgh
United States

Raphael Scholl
University of Cambridge
United Kingdom

Every stage of the scientific process confronts the issue of stability. Scientists wish for their theories to be responsive to the nature of the world, but theories are compared directly only to evidence, and to the world itself. All evidence is at least minimally interpreted, and these interpretations change over time. Is there any sort of evidence that can serve as a stable constraint on theorizing? In addition to confronting theories with evidence, scientists also try to identify scientific phenomena (independently of any commitment to an explanation thereof). These phenomena are discovered and validated through the use of diverse experimental protocols. The hope is that the "same thing" can be identified in all cases, but how is such an identification of stable phenomena achieved? And, lastly, scientists seek to discover entities and processes that cannot be directly observed. But, as debates over theory change in science have shown, the firmly established entities of one era are the refuted "speculations" of the next. How can we be confident that unobservable entities of contemporary science will not suffer the same fate?

The talks in this session all address the ways in which scientists address these problems of in-stability. Nora Boyd's talk, "The Tree, the Labyrinth, and the Minefield," looks at the strategies astrophysicists have adopted in order to generate good evidence, and shows how, with sufficient care, such evidence can serve a stable constraint on theorizing even as it is reinterpreted over time. David Colaço's talk, "The Identification of Scientific Phenomena," analyzes the processes by which neuroscientists and psychologists identify scientific phenomena across distinct experimental contexts, showing how these processes are geared to generate identifications of stable, repeatable occurrences. Aaron Novick's talk (based on a paper co-authored with Scholl), examines the standards of reasoning that biologists apply to claims about unobservable entities and processes, showing how these standards allow biologists to generate knowledge of these entities and processes that, as a rule, grows cumulatively, even as biological theories change. Finally, Raphael Scholl's talk, "Stability without Stasis: Ambition and Modesty of Realism about True Causes", develops an account of cumulative growth in causal knowledge. Taking the his-tory of genetics as a case study, he highlights sources of both stability and change: even after DNA had been accepted as a true cause of hereditary traits, there remained ample room for doubt, revision and extension.

Simplicity as Integrity in Galileo's Dialogue Concerning the Two Chief World Systems

Daniel Schwartz
Rowan University
United States

The early modern period offers a rich variety of perspectives on the role of simplicity in a proper scientific method. It offers everything from highly original defenses of the virtue of simple explanations—such as Leibniz's idea that God created the best of all possible worlds—to attacks on simplicity, such as those of Francis Bacon, which are rooted in reflection on human psychology. Far more commonly, though, the natural philosophers of the time appear to take it as an axiom, unquestioned since the time of Aristotle, that nature acts in the simplest of ways.

That is certainly how Galileo appears to treat simplicity, if you look only to his explicit commentary. In Two New Sciences, he tells us to "remember the very true Aristotelian principle saying that it is useless to do with more means what can be done with fewer" since nature "habitually employs the first, simplest, and easiest means." (141, 153).

The goal of this paper is to show that Galileo's practice—the way in which he actually employs the concept of simplicity in the course of particular arguments—offers a far more interesting perspective, one where simplicity is often best understood as integrity. A simple explanation is one which fits together into a unified whole. The virtue of simplicity may then be derived from the unity of reality and truth.

Galileo's most suggestive gesture in this direction is in the following passage from the Dialogue Concerning the Two Chief World Systems.

"Copernicus himself writes, in his first studies, of having rectified astronomical science upon the old Ptolemaic assumptions, and corrected the motions of the planets in such a way that the computations corresponded much better with the appearances, and vice versa. But this was still taking them separately, planet by planet. He goes on to say that when he wanted to put together the whole fabric from all individual constructions, there resulted a monstrous chimera composed of mutually disproportionate members, incompatible as a whole" (Drake 341).

In the Copernican system, on the other hand, "the whole […] corresponded to its parts with wonderful simplicity." I survey a number of Galileo's other simplicity arguments—focusing on his series of simplicity arguments in favor of the Copernican theory—and argue that many (though not all) of them fit this mold.

An advantage of this interpretation is the fact that it requires no insight into God's intentions. Galileo says that "it is rash to want our very weak reason to be the judge of God's works, and to call vain and superfluous anything in the universe which is of no use to us." If this is right, then, perhaps God has goals which require Ptolemaic contrivances for which we see no need. But that doesn't matter. God created a single reality, and that reality must form a coherent whole.

The Socio-Technical Epistemology of Clinical Decision-Making

Sophie van Baalen
University of Twente
Netherlands
Annamaria Carusi
University of Sheffield
United Kingdom

Most of the literature on the epistemology of clinical decision-making has taken the individual physicians as the central object of analysis. We will argue that rather than focusing on the individual doctor's reasoning and knowledge, it is more fruitful to think of clinical decision-making as a form of social knowing, in which technologies play a key mediating role. In such a system, decision-making cannot be performed by any individual, but is instead performed by an assemblage of people and instruments in coordinated actions.

In medical practice, these assemblages consist of practitioners with different expertises, such as clinicians, radiologists, nurses and many others. Such assemblages therefore entail interdisciplinary distributions of knowledge and know-how. Physicians must assess the reliability of information provided to them from different sources, including judgments and interpretations from others, data provided by technologies, and measurements performed by instruments.

This paper examines clinical decision-making through a detailed study of image assisted diagnosis and treatment of a pulmonary disease. The study shows the social knowledge processes involved among the different epistemic agents. We show the extent to which trust plays a pivotal role in epistemic dependence (Wagenknecht, 2015), but at the same time, how trust is mediated by the imaging technologies in this 'space of reasons' (Carusi, 2009). In repeated interactions, medical teams cultivate a collection of stable, agreed upon orientations towards evidence and knowledge that provides an inter subjective framework within which claims and interpretations can be justified, and decisions can be arrived at and shared by others. Medical images, such as X-rays and MRI scans, play an important role in these assemblages of distributed knowing, by allowing to build up these inter-subjective frameworks of trust practices. Images fulfill this role in two ways, firstly as epistemic objects that can be distributed among all members of a team, as well as interpreted and discussed, thus facilitating communication and sharing of information in a specific way. In addition, the sharing of images and communicating through them produces shared vision (Goodwin 1994), through which the members of the assemblage come to see and perceive in a common way. This reinforces the trust framework within which medical decision-making operates.

The socio-technical epistemology we propose has important implications for the epistemological responsibility of physicians (Van Baalen & Boon, 2014): within these assemblages of distributed cognition, physicians and others take moral as well as epistemological responsibility for clinical decision-making. This requires an account of what it means to trust well within a socio-technical assemblage for clinical decision-making.

In summary, in this paper we will analyze clinical decision-making within a medical expert team involved in diagnosis and treatment of patients with a severe an complex disease called pulmonary hypertension to show how these complex systems of distributed knowing function in clinical practice, how trust is cultivated among medical teams and the role played by images within these processes.

References

S. Wagenknecht (2015) Facing the incompleteness of Epistemic Trust: Managing Dependence in Scientific Practice, Social Epistemology, Vol. 29(2), pp. 160-184

A. Carusi (2009) Implicit trust in the space of reasons and implication for technology design, Social Epistemology, Vol. 23 (1), pp. 25-43

S. Van Baalen & M. Boon (2015) An epistemological shift: from evidence-based medicine to epistemological responsibility. Journal of Evaluation in Clinical Practice, Vol. 21(3), pp. 433-439

C. Goodwin (1994) Professional vision, American Anthropologist 96(3) 606-633

Stability and Scientific Realism: The Coming of Age of Perturbation Theory

Massimiliano Badino
Universitat Autònoma de Barcelona, Massachusetts Institute of Technology
United States

A sophisticate form of scientific realism claims that one's epistemic commitment should go on those elements of our mature and robust theories which actually play a truly active role in explaining the world. This thesis is supposed to meet the challenge of the pessimistic meta-induction according to which, since theories in the past have been regularly superseded by other theories and this is likely to happen to ours, no realist commitment on current science can be rationally justified. At a first glimpse, the pessimistic meta-induction appears unquestionable: theories of the past have certainly been replaced by other theories. However, the realist retorts that one does not need to show that theories are always globally true: "It is enough to show that the theoretical laws and mechanisms which generated the successes of past theories have been retained in our current scientific image" (Psillos 1999, 103). Even this cursory survey, makes clear that the realist program requires a substantial support from history of science. Take, for instance, the concept of "mature theory". According to Anjan Chakravartty, "a mature theory is one that has survived for a significant period of time and has been rigorously tested, perhaps belonging to a discipline whose theories typically make novel predictions" (Chakravartty 2007, 27-28). Only concrete historical analysis can tell us whether the survival period of a theory was significant and whether the tests it passed were rigorous. However, the integration of philosophical and historical research is a notorious bone of contention. In this paper, I claim that history can significantly help the cause of scientific realism by investigating the strategies used by scientists to "stabilize" their theories—i.e., to produce what philosophers call a mature and successful theory. These strategies include the interrelation of a theory with other, already stable, portions of science, the use of more and more severe observational testing, the generalization and simplification of the formal structure, the improvement of the symbolic notation, and so forth. The common denominator of these procedures is to improve the control on the theory and to distinguish artifacts from genuine information on the world. In turn, this philosophical perspective can also produce novel and interesting historical narratives. I explore a concrete historical case, that is the development of perturbation theory and the early treatments of the problem of stability of the solar system. I claim two theses. (1) Contrary to the common wisdom, the problem of stability became possible only when perturbation theory reached a level of sophistication that allowed mathematicians to solve it. (2) Lagrange and Laplace adopted very different strategies to make perturbation theory a reliable tool for astronomical investigation. In particular, Lagrange tried to improve it, so to say, from within, i.e., by generalizing its practices and by simplifying its methods. Instead, Laplace tried to test perturbation theory on concrete problem and to control it by using other resources, for instance probabilistic analysis of causes. This story can offer us a useful window on the methods deployed to create successful and enduring theories.

Stability without stasis: Ambition and modesty of realism about true causes

Raphael Scholl
University of Cambridge
United Kingdom

Realists about true causes (Novick, this session) argue that stable claims in the life sciences meet the *vera causa* ideal: it must be demonstrated (1) that a putative cause exists, (2) that it is competent to produce a particular type of effect, and (3) that is responsible for that effect in a particular instance. Life scientists themselves recognize claims that are established to this high standard as much more robust than those whose main virtue is their explanatory power.

In addition to identifying stable claims, *vera causa* realism also offers many resources for understanding instability and controversy. It gives an account of the revisions and expansion we should expect around established true causes. For instance, biochemists in the middle of the 20th century established quite firmly that DNA is a cause of hereditary traits in pneumococci. This was accepted only after a long series of experiments and extended debate, after which most researchers came to agree that DNA could be extracted to a sufficient level of purity for its causal competence to be demonstrated, and that the experiments successfully excluded confounders. However, this hard-earned consensus about a true cause did not stifle the further growth of knowledge. To the contrary: it provided a framework for it.

First of all, there was the possibility of *alternative pathways* of heredity. Even though a heritable polysaccharide capsule had been transferred from one strain of pneumococci to another, this could have been merely a superficial trait. The question remained whether “deeper”, species-specific characteristics had the same physical basis, or whether they were, perhaps, inherited via a more complex protein or nucleoprotein. In the *vera causa* framework, this is the question of responsibility: a cause of hereditary traits had been found, but how many instances of such traits were in fact attributable to that cause? Second, causes are generally not by themselves sufficient for bringing about an effect: for DNA to play its causal role, a number of *co-factors* are required that enable it to be duplicated and transcribed in a coordinated way. DNA does not do much without polymerases, helicases, topoisomerases, and a host of other proteins, all of which required extensive research and shaped our view of how DNA exerts its effects. Third, the *intermediate steps* linking DNA to phenotypic traits remained entirely open. It was far from clear that the lacunae would be filled in the way that they have been. It was initially speculated, for instance, that DNA itself possessed catalytic activity by which it directed cellular function and differentiation. There were also some mechanisms that explained away the experimental results. Some researchers suspected, for instance, that the explanation for the transformative effect of pneumococcal DNA was almost the reverse of what we now know to be the case: DNA was taken to enclose and insulate a protein that was the true carrier of hereditary information, so that our interventions (such as transferring or destroying DNA) only made it appear as if DNA produced hereditary effects.

In summary, realism about *verae causae* is far from a throwback to an overreaching, static view of scientific knowledge. Claims about true causes can turn out to rest on mistaken causal inferences (for instance, if confounding occurred), in which case revisions are indicated: scientists do not accept experimental results lightly. But even where the ideal is in fact met, the result is anything but stasis. Claims about true causes invite the search for additional ones: alternative pathways, co-factors and intermediate steps must be investigated. There is thus a rich dynamic of growth in our knowledge even while some claims that meet the *vera causa* ideal can be accepted as simply and stably true.

The Strategy of Engineering Cost Analysis?: Independence within Aerospace Cost Analysis Techniques

Zachary Pirtle
George Washington University
United States

Jay Odenbaugh
Lewis and Clark College
United States

Zoe Szajnfarber
George Washington University
United States

Engineers and scientists eventually have to answer: 'How much will it cost?' You can only do as much research and development (R&D) as you can afford, after all. Such questioning may become more important, as recent scholars have called for more rigorous R&D evaluation (Bozeman and Sarewitz 2010). However, there is a long history of challenges in fulfilling promises about the cost and schedule of R&D. A major confounding factor is the complexity of R&D tasks throughout engineering and science. It can be difficult to assess exactly what progress is made at a given time within an R&D project, much less predict exactly what it will take to perform the task, therefore making it difficult to predict cost. Any future approaches at R&D evaluation need to improve at controlling the complexity that has frustrated past attempts at cost control.

One way of improving how a field such as cost analysis handles complexity is to assess the extent to which the field can utilize diverse and independent analysis techniques to understand a problem. In the classic 1966 article, "The Strategy of Model Building in Population Biology," Richard Levins reviewed three different types of ecological models that made predictions about evolution in uncertain environments. Levins deemed it a virtuous 'strategy' in population biology that each model had its own unique assumptions, each of which might be wrong, but that if the collection of independent tools converged on a single conclusion, they would lead to a more likely 'robust' truth.

As a 'strategy', using an independent ensemble of tools to assess a complex system can be a more reliable way to make accurate predictions and to characterize progress. Some researchers have explore different disciplines to examine whether the models within a field have had sufficient diversity in tools (Wade 1982).

To inform a broader conversation about how we evaluate R&D costs, we will assess independence among cost and schedule analysis techniques from one specific context: aerospace systems development. Specifically, we will discuss approaches and tools about how NASA works on projecting costs for major engineering developments. There are four general methods for cost estimation in aerospace: parametric cost modeling, engineering build up estimates, estimates by analogy, and extrapolations from actuals.

Using an independence analysis approach to examine prominent cost analysis tools, we show both independence and interdependence among the tools. Some tools only cover one variable for programmatic analysis, such as focusing on cost, risk or schedule alone, while others try to combine multiple variables into one model. There are also analytical limits to each tool, such as flexibility in supporting what if scenarios and ability to assess uncertainty. We will discuss recent trends to only utilize a single, comprehensive model for cost analysis, as well as overview what an alternative combination of simple models may look like.

Exploring independence and the limits of different cost analysis tools may be an important part of maturing cost analysis to support better cost analysis as well as broader R&D evaluation techniques.

The Structural Heterogeneity of Scientific Concepts

Hyundeuk Cheon
Ewha Womans University
South Korea

Given that scientific concepts are arguably a kind of concepts, some philosophers have attempted to develop the theories of scientific concepts and conceptual changes by exploiting empirical psychology of concepts (Thagard 1986; 1990; Nersessian 1989; Andersen et al. 2006). The methodological strategy they adopted is to selectively pick up a subset in a wide range of psychological literature on concepts, and to apply it to scientific concepts. However, this strategy fails to take a comprehensive perspective and overlooks an embarrassing situation in the empirical studies about concepts: several theories of concepts (the prototype theories, the exemplar theories, and theory theories) are still competing, and there is no consensus on the nature of concepts. Strikingly, no single theory of concepts, which takes one type of informational structure (i.e., prototype, exemplars, and theories) as the basic building block for human higher cognition, has failed to provide a satisfactory explanation of all the experimental results. Some results can be easily explained by one theory, but not by others, and vice versa. The challenge we face is to specify how to accommodate all of them. In this paper, it is suggested that concepts are structurally heterogeneous in that each category may be represented by multiple types of informational structures, each of which is involved in most, if not all, of higher human cognition. Not only does this structural pluralism make a sharp contrast with the monolithic view that concepts are the unified entities underlying higher cognition, but it is distinguished from other radical solutions like Machery's (2009) concept eliminativism. The adoption of the structural heterogeneity of scientific concepts, I will argue, allows us to illuminate the conceptual problems, where there are no agreed-upon definitions of central theoretical terms such as 'species' and 'human nature'.

The Structured Use of Concepts as Tools in Neuroimaging Experiments

Eden Smith
University of Melbourne
Australia

By exploring the interdependent histories of two concepts, this paper examines the structured use of each concept as a tool for investigating discrete epistemic aims in neuroimaging experiments. Studies into the use of concepts in experiments include descriptions of concepts as tools (Feest 2010, 181-182), and as embodying epistemic aims (Brigandt 2012, 78). As with other recent historical and philosophical studies of conceptual practice, these accounts explore how the contributions of concepts to empirical knowledge extend beyond their roles in mental and linguistic representation. Descriptions of concepts contributing to experimental practice can also be found within early twentieth-century historical epistemology (Méthot 2013; Hans-Jörg Rheinberger 2005; 2010; Schmidgen 2014) as well as within technoscientific studies (Pickering 2006). These accounts complement recent approaches to concept-use by offering a view of concepts as functioning within networks of associations that structure concept interactions within the historically contingent, and temporally dynamic, processes of scientific practice. The intersection between these different approaches to conceptual practice suggests that the use of a concept as a research tool is structured by routinized networks of entrenched associations. It is this intersection that is developed by examining how two interdependent concepts came to be used as independent research tools. This examination begins with a brief account of how the concepts of mental imagery and hallucinations, as used in relation to each other during the nineteenth and twentieth centuries, came to be distanced from each other by an inverse set of characteristic traits. These characteristic traits will then be presented as embodying a network of routinized associations that structure a systematic organization of concept relations within experimental practice. This historical context highlights how the relationship between the concepts of mental imagery and hallucinations provides the structures within which each can be used as an independent tool for investigating a discrete epistemic goal in neuroimaging experiments.

References

Brigandt, Ingo. 2012. 'The Dynamics of Scientific Concepts'. In Scientific Concepts and Investigative Practice, edited by Uljana Feest and Friedrich Steinle, 75-103. Berlin Studies in Knowledge Research, volume 3. Berlin: De Gruyter.

Feest, Uljana. 2010. 'Concepts as Tools in the Experimental Generation of Knowledge in Cognitive Neuropsychology'. Spontaneous Generations: A Journal for the History and Philosophy of Science 4 (1): 173-90.

Méthot, Pierre-Olivier. 2013. 'On the Genealogy of Concepts and Experimental Practices: Rethinking Georges Canguilhem's Historical Epistemology'. Studies in History and Philosophy of Science Part A 44 (1): 112-23.

Pickering, Andrew. 2006. 'Concepts and the Mangle of Practice: Constructing Quaternions'. In 18 Unconventional Essays on the Nature of Mathematics, edited by Reuben Hersh, 250-88. New York: Springer.

Rheinberger, Hans-Jörg. 2005. 'Gaston Bachelard and the Notion of "Phenomenotechnique"'. Perspectives on Science 13 (3): 313-28.

Rheinberger, Hans-Jorg. 2010. An Epistemology of the Concrete: Twentieth-Century Histories of Life. Experimental Futures: Technological Lives, Scientific Arts, Anthropological Voices. Durham [NC]: Duke University Press.

Schmidgen, Henning. 2014. 'The Life of Concepts: Georges Canguilhem and the History of Science'. History and Philosophy of the Life Sciences 36 (2): 232-53.

Terminological Ambiguity in Cross-Disciplinary Collaboration

Julie Mennes
Ghent University
Belgium

Cross-disciplinary collaboration (CDC) transcends disciplinary boundaries. As there are no requirements regarding the extent or type of integration of the disciplines involved, CDC includes inter-, multi- and transdisciplinary collaboration. (O'Rourke & Crowley, 2013). The literature on cross-disciplinarity is extensive, including many writings on language-related problems for CDC. My contribution addresses an imbalance between discussions of language-related problems and the methods which have been developed to deal with these problems.

Each discipline has its own jargon. Combining (parts of) multiple jargons gives rise to misunderstandings of terms. Even though researchers are not eager to share their struggles, several reports can be found on projects being impeded by such misunderstandings. (e.g. Ranade et al., 2011; Bracken & Oughton, 2006) For example, a collaborative effort to develop an algorithm for a medical expert system was hindered because the engineers had erroneously equated the medical expert's terms 'diagnosis' and 'outcome'. (Dugard, 1997) The phenomenon underlying misunderstanding of terms, is that of terminological ambiguity, i.e. terms having multiple meanings across disciplinary jargons. In the case of the expert system, medical experts use 'diagnosis' in a pre-treatment context and 'outcome' in post-treatment contexts, whereas engineers consider 'diagnoses' to be the 'outcome' of their algorithm.

CDC requires communication across disciplines, and hence, integration of jargons. The resulting terminological ambiguity has been acknowledged as an important cause of problems for CDC, both by researchers studying CDC and researchers engaged in CDC. For example, Thompson (2009) says that "[i]nterdisciplinary colleagues may have different meanings for the same words, or not even recognize some terms used by team members with different expertise", and Benda (2002) notes that "[o]bstacles often arise in collaborative efforts for several well-known reasons. First, it is often difficult to find a common language because of disciplinary specialization."

Methods for dealing with language-related problems in CDC take an indirect approach on terminological ambiguity. Some researchers developed methods to address obstacles such as group communication (e.g. Naiman, 1999) or poor knowledge of other disciplines (e.g. Heemskerk, 2003). Their hope seems to be that addressing these obstacles will simultaneously resolve misunderstandings arising from terminological ambiguity. Others, such as O'Rourke and Crowley (2013), characterize terminological ambiguity as an effect of more fundamental differences. Each discipline has its own worldview, i.e. a set of metaphysical and epistemic assumptions, which shapes the knowledge generated within the discipline. As the language used within a discipline is inextricably bound to the underlying worldview, terminological ambiguity across disciplines is a (mere) reflection of differences between worldviews. Their method allows to increase the mutual understanding of each other's worldviews.

Thus, even though terminological ambiguity is explicitly and repeatedly recognized as a cause of problems for CDC, there is no method with allows to address ambiguous terms directly and systematically. In my papers, a new method is presented which integrates techniques from computational linguistics with insights from philosophy of science. It allows to detect and deal with terms which are ambiguous in the context of a given CDC.

There and Back Again: Comparative Resituation of Models Between Ecology and Political Economy

Jason Oakes
University of California, Davis
United States

Borrowings, propagation, and translocation of models is a significant source of scientific novelty and one of the drivers of innovation in research. This process should fit into the framework of the integration of disciplines, but there are complications. The relata in these processes are often from distantly related specialties not in the process of developing closer ties of integration. More challenging for the analyst, in many cases at least one of the related lines of work lie outside of academic research. This presents a challenge to clarifying these processes conceptually, which may be eased by historical examples.

To this end this paper does two things. First, I revise Weisberg's anatomy of models (structure, construal, target generation) and their use (abstraction, idealization, weighted feature-matching) so they can be used to describe the movement of models between different research contexts. Second, I offer a comparison of two historical cases to explore regularities and contrasts in the journeys of different models.

Case 1: Although the Lotka-Voterra equations are a popular example in the philosophy of science literature, it is rarely commented that Alfred Lotka developed his model in the context of his employment at the Metropolitan Life Insurance Company of New York. Lotka's equations were ported directly from his calculations of the depreciation of capital goods. Subsequently, the predator-prey elaboration of his work was adopted by mathematically-minded economists such as Paul Samuelson. This is a story of a model being taken from industry by ecology, and then economics.

Case 2: The model in Garrett Hardin's 1968 article The Tragedy of the Commons was a quite different type of model, being qualitative and computational instead of mathematical. Hardin developed it as an example of the dangers of population growth and the inadvisability of unrestricted access to common natural resources. Hardin's model was informed by his reading of classical political economy, including Ricardo and Malthus, and resituated into the context of the post-war systems sciences. The model in Tragedy was taken up by diverse specialties, from behavioral science to political theory, but it's most interesting new role was in the public choice school of political economy. There, Elinor Ostrom took it up but modified it to fit her own research program.

The comparison of these two cases will explore the following: the different transformations of models in the course of their resituation; and the importance of the alliances necessary for a local situation to bring in a new model. More generally, it is hoped that this will open up the topic of the relationship between lines of research in the life sciences and the human sciences to a broader audience in the philosophy of scientific practice.

Trading Zones in Peripheral Science: Scientific Practice from a Peripheral Point of View

Deepanwita Dasgupta
University of Texas at El Paso
United States

My presentation will examine the nature of scientific creativity when such creativity occurs under peripheral conditions in science. More specifically, I want to focus on the stages of a scientific practice when a new community (or a peripheral protagonist) is in the process of building a track record in science at a considerable distance away from the main community, and thus gain an entry into the research programs of science. Naturally, such communities operate under various types of constraints. Yet, constraints like this often give rise to the creation of novel concepts and ideas in science, thus serving as unusual conduits for scientific creativity. Engagements like this also often give rise to new scientific communities in such peripheral contexts. Thus, I seek to examine the nature of peripheral creativity in science, the relationship between such emerging scientific communities and their more resource-rich counterparts, and lastly, the cognitive contributions that such newcomers can bring to the practices of science. To ground this inquiry historically, I shall look briefly at the formulation of Bose-Einstein statistics by S. N. Bose in 1924. Working in the peripheral conditions of the early 20th-century colonial India, Bose produced the first non-classical proof for Planck's law which, in turn, brought in the concept of indistinguishable particles in quantum theory. By creating such a new statistics, Bose joined the practices of modern science from his peripheral location and also established a trade with the European scientific community. Yet, his track record in quantum theory lasted only for two brief years and ended with a controversy with Einstein. This complex track record of Bose, and other similar people like him, can give us an understanding of how scientific practice shapes itself in such peripheral contexts.

Transcending the Distinction Between Making and Using: The Phenomenology of Technical Artifact Functions

Mark Thomas Young
The University of Bergen
Norway

In the early years of the twentieth century, the Geiger counter was a device which, although widely used in European physics laboratories, had a reputation for unreliability . Many, believed the problem to be interference issuing from the internal components of the device itself, including Geiger himself, who sought to resolve the problem by redesigning the counter's walls and central wire. The problem would remain unresolved however, until 1928, when Walther Müller demonstrated that operating the device within an insulated container caused the interference to cease; an innovation which both drastically improved the reliability of the device but also confirmed the source of interference as external to the device itself—providing further evidence for the hypothesis of cosmic radiation.

This presentation begins with a question; should Müller's modification of the device be considered a feat of technology or technique? Or to put it differently, did Müller make a new device, or demonstrate a new way of using an already existing instrument? This distinction, between making and using, marks a central dichotomy in much thinking about the nature of technology in philosophy, and is understood to have crucial importance for a variety of issues in science and technology studies, including questions of ethical responsibility and the issue of ownership and priority for technical innovations. My goal in this paper is to illustrate how the seemingly innocuous ambiguity between making and using connects inevitably with larger questions and radical conclusions concerning the nature of human practices. I will argue that the distinction between making and using that has hitherto provided a framework for much philosophy and history of technology is untenable, both from an historical and philosophical perspective.

The first section of this paper seeks to problematize traditional philosophical conceptions of making in historical studies of technology, after which I will outline and explore an alternative conception of technological practice that draws on the phenomenological framework for material culture provided by Martin Heidegger and further elaborated by Timothy Ingold . The argument that emerges seeks to demonstrate that the distinction between making and using is itself derivative of a more fundamental mode of engagement with technology. In the final section, I will seek to apply this phenomenological strategy to the account of technical functions recently proposed by Wybo Houkes and Pieter E. Vermaas with the goal of demonstrating its capacity to resolve some the tensions inherent in this approach.

Transdisciplinary Understanding in the Courtroom: The Case of Legal Insanity

Henk de Regt
Department of Philosophy, Faculty of Humanities, VU Amsterdam
Netherlands

Gerben Meynen
2Department of Criminal Law, Tilburg Law School, Tilburg University
Netherlands

Transdisciplinary approaches to problem situations require communication and interaction between expert scientists, professionals and the general public, where each group brings its own understanding of the problem situation. An obstacle to achieving consensus is that the involved groups will typically adhere to different values and intelligibility criteria, which may hamper transdisciplinary understanding and communication. Philosophical analysis can clarify the way in which the groups' understandings of the problem situation differ, and indicate how conflicts may be resolved and consensus about appropriate problem solutions be achieved. An important question to be answered is whether expert scientific understanding, professional understanding, and public understanding differ in degree or in kind. More generally, what are the conditions for effective transdisciplinary communication? Our paper addresses these questions by examining the role of scientific expertise in the legal 'insanity defense', which is a concrete example of a transdisciplinary problem context. The case concerns problems regarding expert testimony on a defendant's responsibility for a criminal action. Psychiatrists, psychologists, or neuroscientists may testify about a defendant's responsibility for his or her actions. The insanity defense is used in many legal systems. If the defense is successful, a defendant who suffered from a mental disorder at the time of the crime is not held accountable for the criminal act s/he committed. An example would be a psychotic defendant who attacked his neighbor because of a paranoid delusion: because of the delusion, he may be excused for his actions and will not be sent to prison but admitted to a (forensic) mental hospital. Assessing the defendant's mental state or psychopathology is a medical/scientific matter, which will be assigned to expert scientists and professionals, such as psychiatrists, psychologists, and—in some cases—neuroscientists. As neuroscience progresses, it may provide us with increasingly reliable and detailed understanding of mental disorders or brain pathology, which may lead to conflicts with our everyday understanding of humans and our related moral judgments of their behavior. Resolving such conflicts requires that communication and interaction between scientists, professionals, and the public is not hampered by the differences in understanding. In our paper we will analyze problems for transdisciplinary understanding and communication in the courtroom. It turns out that expert neuroscientific understanding relevant to alleged cases of legal insanity crucially involves highly complicated technical and statistical details, for example, concerning the application of a neuroscientific technique such as fMRI. We submit that analogies can fulfill an important function in conveying a sufficient degree of understanding of such techniques to the lay audience in the courtroom (e.g. members of the jury). The use of analogies has proven to be highly effective in popular-scientific expositions of conceptually and mathematically intricate scientific fields such as quantum mechanics. We will present some examples of analogies that may be employed in the courtroom to render neuroscience intelligible. More generally, we will identify obstacles to, and specify conditions for, fruitful transdisciplinary communication and interaction, so that different groups with different understandings may reach consensus about appropriate problem solutions.

The Tree, the Labyrinth, and the Minefield

Nora Boyd
University of Pittsburgh
United States

It is perhaps tempting to think of scientific results in headline form, isolated from the manner in which they were produced: the Higgs mass is 125 GeV or the universe is 13.8 billion years old. Yet it would be a dangerous epistemic mistake to detach such polished results from awareness of the conditions that yielded them. Without their attending conditions, scientific results are hollow. Consider the well-known astrophysical results from the Hulse-Taylor binary pulsar interpreted as indirect evidence for the existence of gravitational radiation. If the pulsar can be well-modeled as an intrinsically accurate clock, if the two objects in the binary system can be well-modeled as point masses, if the interstellar plasma dispersion delay has been accurately calculated and correctly removed, if (assuming general relativity) the relativistic corrections to the pulsar times have been properly calculated and subtracted…then the orbital period derivative is consistent with the value expected according to general relativity. Often the pieces that need to be integrated together in order to arrive at a result are themselves results backed by prior investigation and analysis. For any scientific result, we can initiate the process of inquiring after the conditions associated with these prior results. One could become swamped in these nested conditions since not only does there appear to be a bottomless supply of them, but the more we probe, the more they branch horizontally. Instead, we usually truncate this incessant branching at some point or another for our various purposes, and accept some assumptions without asking further questions. Of course, the assumptions we do accept could always be wrong and we may presently come upon new reasons to think that they are. When results are reconsidered, the conditions that have been integrated together in the production of the result may have to be rolled back. This can only be accomplished if there is enough available documentation about how the original result was produced. When there is, investigators can revisit, reanalyze and reinterpret in order to generate a new result. It seems to me that this process of retracing steps through epistemic labyrinths is not only possible in many cases, but that the imperative to do so is at the heart of what it means to maintain empirical adequacy. Scientific results are like mines in a minefield. Those who care to strive after ever deeper empirically adequate science must chart a trajectory through them, either skillfully avoiding the live ones (would-be anomalies), taking pains to disarm them (by reprocessing the attending conditions), or finding them already harmless to the proposed path. In this talk, I invoke details of the Hulse-Taylor pulsar case to illustrate a three part narrative: the branching conditions that must be supposed in order to obtain the final result, the manner in which the integration of such conditions can be unraveled by retracing stages of analysis, and the sense in which the result nevertheless stands as a bona fide empirical constraint.

Trust, Objectivity, and the Democratisation of Science

Inkeri Koskinen
University of Helsinki
Finland

A process of ‘democratisation’ affects many disciplines today. Collaboration with diverse extra-academic agents—such as local communities, private enterprises, patient associations, or artists—has become common, and researchers in many fields are interested in promoting socially inclusive research practices. Activist research has gained importance in fields such as anthropology, and in some disciplines, like development, transdisciplinarity and extra-academic participation have become almost the norm. Researchers all over academia are coming up with ‘citizen science’ projects. Scientists and other academic researchers attempt to integrate tacit knowledge, the knowledge of ‘experts by experience’, indigenous knowledge, or artistic knowledge with scientific or academic knowledge.

This kind of democratic knowledge production is often supposed to create solutions to pressing social and practical problems; thus the ongoing development is prominent especially in fields that are expected to produce policy-relevant knowledge. So it is particularly important that the results are reliable. However, this is not necessarily the case. A recent editorial in Nature remarks on growing concerns related to the democratisation of scientific knowledge production: the people participating in citizen science projects often do so in order to advance their political goals, and this may lead to biases.

It is difficult to ensure value-freedom in the new, more democratic forms of research. However, this does not mean that these forms of research could not be objective. The value-free ideal has been questioned in recent philosophical discussions on objectivity (e.g. Douglas 2007). Also the move towards more democratic forms of scientific knowledge production has been noticed in these discussions (e.g. Wylie 2015). But thus far philosophers of science have mainly concentrated on the ways in which 'democratisation' can increase the objectivity of scientific knowledge production—not on the ways in which the new, democratic approaches may be less objective than could be hoped. I will concentrate on the discussion on trust, trustworthiness and objectivity, and show how 'democratisation' may lead not only to the increase of trustworthiness and objectivity, but to their decrease.

One of the objectives of participatory research, citizen science, and transdisciplinarity—and in another way also activist research—is to increase the public's trust in science. In feminist philosophy of science, trust and trustworthiness have been linked to objectivity. Naomi Scheman (2001) has argued that objectivity is connected to the idea of universal acceptability: when we call something objective, we make the claim that others too should accept it. Lay communities may, however, have rational reasons for distrusting scientists. Scheman views research communities as epistemically responsible for building rationally grounded trust not only within the research communities themselves, but also in lay communities. Heidi Grasswick (2010) suggests that participatory research is one of the possible ways of building such trust, and thus of increasing the objectivity of science.

The new, democratic forms of research may increase public trust in science. However, in other ways objectivity may be threatened when scientific knowledge production is democratised—making the results less trustworthy.

"Typically, But Not Exclusively": Interactional Expertise, Language Use and the Politics of Nanotechnology Terminologies Standardization

Sharon Ku
Drexel University
United States

Frederick Klaessig
Pennsylvania NanoSys. Ltd
United States

Nanotechnology, while being portrayed as the science grounded in precision control and manufacturing ability, is notoriously known for its ill defined scope in terms of research and regulation. The common "1 to 100 nm" definition promoted by the US National Nanotechnology Initiative has been challenged by the international society, causing constant debates among scientists, database constructors, regulators and industrial players over the proper usage of the term "nano" in labeling chemical substances. Problems of naming and classifying also induced a series of technical and philosophical debates regarding the intrinsic/extrinsic properties, along with the metadata selection for the appropriate depiction of nanomaterials. Although the chaotic language use has led to significant communication hurdles communication among science, policy and public actors regarding product safety and innovation governance, the linguistic dimension regarding the innovation and regulatory policymaking is often ignored in current nanotechnology studies. In this paper, we apply Collin's "interactional expertise" to investigate the relationship between language and practice in nanotechnology terminology standardization in ISO (International Organization of Standardization). We are particularly interested in the role language plays in shaping scientific practice and policy orientation: Which terms are considered as "core terms"? What are the socio-political concerns behind those choices and different language use? And how can international agreement be reached? Our data suggest that the battles in standardizing terminologies involve three types of language use:

* Commonly used concepts such as "size", "shape", "particle", "film".

* Strategic usage of adjectives such as "approximately", "typically".

* The selection and definition of "plain language" to communicate between technical experts and general societal actors.

Through analyzing these different linguistic choices, we reveal the ontological multiplicity of nanotechnology and the embedded economic, legal and national concerns. In addition, we find that "non-technical terms" play a key role in nanotechnology standardization. They serve as boundary drawing tools that tactically exclude potential disagreements, a vehicle to re-channel left-out opinions in technical terminologies, and a repository of the tacit knowledge, cultures and power that enables the English-speaking countries to exercise more control in the standardization process. The study of nanotechnology terminology standards demonstrates the penetrative influence of language use in shaping the regulatory rules, risk perception and science communication. We thus argue that sociology and philosophy of science can play a more radical role in science policy-making by revealing the power and constraints of language. We conclude this paper by reflecting on our hybrid role as standardization analyst and actor in the ISO TC229 Nanotechnology Committee, and on the potential to incorporate multiple ontological concerns into standardization.

Understanding Cancer Through the Practices of Microbial Experimental Evolution

Katherine Liu
University of Minnesota
United States

Our understanding of cancer continues to become much more sophisticated, in large part due to the advancement and availability of various molecular biology techniques, such as whole genome sequencing and fluorescence imaging. The translatability of this knowledge into clinical applications, though, has been underwhelming. The median gain in survival for the 71 drugs approved for metastatic cancers by the US FDA between 2002 and 2014 was just 2.1 months (Fojo et al 2014). Additionally, cancer is expected to soon pass heart disease as the leading cause of death in the United States (Siegel et al 2015). Here I argue that this situation—the advancement and accumulation of techniques and knowledge alongside the underwhelming development of clinical applications—emerges from a particular assumption about the relationship between theory and practice in cancer research. A molecular biology orientation assumes that the important causal factors in cancer can be found by identifying biomarkers that differentiate cancerous cells from non-cancerous cells. The biomarkers can be isolated from other factors in a complex organismal environment, which encourages researchers to think that translation of their gained knowledge into the clinic will be relatively straightforward. Different researchers challenge these assumptions, and argue for the application of evolutionary and ecological theories to cancer research, mostly responding to challenges caused by drug resistance from conventional treatment regimes. This involves a different relationship between theory and practice. Rather than a relationship based on individual changes in individual patients, the relationship here is about population-level predictions of cells or organisms. However, in clinical settings, these practices most effectively translate into applications of prevention rather than intervention, meaning the primary cause of death due to cancer—metastasis—remains unaddressed.

If the goal is to find a relationship between theory and practice in cancer research that fosters the development of effective clinical applications, the methods and techniques need to be scrutinized for whether they have the potential to translate into clinical applications. Since cancer is simultaneously a developmental, evolutionary, and ecological phenomenon, practices that capture this multidimensionality will be more promising candidates for effective translation. And yet, the most successful methods available to study the complex relationships between evolution, ecology, and development have not been applied to the complex causal etiology of cancer. Microbial experimental evolution methods comprise one set of successful practices that have promise for clinical application when directed at the processes underlying cancer progression, from carcinogenesis to metastasis. For example, the effects of spatial structure on the development and evolution of populations of cells, including their subsequent ability to disperse, can be studied at molecular, cellular, and phenotypic levels. Thus careful attention to the nature of practices used in investigating cancer and their relation to the theoretical perspective that encompasses the complex causal architecture (molecular, cellular, developmental, evolutionary, and ecological) shows how conceptual reflection on the scientific study of cancer can open pathways to novel treatment approaches.

Fojo, et al. 2014. JAMA Otolaryngol Head Neck Surg 140: 1-12. Siegel, et al. 2015. CA Cancer J Clin 65: 5-29.

Understanding the Explanatory Power of Dynamical Models in Systems Science

Karen Yan
Institute of Philosophy of Mind and Cognition, National Yang Ming University
Taiwan

Given the important role of the system dynamics of biological mechanisms and the power of dynamical models for modeling system dynamics, one can ask whether such dynamical models are explanatory on their own. A number of philosophers are currently attempting to answer this question. And since the mechanistic account of explanation is arguably the current best account of how explanation works in the biological sciences, these philosophers have focused on the issue of whether dynamical models can be explanatory even without relating to mechanistic models in some way.

One group of philosophers, advocating the mechanistic account of explanation, argues that dynamical models, if they are explanatory, must have some connection to mechanistic models (Bechtel, 2011; Bechtel et al., 2010 & 2013; Brigandt, 2013; Kaplan et al., 2011; Kaplan et al., 2011; Kaplan, 2015; Matthiessen, 2015; Zednik, 2011). For example, Kaplan and Craver (2011) hold that dynamical models cannot be explanatory merely in virtue of their descriptive or predictive power. They argue that the amount of explanatory power of a dynamical model matches the degree to which it provides a causal description of a mechanism. This idea is encapsulated in their model-to-mechanism mapping constraint (the 3M constraint).

The other group of philosophers argues that dynamical models can have non-mechanistic explanatory power (Chemero et al., 2008; Chirimuuta, 2013; Gervais, 2014; Issad et al., 2015; Gelder et al., 1995; Ross, 2015; Weiskopf, 2011). They differ from one another with regard to whether all, or just some, dynamical models can have such explanatory power, and also with regard to the ways in which they ground this explanatory power. For example, Ross (2015) argues that some dynamical models are explanatory in virtue of their power to abstract a certain feature of system dynamics from the more fine-grained details of a mechanism. On her view, dynamical models can therefore be explanatory without meeting Kaplan and Craver's 3M constraint.

In what follows, I will examine the above-mentioned debate by focusing on Kaplan and Craver's argument on the one hand, and Ross's argument on the other. Against Kaplan and Craver, I point out a deficiency in the 3M constraint, namely, that it neglects causal relations among the activities of a mechanism. And against Ross, I argue that the dynamical model in her case study is explanatory in a mechanistic sense. In order to do so, I propose a modified mapping constraint that accords an important role to the kinds of causal relations that Kaplan and Craver neglect, and I show that the dynamical model from Ross's case study satisfies this modified constraint. In that case, my revised constraint works within the general framework of mechanistic explanation, and provides a way to identify the unique and indispensable mechanistic-explanatory role that dynamical models can play in that framework.

The Use of Simulation Models in the ATLAS Experiment at CERN's Large Hadron Collider: Implications for Model-Based Experimentation

Koray Karaca
Department of Philosophy / University of Twente
Netherlands

According to the "hierarchy of models" (HoM) account of scientific experimentation, which was developed by Patrick Suppes (1962) and elaborated by Deborah Mayo (1996), the relationship between theory and experiment is not a direct one, but mediated by a linear hierarchical structure that consists of various types of models. To be more specific, the HoM accounts holds that the procedures at different stages of an experiment are carried out by using various types of models that relate to each other through a hierarchy ranging from "models of theory" lying at the top of the proposed hierarchy, via "models of experiment," to "models of data" lying at the bottom of the proposed hierarchy. The HoM account has been applied over the years to various experimental contexts, such as atomic physics (Harris 1999), astrophysics (Mayo 2000), and particle physics (Staley 2004).

While arguing for the HoM account, however, philosophers have not paid due attention to the use of simulation models in experimentation. In this talk, I shall confront the HoM account with the current practice in experimental high-energy physics (HEP), where simulation models are used for various purposes at different stages of experiment, including design of experimental set-up, data-acquisition, data-analysis, as well as interpretation of experimental results. Simulation models are also part of theoretical considerations involved in present day HEP experiments. In cases where analytical solutions are not available, simulation models are used to understand the final stable-states of high-energy particle collisions and thereby to extract testable predictions for HEP experiments.

As a case study, I will consider the ATLAS experiment that has been running at CERN's Large Hadron Collider, where the long-sought Higgs particle has been discovered in 2012. First, I will show what specific functions simulation models serve at different stages of the ATLAS experiment (ATLAS Collaboration 2010), as well as how they relate to non-simulation models involved in the same experiment. I will then argue that simulation and non-simulation models are used in complementary ways at different stages of the ATLAS experiment in order to carry out various experimental procedures. Based on this discussion, I will point out that the relationships that exist among simulation and non-simulation models involved in the ATLAS experiment are so intricate and multi-faceted that they cannot be accommodated within a linear hierarchical structure as suggested by the HoM account. Rather, I will argue that the various relationships existing among simulation and non-simulation models form a network-like structure, through which one can keep track of the interplay among experimentation, simulation and theorizing in the context of the ATLAS experiment.

References

ATLAS Collaboration (2010): The ATLAS Simulation Infrastructure. European Physical Journal C, 70:823-874.

Harris, Todd (1999): A Hierarchy of Models and Electron Microscopy. In Magnani, Nersessian and Thagard (eds.), Model-Based Reasoning in Scientific Discovery. New York: Kluwer Academic/Plenum Publishers.

Mayo, Deborah (1996): Error and Growth of Experimental Knowledge. Chicago: University of Chicago Press.

Mayo, Deborah (2000): Experimental Practice and an Error Statistical Account of Evidence. Philosophy of Science, 67:193-S207.

Staley, Kent (2004): The Evidence for the Top Quark Objectivity and Bias in Collaborative Experimentation: Cambridge: University of Cambridge Press.

Suppes, Patrick (1962): Models of Data. In Logic, Methodology, and Philosophy of Science, eds., E. Nagel, P. Suppes and A. Tarski, pp. 252-261. Stanford: Stanford University Press.

Using Philosophy of Statistics to Make Progress in the Replication Crisis in Psychology

Deborah Mayo
Virginia Tech, Department of Philosophy
United States

Caitlin Parker
Virginia Tech, Department of Philosophy
United States

The discussion surrounding the replication crisis in psychology has raised philosophical issues that remain to be seriously addressed. These touch on foundational questions in the philosophy of statistics about the role of probability in scientific inference and the proper interpretation of statistical tests. Such matters are key to understanding a paradox related to replicability criticisms in social science. This is that, although critics argue that it is too easy to obtain statistically significant results, the comparably low rate of positive results in replication studies shows that it is quite difficult to obtain low p-values. The resolution of the paradox is that small p-values aren't easy to come by when experimental protocols are preregistered and researcher flexibility is minimized. They are easy to generate thanks to biasing selection effects: cherry-picking, multiple testing, and the type of questionable research practices that are widely lampooned. The consequence of these influences is that the reported, 'nominal' p-value for the original study differs greatly from the 'actual' p-value. As Andrew Gelman has argued, the same problem occurs due to the flexibility of choices in the "forking paths" leading from data to inferences, even if the critique remains informal. It follows that to avoid problematic inferences, researchers need statistical tools with the capacity to pick up on the effects of biasing selections. Significance tests have a limited but important goal, especially in testing model assumptions. To trade them in for methods that do not pick up on alterations to error probabilities (Bayes ratios, posterior probabilities, likelihood ratios) is not progress, but would enable their effects to remain hidden . The sensitivity of P-values to selection effects is actually the key to understanding their relevance to appraising particular inferences, not just to long-run error control. The problems of hunting and cherry picking are not a matter of getting it wrong in the long run, but failing to provide good grounds for the intended inference the immediate inquiry. There's a second way in which reforms are in danger of enabling fallacies. It is fallacious to take the falsification of a null hypothesis as evidence for a substantive theory (confusing statistical and substantive hypotheses). Neither Fisherian nor NP tests permit moving directly from statistical significance to research hypotheses, let alone from a single, just significant result. Yet in order to block an inference to a research hypothesis, a popular reform is to assign a lump of prior probability on the "no effect" null hypothesis. But this countenances rather than prohibiting blurring statistical and substantive hypotheses! This is not only a statistical fallacy, it draws attention away from what is most needed in psychology experiments with poor replication: a scrutiny of the relevance of the measurements and experiments to the research hypotheses of interest.

We're Mismeasuring Diversity in STEM and Biomedicine: It's Time to Choose More Equitable Race and Ethnicity Variables

Sean M. Valles
Michigan State University
United States

This presentation will argue that the data collection and reporting practices in US higher education are inadequately designed for their primary tasks of monitoring and aiding in the promotion of diversity in higher education.

For example, National Science Foundation statistics lump together mixed-race people into a marginalized “other or unknown race/ethnicity” category. Meanwhile, Department of Education guidelines for higher education institutions’ diversity statistics require that all students fit into a single reporting category, such that, for example, someone reporting their ethnicity as "Hispanic” and their race as “Black” is listed exclusively as “Hispanic” in diversity reports. I have previously argued that such practices have pernicious effects on the availability of biomedical data on people with mixed racial or ethnic identities. I argue that the data management processes used in STEM diversity statistics similarly serve to distort our picture of STEM diversity, which undermines efforts to ameliorate the inequities of demographic underrepresentation patterns in STEM.

What After the Morning After Pill? Values in the Science and Regulation of Contraception

Christopher ChoGlueck
Indiana University Bloomington, History and Philosophy of Science
United States

Many regulatory bodies use scientific evidence and expert testimony for their decision making; however, the value-laden nature of both evidence and testimony creates difficulties, especially when societal values are at the root of conflicting recommendations. Consider the regulatory proceedings of the emergency contraceptive Plan B (the “morning after pill”) by the U.S. Food and Drug Administration (FDA). At a 2003 advisory meeting, 23 scientists agreed that Plan B should become available over the counter with 3 others dissenting. Much of the support for the drug came from Plan B’s potential to achieve the social goal of decreasing unplanned pregnancies as well as the political goal of increasing effective contraceptive options for women, especially young women and poor women. The FDA, however, retained Plan B’s limited prescription-only status, based on religious concerns about the possibility of post-fertilization effects, i.e., abortion for many Christians, and evidentiary concerns about the effectiveness of the drug for 15-year-olds and younger. Because proponents of the drug argued that the latter concern was debatable, the values of proponents and opponents appear to have influenced their assessment of the adequacy of evidence for making a recommendation. This paper examines these two concerns in the recent history of Plan B in order to analyze how various sorts of values (e.g., feminism, religious freedom, anti-abortion, social conservatism) have functioned in the science and regulation of Plan B, and it provides a novel normative analysis based on the institutional standards of the FDA.

The analysis follows two episodes: in 2006 the FDA re-labelled Plan B as potentially having a post-fertilization (abortifacient) effect, and in 2011 U.S. Department of Health and Human Services Secretary Kathleen Sebelius rejected the FDA’s decision to increase access to adolescents, citing no data on 11-year-olds. These episodes correspond with two debatable functions for societal values in science, namely describing phenomena (what constitutes an abortion?) and determining evidential sufficiency (how much evidence about adolescents in necessary for making a regulatory decision?). The paper then analyzes these episodes in terms of the evidential reasoning of the various parties involved, using a contextual analysis of operative background assumptions, societal values, scientific standards, and scientific evidence.

After accounting for the roles of these various factors in the reasoning of the actors in these episodes, the paper criticizes the reasoning with respect to three norms specific to the FDA: (a) based on evidentiary considerations; (b) constrained by democratic-liberal pluralism i.e., consideration & tolerance of different societal values; and (c) non-resistant to constitutive values of science. By providing a novel analysis of the how values have influenced reasoning in science policy and how they might function, this paper hopes to provide the philosophical groundwork for better regulatory decision-making.

What Is Wrong With Our View of Prediction?

Karim Bschir
ETH Zurich
Switzerland

It is commonly acknowledged that predicting is an important part of scientific practice.

In the philosophy of science, however, predictions are rarely treated as a topic in its own right (cf. Douglas 2009). If prediction happens to become the subject of philosophical analyses, it often pops up as a side problem in debates about explanation, confirmation, or realism. As a rough indicator for this, we might want to consider the fact that the number of publications dealing with predictions is negligible compared to the number of publications on explanation. (On philpapers.org, the category "Explanation in General Philosophy of Science" contains 1142 items, whereas "Predictions in General Philosophy of Science" brings up no more than 32 entries. The submission page for this conference does not contain "Prediction" as a keyword.)

In particular, the issue of temporal prediction has received less philosophical attention than it deserves. To be sure, philosophers do not simply ignore temporal prediction. They acknowledge that the prediction of future events is an important part of scientific practice. Yet the philosophically more intriguing case seems to be confirmational prediction, where the prediction follows as a consequence from a theory and stands in a confirmatory relationship to the latter. The reason for this, I assume, is that many philosophers still look at temporal prediction as a special case of conformational prediction. In this perspective, the only thing that is special about temporal predictions is that they are time indexed. What makes them epistemologically interesting is still the fact that they can serve as potential confirmations of the theory from which they were derived. Accordingly, predictive success is seen as a means of science and not an end in itself. The true aim of science is still supposed to be the explanation or truth-apt description of natural processes.

I want to challenge this view. To do so, I will proceed in three steps. First, I will reexamine (with the help of Douglas 2009 and Oreskes 2000) the contingent historical reasons for the negligent treatment of temporal prediction in current philosophy of science. Second, I will point out that the conditions for confirmational prediction are rather strict and that many successful predictions in current science are in fact not confirmational. That is to say that in many relevant cases, the aim of predicting is not so much epistemic, but rather practical and immediately related to the social value it generates. Successful predictions often do not allow us to infer the truth of the model that was used for their derivation. And scientists know that. They have long exploited the fact that having a bad model, or even no real model at all, does not prevent us from making good predictions. I will end, by way of example, by looking at some recent trends in the theory of dynamical systems, where the prediction of extreme events does not depend on the realistic representation of the causal structure of the underlying system (Sornette 2009, 2012).

What kind of crisis is psychology (supposedly) in?

Brian D. Earp
The Hastings Center Bioethics Research Institute
United States

In a much-discussed New York Times article, psychologist Lisa Feldman Barrett recently claimed, "Psychology is not in crisis." She was responding to the results of a large-scale initiative called the Reproducibility Project, published in Science, which appeared to show that results from over 60% of a sample of 100 psychology studies did not hold up when independent labs attempted to replicate them. In this paper, I address three major issues:

(1) What did the Reproducibility Project really show, and in what sense can the follow-up studies meaningfully be described as "failures to replicate" the original findings? I argue that, contrary to what many are suggesting, almost nothing can be learned about the validity of the original studies based upon a single (apparent) failure to replicate:

instead, multiple replications (of sufficient quality) of each contested experiment would be needed before any strong conclusions could be drawn about the appropriate degree of confidence to be placed in the original findings.

(2) Is psychology in crisis or not? And if so, what kind of crisis? I tease apart two senses of crisis. The first sense is crisis of confidence, which is a descriptive or sociological claim referring to the notion that many people, within the profession and without, are experiencing a profound and, in some ways, unprecedented lack of confidence in the validity of the published literature, considered on balance. Whether these people are justified in feeling this way is a separate but related question, and the answer depends on a number of factors. The second sense of "crisis" is scientific crisis—i.e., the notion that (due in large part to apparent failures to replicate a substantial portion of previously published findings), psychological science is "fundamentally broken," or perhaps even not a "true" science at all. This notion would be based on the assumption that most or even all of the findings in a professionally published literature should "hold up" when they are replicated, in order for a discipline to be a "true" science, or to not be in a state of "crisis" in this second sense. But this assumption, I will argue, is erroneous: failures of various sorts in science, including bona fide failures to replicate published results, are often the wellspring of important discoveries and other innovations. Therefore, (apparent) replication failure, even on a wide scale, is no evidence at all that science is broken, per se.

Nevertheless, (3) this does not mean that there is not substantial room for serious, even radical improvements to be made in the conduct of psychological science. These issues must not be brushed under the rug. Even holding the replication debate aside, that is, we have many at least partially independent reasons to push for deep changes in contemporary research and publication norms. Problems that need urgently to be addressed include: publication bias against "negative" results, the related "file drawer" problem, sloppy statistics and lack of adequate statistical training among many scientists, small sample sizes, inefficient and arbitrary peer review, and so on.

When Are A Priori Assumptions Warranted in the Metaphysics of Scientific Practice?

Thomas Reydon
Leibniz Universität Hannover
Germany

Philosophical work on kinds and classification, in particular in practice-oriented philosophy of science, has recently witnessed a turn toward the epistemology of kinds and classifications and away from metaphysical issues. However, abandoning the search for an account of the metaphysics of kinds and classifications would be too quick, as such an account is a crucial element of the explanation why some kinds and classifications are used in the sciences with more success than others, and why some ways of grouping things turn out not to be useful at all. After all, barring cases of epistemic luck the reason for the epistemic and practical success of kinds and classifications must be that they adequately connect to some or other feature(s) of the world.

While an account of kinds and classifications thus needs to encompass metaphysical elements, it is not clear whether practice-oriented philosophy of science is at all able to provide such elements. In this respect, practice-oriented philosophers of science face two problems. First, metaphysics cannot be read off from either epistemology or practice: simply examining scientific kinds and classifications and the ways in which investigators in the various areas of science employ them will not reveal their metaphysical underpinnings. Second, once the metaphysics is elucidated for individual cases, these different metaphysical pictures need to be unified into an overarching account of kinds and classifications—an issue which some authors hold to be insurmountable (cf., Dupré, 1993).

Thus it seems that at least some a priori considerations should be allowed to enter into the picture as guidelines for the metaphysical analyses of individual cases. But as a priori metaphysics is suspect from naturalistic and practice-oriented viewpoints, the challenge for a practice-oriented metaphysics of kinds and classification is to bring a priori considerations into play without removing the account unacceptably far removed from actual scientific practice. In this talk I addresses this challenge and question when a metaphysical account of a particular part of science would count as being unacceptably far removed from the actual practice it is applied to. My answer will hinge on the extent to which metaphysical claims can accommodate a variety of philosophical interpretations/reconstructions of a particular area of practice. The idea is that the less a metaphysical claim fits a wide variety of interpretations or reconstructions of a particular area of practice the more it should be considered unacceptably removed from that area of practice. I will use a case study on the classification of organismal morphological and genetic traits on the basis of homology to show how in this case the concept of sameness of kind can be interpreted in different ways and how, accordingly, a priori metaphysical assumptions about the relation of a classification or kind to the world can be ranked. The overarching aim is to explore what a thoroughly naturalistic and practice-oriented metaphysics of scientific kinds and classifications could look like.

Dupré, J. (1993): The Disorder of Things: Metaphysical Foundations of the Disunity of Science, Cambridge (MA): Harvard University Press.

When Students Go to the Lab: A Quantitative Study of the Scientific Conduct of Students' Relation to Experimental Data

Mikkel Willum Johansen
Department of Science Education, University of Copenhagen
Denmark

Frederik Voetmann Christiansen
Department of Pharmacy, University of Copenhagen
Denmark

Much is known about plagiarism (Schrimsher et.al. 2011) and test cheating (Hughes & McCabe 2006; Gallant et.al. 2011) among students, but very little is known about grey zone behavior in relation to experimental data. In order to fill this important gap in our knowledge we have conducted a quantitative study probing the scientific conduct of Danish chemistry students.

The question of research conduct and integrity is currently attracting a lot of attention. One of the interesting outcomes of the current research on research conduct is the relatively high frequency of questionable or 'grey-zone' behavior by scientists. Martinson et. al. (2005) report from a survey answered by 3247 scientists who had received funding from the US National Institutes of Health. In this survey 15,3% of the responding scientists admitted to have been "dropping observations or data points from analyses based on a gut feeling that they were inaccurate", 15,5 % admitted to have been "changing the design, methodology or results of a study in response to pressure from a funding source" and 6% admitted to have been "failing to present data that contradict one's own previous research" (Martinson et.al., p. 737). In a meta-study compiling data from 21 similar studies (including the one by Martinson et.al.) it was concluded that 33% of the scientists covered by the surveys admitted to have been engaged in "questionable research practices" other than falsification or fabrication of data, such as the behaviors mentioned above (Fanelli 2009; see also De Vries et. al. 2006). Understanding the causes of the high frequency of such dubious research behavior is important, and it is natural to focus on how science education may encourage or discourage such behavior. In our study we investigate the research conduct of science students and how research conduct is addressed in their training. We ask the students whether they engage in various grey zone behaviors in relation to experimental work and we ask them about their conception of and motivation for engaging in such practices. In this talk we will present and discuss our results and our preliminary analysis of them.

Bibliography

De Vries, R, Anderson M & Martinson, BC (2006): Normal misbehavior: scientists talk about the ethics of research. Journal of Empirical Research on Human Research Ethics, 1(1): 43-50.

Gallant, T.B., Anderson, M.G. & Killoran, C. (2011): Academic Integrity in a Mandatory Physics Lab: The Influence of Post-Graduate Aspirations and Grade Point Averages, Science & Engineering Ethics 19:219-235

Hughes, J.M.C. & McCabe, D.L. (2006): Academic Misconduct Within Higher Education in Canada, Canadian Journal of Higher Education 36(2): 1-21.

Fanelli, D (2009): How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data. PLoS ONE 4(5): e5738. doi:10.1371/journal.pone.0005738

Who Makes Scientific Graphics, and What Makes them “Good” Graphics?

Benjamin Sheredos
UC San Diego
United States

Scientists’ graphical practices have recently become a target of inquiry in philosophy of science, and in the cognitive sciences. However, our current methodologies leave much of graphical practice unexplored. In cognitive science, the majority of relevant research examines how non-expert subjects (undergraduates) consume finished graphics. Some work has also examined how non-expert subjects produce their own graphics, though this is rare. In general, this research treats graphical practice as an individualistic affair. And there is little examination of how expert scientists, themselves, produce and consume graphics. Nonetheless, cognitive scientists have recently begun leveraging their empirical results to formulate prescriptive “cognitive design principles” for how one ought to construct effective graphics. (For review of all these points, see Tversky 2011 and Hegarty 2011). To provide a simplistic summary of these design principles, one basic recommendation is to “keep graphics simple” by removing any information which is not task-relevant, making intended meaning readily accessible, and keeping a basic visual style constant between graphics. Another recommendation is to use to use space in ways which are “natural,” or which are held to reflect everyday embodied experience—e.g., use vertical axes for valuations, with up meaning more/better and down less/worse, and use horizontal axes for neutral dimensions. Meanwhile, philosophical investigations of scientific graphics typically examine experts’ finished products. (See, e.g., Nersessian 2008; Perini 2005, 2012; Griesemer 2012; Sheredos et al., 2012). The practices which led to graphics’ production are largely inferred and not observed. Moreover, there has been a tendency to focus on graphics produced by influential individuals (Maxwell, Darwin, etc.). As a result, we again have a set of methodologies which are ill-suited for uncovering the details of expert production, and we again have a tendency towards an individualistic conception of graphical practice. In this talk I supplement our understanding of scientists’ graphical practices via a novel case-study of how researchers crafted a peer-reviewed research publication in a top biology journal (what I shall call “the target article”). My data-set (provided by the authors of the target article) consists of 12 different drafts of the target article and its graphics, plus reviewer comments on the penultimate draft, as well as the authors’ own revisions and rejoinders in response to reviewers. The target article is a publication in the biology of circadian rhythms, and thus this case-study finds robust context in, and was facilitated by, recent work in collaboration with William Bechtel. I show that the development of the target article involved social and communalized strategies of graphical production which previously methodologies have left understudied—especially concerning the negotiation of publication. I also argue the case supports a challenge to the claim that “cognitive design principles” offer an apt understanding of the norms of success which govern scientists’ graphical practices: scientists’ own practices of revision and drafting suggest that they work under a different conception of what good graphical practice aims to accomplish. I offer some clarification of the norms of good graphical practice, and call for renewed attention to this issue.