Learning from Empirical Approaches to HPS (LEAHPS)
April 6th-7th, 2018, Center for Philosophy of Science

KEYNOTE ABSTRACTS

John Bickle (Mississippi State University, University of Mississippi Medical Center)
"Philosophers in Wet Labs, Doing Metascience"

Philosophy of science and actual practices of day-to-day science stand in a curious relationship. Philosophers seem more interested in the products of scientific practices, and mostly in the more theory-connected ones (explanations, models, simulations, and theories themselves). Judged by their scientific examples, few philosophers think that a focus on the processes generating these products, the scientific practices themselves, are central concerns. Philosophy of neuroscience is especially illustrative of this point. The mainstream of contemporary neuroscience, what we might call "Society for Neuroscience (SfN) neuroscience," is cellular and molecular. It is experiment-driven in the same way as are molecular biology and biochemistry. The day-to-day work of SfN-neuroscientists (aside from writing grants, for course!) is that of the lab bench. And yet to read the philosophy of neuroscience, one would hardly know that mainstream neurobiology exists! One would think that the discipline is all functional neuroimaging, modeling, computation, and computer simulation. In neuroscience reality, it is these latter fields that are a minority.

Suppose we adopt a metascientific perspective; suppose we seek to explicate science as scientists practice it, rather than as philosophers wish or recommend? How would the philosophy of science change? Speaking from a metascientific focus on current SfN-neuroscience, I will argue for numerous important ways. One, the still-pervasive theory-centrism would become of secondary importance to a philosophy of experimentation. Second, experimentation would be freed from the subsidiary role it has played in "hypothesis confirmation," and the nuances of experiment design and the role of multiple-experiment research programs would become the main epistemic focus. Third, the importance of tool development, along with its primarily engineering focus, would become prominent. Fourth, the influence of experiment-driven sciences in the early-21st century, along all reasonable measures (citation indices, number of practitioners, amount of research funding, etc.), would finally reveal theoretical physics to be the outlier empirical science, rather than the paradigm that all other sciences should mimic.

Nancy Nersessian (Harvard University, Georgia Institute of Technology)
"Modeling Dynamics: Cognitive Ethnography of Pioneering Research Labs"

Using empirical data per se (archival, published) has a long history in philosophy of science. Generating one's own data, by contrast, is a quite recent undertaking. Empirical methods available for investigating science have been developed for purposes other than philosophical inquiry and come with canons for proper use. They need to be adopted/adapted in light of an understanding of these canons and the affordances and limitations of the method. Choice of method is, of course, based on one's research questions, goals, and project framing.

The ethnographic research project I discuss is multidimensional, but my primary purpose for undertaking it was to address philosophical questions. The first set of questions concerns

determining how modeling methods accomplish their epistemic goals through examining their development and use in practice. Questions of how a model is built and manipulated or experimented with are important to traditional questions of what it represents and of the epistemic warrant of inferences made from it. The second set concerns the problem of integration: to develop an account in which cognitive, social, material, and cultural factors are mutually implicative in the problem-solving practices.

Pioneering interdisciplinary research labs in the bioengineering sciences offer a prime context for examining both the dynamics of methodological innovation in modeling and of building the complex cognitive-cultural investigative system of practices that is "the lab." The bioengineering sciences conduct basic biological research in the context of application. They self-consciously seek to merge concepts, methods, theories, and materials from engineering, biology, and medicine. The basic and applied research problems the four labs we have been investigating (two in biomedical engineering and two in integrative systems biology) require innovation in devising modeling methods suitable to investigating complex dynamical biological systems. In this talk I will concentrate on insights into innovative practices we uncovered, especially around a class of models largely unnoticed in the philosophical literature – hybrid in vitro physical/material simulation models (comprising biological and engineering materials) – widely used in biomedical engineering as surrogates for in vivo phenomena that cannot be investigated directly due to issues of ethics, control, or complexity. Throughout I illustrate our application and adaptation of qualitative methods of data collection and analysis appropriate to the project goals, questions, and framing, while also being mindful of their limitations and constraints.

Paul Thagard (University of Waterloo)
"Medical Inference: Using Computer Modeling to Explain Mental Health Assessment and Epidemiological Reasoning"

Empirical methods such as case studies and interviews make valuable contributions to historical philosophy of science; but evidence without theory is blind, even if theory without evidence is empty. Explanations of scientific developments are usually just narratives, but mechanistic explanations are increasingly available, in line with the view of theories as descriptions of mechanisms. Scientific practice and history can be explained by cognitive mechanisms for individual thinking, and by social mechanisms for the spread of ideas in communities. Computer modeling is a valuable tool for evaluating the explanatory power of mechanisms, if the description of a mechanism is precise enough to be translated into data structures (which describe the parts/entities), and algorithms (which describe the interactions/activities). Running a computer program connects mechanisms with the phenomena they explain, showing whether a proposed mechanism is capable of generating the targeted regular changes and resulting phenomena.

This methodology is illustrated by recent computer modeling of medical inference as resulting from mechanisms of explanatory coherence. The phenomena to be explained include medical diagnosis, mental health assessment (in which psychiatrists and psychotherapists explain and treat mental illnesses), and epidemiological reasoning concerning the environmental causes of diseases. Other aspects of medical inference susceptible to computer modeling include the social processes of debate and consensus concerning the causes of disease. Advances in

computational neuroscience enable modeling of embodied cognitive processes including sensorimotor and emotional aspects of medical thinking.

PRESENTATION ABSTRACTS

Hakob Barseghyan (University of Toronto) [w/Gregory Rupik (Free University of Berlin)]
"Empirical HPS: Scientonomy as a Missing Link"

Despite a growing number of attempts to re-integrate HPS, there is no agreement on how this integration should take place. This is partially due to the lack of consensus on what was wrong with the original Lakatosian-Laudanian notion of integrated HPS and what led to the exodus of historians. We argue that the key drawback of the original HPS was its conflation of two distinct projects: the search for a *normative* methodology of science and the search for a *descriptive* theory of scientific change. The conflation stemmed from the assumption that what makes a transition from one theory to the next *rational* is its obedience to the norms of a certain universal method. This assumption ignored the fact that the actual methods employed by scientists in theory evaluation can differ drastically between different epistemic communities, fields of inquiry, and time periods. Consequently, it produced several unfortunate misconstructions of historical episodes, which convinced mainstream historians that any philosophically-informed history is doomed.

To successfully re-integrate HPS, the *descriptive* task of extracting the general patterns of changes in theories and methods of their evaluation should be separated from the *normative* task of selecting the best methodology going forward. We argue that there is a missing link between the *descriptive* history of science and the *normative* philosophy of science – the *descriptive empirical general theory of scientific change*. As evidence for this proposal, we consider the work currently being done by a community of scholars that aim at establishing an empirical descriptive science of science, *scientonomy* (www.scientowiki.com). We discuss the theoretical foundations of scientonomy and demonstrate how it addresses the historians' concerns that led to the collapse of the original HPS.

Evelyn Brister (Rochester Institute of Technology) and **Phillip Honenberger** (University of Pittsburgh)
"Text Analysis of Contributor Sections and the Collaborative Dynamics of Research Teams"

Text analytics and bibliometrics are now established methods for investigating scientific knowledge production and its social dynamics. In this paper, we explore how text analysis of one unusual data source can inform theories about the collaborative dynamics of research teams.

Since roughly 2009, scientific journals such as *Nature* and *PNAS* have adopted a protocol according to which co-authored research papers must explicitly specify each author's contribution – for instance: "H.R. performed experiments, R.C. analyzed data, and C.H. wrote the paper." Though not unbiased, these "contributor sections" have the virtue of providing explicit representations of the division of credit, responsibility, and labor in research teams. Here we present methods for analyzing this data source and using the results to suggest and test theses about scientific collaborations.

Preliminary text analysis of 107 full-length articles published in four specialist *Nature* journals in March-June 2017 indicates distinct differences in the reporting of certain kinds of labor between fields (e.g. "theory" only in physics; "website and/or application design" only in

biotechnology) and a greater variety of types of labor reported in some fields as compared to others (e.g. greater variety of types of labor in genetics than in ecology and evolution). We connect patterns such as these to philosophical questions concerning collaborative knowledge production and differences in collaborative structure between fields. We also propose a method for using such data to test hypotheses about correlations between the division of cognitive labor, degree of interdisciplinary integration, and epistemic uptake.

The project illustrates both the promises and challenges of using empirical approaches in philosophical research. Such approaches are promising as tests of philosophical hypotheses, but limited by the type and quality of data that is available. The project further demonstrates how the availability of a promising dataset can provoke creativity in the treatment of philosophical questions.

Chad Gonnerman (University of Southern Indiana) [w/Itai Bavli (University of British Columbia), Aaron McCright (Michigan State University), and Daniel Steel (University of British Columbia)]
"Gender and Scientists' Views about the Value-Free Ideal"

Scientists' attitudes about the value-free ideal have been studied in some recent philosophically oriented survey work, as well as by a number of surveys of ecologists on advocacy. While gender is considered as a predictor variable in some of these studies, previous work has important limitations. Survey instruments in prior studies sometimes presume that objectivity and value-freedom are inherently linked, sometimes ask only whether value-free science is possible and not whether it is desirable, and none guard against ambiguity between epistemic and non-epistemic readings of the word "value." A second limitation is that previous empirical work has not articulated hypotheses about the relationship between gender and scientists' views regarding the value-free ideal.

In response, we developed a survey instrument designed to avoid the difficulties noted above, and we report results from an administration of this survey to science faculty at the University of British Columbia, in Vancouver, Canada. Given the results, we suggest two hypotheses about how gender might be related to views regarding the value-free ideal. The first hypothesis is that gender socialization tends to make women more inclined to prosocial behavior than men, which results in female scientists' being more likely to favor linking science to prosocial aims, such as promoting human welfare or social justice. According to the second hypothesis, female scientists are more likely than their male colleagues to view knowledge as socially situated and to think that those suffering discrimination may be epistemically advantaged in some respects.

Finally, we suggest reversing the gaze implied by questions about the implications of empirical work for questions traditionally posed by philosophers of science. To paraphrase John F. Kennedy, don't just ask what science can do for philosophy—also ask what philosophy can do for science. This about-face suggests further possibilities for how empirically oriented philosophy can be useful.

Nora Hangel (Indiana University, Bloomington)
"Analyzing Epistemic and Methodological Dimensions in Interviews with Scientists Talking about Challenges in their Day-to-day Research Practice"

Scientists find themselves increasingly challenged to articulate and defend their strategies concerning scientific inquiry. Especially in collaborative and interdisciplinary environments, scientists' abilities to give clear accounts of the methodological norms and standards are a crucial prerequisite for the functioning of scientific practice. How scientists themselves conceptualize situations of choice in non- algorithmic decisions and selection criteria in scientific inquiry has not been systematically analyzed. We (Hangel & Schickore) analyze aspects of how scientists conceive of methodologically relevant challenges using interviews with scientists. In delineation to the social sciences, for an empirically informed POS, the internal epistemic dimension stays central to the investigation but other contextual aspects are integrated in respect to their epistemic and methodological relevance. When we analyzed *narrative reconstructions* of scientists talking *about* their practice we contribute to a *conceptual framework* about how science is done. The data was collected in a previous project that following an ethnographic approach. The collected material exceeded the initial set of research questions greatly, which made it possible to revisit the data in a subsequent project to focus on methodologically and epistemically relevant aspects of scientific practice. On the one hand, we used qualitative data analysis to identify patterns emerging from comparing 'quotes' – or semantical units. On the other hand, we drew on philosophical debates about theory choice, exploratory research, epistemological strategies for experimentation, and values in science as guides for the exploration of scientists' reflections and ideas. We found that while scientists routinely express broader methodological commitments, they regularly express their doubts about how criteria such as reproducibility or fit with accepted theories should be put to work in concrete situations. We also found a conflation of error, fraud and technical or personal failure and a mismatch of applying norms and standards when promoting their own research and evaluating research of others.

Rebecca Hardesty (University of California, San Diego)
"Getting Serious About Model Description: Grounding Analytical Categories in Material Practice"

I will demonstrate how philosophers of science can employ traditionally social-scientific methods to study scientific practice while remaining disciplinarily distinct from those conducting social studies of science. In furtherance of this, I will engage with Michael Weisberg's practice-oriented framework for analyzing models and their descriptions, as presented in *Simulation and Similarity* (2013). I will primarily focus on his analysis of how graphical/textual descriptions (e.g. diagrams and captions) "specify" the representational aspects of models and how models "satisfy" these descriptions. I will complicate Weisberg's analysis by attempting to ground these analytical categories in everyday practice through presenting an ethnographic case. This case focuses on a neurobiology lab, which has been materially manipulating a mouse line to serve as behavioral, neural, and genetic models to understand cognition in Down syndrome.

I will show that to provide an adequate analysis of everyday modeling practice it becomes necessary to regard the category of "model description" as having at least two subcategories. One of these subcategories is Weisberg's graphical/textual descriptions. The other subcategory, which I will advance, is "material description," which accounts for how models can be *materially* specified through practices such as dissection. Thematizing this feature of modeling practice enables more fine-grained philosophical accounts of how practitioners use living experimental systems as representations. In particular, my case shows how the analytical categories of "model" and "model description" refer to aspects of the same continuous material process: in the neurobiologists' experimental practices (e.g. dissecting mice, culturing their neurons, etc.) their manipulations both a) transform a single living system into distinct representational models (e.g. behavioral, neural, and genetic), and b) serve as the means by which these models are specified, i.e. described. By studying how scientists materially work with models, it becomes possible to provide a more enlightening philosophical account of modeling practice.

Caleb Hazelwood (Georgia State University)
"Navigating a Plurality of Ontological Thresholds in Practice-based Theories of Natural Kinds"

Philosophers of science are abandoning a notion of natural kinds that requires the classifications to demonstrate their reality by offering causal explanations or granting inferential power. Such a move is motivated by an expectation that the kinds our philosophy recognizes ought to map onto the taxonomies our science produces. As such, we are presented with a spectrum of "ontological thresholds" that clash at some junctures. I use the familiar debate about species eliminativism to illustrate this point. Species eliminativists expect that the species category does not exist given the plurality and disunity of species concepts we legitimize in scientific practice. But here we identify a conceptual disagreement. For the eliminativist, a species concept is real insofar as it is legitimized by its epistemic pursuits and methodologies in scientific practice, but the species category must demonstrate a theoretical unity that sufficiently delineates it from other taxa. The difficulty for the species eliminativist, then, is that an account of natural kinds after the practice turn undermines the distinction between ontological thresholds. According to a practice-based theory of natural kinds, the species category is perfectly real by virtue of the same principles that allow us to recognize various species concepts: it is a category legitimized by the epistemic aims, methodologies, and classificatory practices of our best science. Thus, I suggest one possible solution: we may adopt a sort of pluralism about ontological thresholds. That is, I suggest any theory of kinds may be insufficient if it fails to recognize that different classifications will be decided based on different standards of what it is to be a "real" natural kind. The task for the philosopher of science, then, is to determine how we ought to discriminate between different notions, and when they are appropriately applied.

Catherine Herfeld (University of Zurich) [w/ Malte Doehne (University of Zurich)]
"What Empirical Network Analysis Could Offer to Research in Integrated HPS"

Empirical network analysis is increasingly appreciated as a methodology in the social sciences and in history. In recent years, it has also received attention among historians of science. These tendencies have methodological implications for integrated HPS. The aim of this paper is to stimulate a debate about the usefulness of quantitative-empirical methods in general and network

analysis in particular for integrated HPS. We defend the view that empirical network analysis is a particularly useful method because it helps to address a set of methodological issues that HPS researchers have been concerned with for some time now. For one, it allows for a systematic formulation and evaluation of philosophical theories more generally in light of detailed historical case studies. Moreover, it can contribute to addressing the empirical challenge of better connecting stylized philosophical models to reality. This empirical challenge originates in the recent turn towards using simulation techniques and formal models in philosophy of science and should be addressed by eventually testing such modelling results empirically. Network analysis can bring formal models closer to their target systems by drawing on empirical data. Finally, quantitative-empirical studies could allow for further supporting the generalizations that are drawn from historical case studies, and thereby complement them. While we see empirical network analysis as having more advantages for integrated HPS than *prima faci*e visible, we also acknowledge several limitations. Furthermore, we argue that empirical network analysis cannot replace more traditional methods but must rely on them to fully develop its potentials. To illustrate and develop our argument, we refer to a recently conducted network analysis of the diffusion of rational choice theories within and across the social and behavioral sciences.

Stefan Linquist (University of Guelph)
"Testing Rival Models of Cultural Evolution with large Sets of Ethnographic Data"

Memetics, evolutionary psychology, dual inheritance theory, and niche construction are four distinct research programs offering rival evolutionary explanations of culture. At least, these models appear to make different claims about the nature of culture and the best way to understand its evolution. Philosophers have contributed to the debate over which model provides the best explanation for human culture (writ large), often on grounds of intuitive plausibility, or by pointing to psychological experiments of "cultural transmission" that are somewhat artificial. In this paper I consider how this debate is transformed when we consider a more authentic body of evidence.

The Human Relations Area File (HRAF) is a digital archive containing thousands of ethnographic reports which are subject-coded by professional anthropologists. I recently employed the HRAF to test the culture of honour hypothesis –ie. the hypothesis that members of pastoral societies exhibit a heightened tendency for violent aggression compared to agriculturalists. My underlying question was whether one of the four rival models provides a superior explanation of this phenomenon.

It turned out that the alternative models made overlapping predictions as far as the ethnographic data were concerned. In effect, they were empirically indistinguishable. This was surprising, perhaps because I was accustomed to thinking about the four models exclusively in abstract (e.g. mathematical) terms. My strategy has been to redefine the core theoretical commitments of each model with an eye towards the ethnographic data.

A second realization was that these models are best understood as competing idealization strategies, as opposed to theoretical definitions of "culture." Memetics, for example, is not a theory about what culture essentially is. Rather, it is an explanatory strategy that foregrounds certain causal factors while backgrounding others.

Finally, as idealization strategies, the value of each model can only be assessed in relation to some actual pattern of cultural variation. It is doubtful that a single model is superior in all

cases. Although niche construction emerged as the best explanation for the cultural differences in my sample, memetics or even evolutionary psychology might be better suited to another trait or pattern of variation.

Miles MacLeod (University of Twente)
"Meeting in the Middle: Adapting Qualitative Methods to Philosophical Questions"

While there has been a recent uptake in interest in qualitative methods in the philosophy of science (see Wagenknecht el. 2015) many philosophers still remain to be convinced that these methods have an informative role to play. Finding a proper place for such methods requires critical reflection on what is considered relevant to philosophical explanation and prescriptions. At the same time it is also clear that such methods cannot simply be imported from other fields. Rather they need to be adapted to fit philosophical goals and interests. Qualitative methods and philosophy of science need to find a common ground. To explore these issues I rely on experiences gathered from an ethnographic study of model-building practices in systems biology. In general I hypothesize that qualitative methods are of a specific use when contextual factors or information outside of typical philosophical argumentation is relevant to a philosophical point of understanding, explanation or guidance. Such factors may be institutional, social, epistemological or cognitive. None of these factors are necessarily extractable from publications alone. At the same time however qualitative methods, like ethnographic methods in particular, tend to privilege the role of observable social and material factors over the role of the more technical cognitive or methodological factors which might be particularly relevant for understanding the methodological choices and epistemic structures philosophers are interested in. Any methodological approach needs to find a way to reformulate its aims in terms of narrower philosophical concerns by zooming on just those aspects it considers important. However such an approach also needs to preserve the benefits which flow from the objectivity and openness of a qualitative approach, for finding relevant factors that would otherwise be invisible.

Michael O'Rourke (Michigan State University)[w/ Stephen Crowley (Boise State University), Chad Gonnerman (University of Southern Indiana), Michael O'Rourke (Michigan State University), and Brian Robinson (Texas A&M University)]
"Micro-integration and Interdisciplinary Negotiation: Lessons from The Toolbox Dialogue Initiative"

Research conducted by interdisciplinary teams is an increasingly significant part of the landscape of science. Such research is often successful; it is also notoriously difficult and frustrating. What accounts for this difficulty and what can be done about it? Our view is that this work is hard because it requires coordinating different worldviews and that you make progress on this task by talking about it.

The Toolbox Dialogue Initiative (TDI) engages scientists and engineers in philosophical dialogue to facilitate their research. For TDI, philosophical dialogue is dialogue intended to support collective reflection on foundational commitments (e.g., values, attitudes, core beliefs) concerning the nature of research practice and the world under investigation. Philosophical dialogue of this sort can be generated in different ways—the TDI approach is *interventionist*, creating and facilitating occasions for philosophical dialogue among scientists themselves.

A key benefit of philosophical dialogue for interdisciplinary teams is that it allows for negotiation of the different disciplinary *worldviews* that team members bring to the project. Such negotiations involve critical moments where the philosophical framing of the dialogue enables collaborators to identify flaws or limits in existing thinking. It also contributes to constructive moments in which a research team's tentative *shared* mental model is enriched in some way. In this talk we analyze transcript data from TDI dialogues, which is rich with what we call *dialogical micro-integrations*, or combinations of ideas that emerge in a dialectical exchange of perspectives between two or more speakers.

Although it is not surprising that dialogue can function as an integrative mechanism, the idea that communication in general and philosophical dialogue in particular are integrative has not been developed in detail in the philosophy of science literature that addresses integration. We aim to contribute to its development here.

Vitaly Pronskikh (Fermi National Accelerator Laboratory)
"How Proto-megascience Evolved at NAL: An Empirical Study"

The chain of high-energy physics experiments that began with E-36—an experiment on small angle proton-proton scattering—started on February 12, 1972, and marked the beginning of NAL's experimental program [1]. This experiment drew collaborators from NAL, the Joint Institute for Nuclear Research (JINR at Dubna, USSR), the University of Rochester (Rochester, New York), and Rockefeller University (New York City). It was significant not only as a milestone in Fermilab's history, but it was also a model of cooperation between the East and West at a time when Cold War tensions still ran high. In this talk I will examine the cultural aspects of the US-USSR collaboration that were uncovered during a study of archival materials, regular and in-depth interviews, and participant observation methods. Among them were mixed language communication during experimental preparations; learning participants' native languages; assigning US companions to the Soviet physicists to create a friendly environment for the newcomers to help them adjust to unfamiliar surroundings; a social calendar; an entertainment program, as well as English language courses for the Soviet participants. I will also focus on political aspects of the collaboration, namely, the decision to host the Soviets as NAL guests; the roles of the scientists as informal ambassadors; a number of political steps undertaken by NAL Director Wilson to enable and facilitate the US-USSR collaboration. I will give examples of how the E-36 collaboration at NAL created many enduring relationships; contributed to rapprochement between otherwise hostile countries; and relaxed Cold War tensions. These and other traits of the first proto-megascience experiment at NAL, elucidated with the help of an empirical approach, will be discussed. I will also highlight some typical difficulties that can be encountered by empirical HPS researchers in a science lab.

Jessey Wright (Stanford University)
"Towards a Participatory Approach to Philosophy of Science"

Approaches to philosophy that incorporate interviews and ethnographic reports are valuable because they make visible the contributions of participants and practices that are difficult to notice from the outsider's perspective philosophers typically adopt. They also provide opportunities for philosophers to have a direct impact on the practices and individuals they are critical of. I propose that these methods can be viewed as part of a participatory approach to philosophy that aims to benefit philosophy and science. To develop this position, I first examine my graduate training and then discuss my postdoctoral appointment as the resident philosopher in a neuroscience lab.

During my graduate degree I was part of a neuroscience lab. My thesis project was born out of the stark contrast I noticed between philosophical analyses and the conversations occurring during the lab meetings I attended. Philosophical critiques of neuroimaging research often overlook small steps in the experimental process invisible from the viewpoint of research papers but plainly visible in day-to-day research practices. The products of this work included contributions to the philosophical literature, and more careful approaches to the interpretation of data within the lab. Now, I am a resident philosopher in a neuroscience lab that aims to improve reproducibility and the quality of evidence in neuroimaging research. My project aims to examine how the development of infrastructures for sharing and analyzing data are influencing the standards of evidence in neuroscience, and to positively influence the form, presentation, and promotion of those tools. Semi-structured interviews have been an important tool for working towards both goals. In particular, interviews have provided opportunities to identify and resolve barriers to communication, have helped me articulate philosophical projects with the potential to productively impact neuroscience, and have revealed opportunities for collaboration. When philosophers engage in constructive conversations with scientists, philosophy and science benefit.

POSTER ABSTRACTS

Marion Boulicault (Massachusetts Institute of Technology)
"The Role of Values in Measurement: Brain-Computer Interfaces and the Illiteracy Metric"

Brain-computer interfaces (BCIs) are implantable devices that allow for computer-mediated interaction between a person's brain activity and their environment. Examples of devices include those aimed at controlling prosthetic limbs and epilepsy. They work by analyzing brain activity (e.g. to determine an intention to move, or the beginnings of a seizure), and then translate that activity into action (e.g. the movement of a prosthetic arm, or neurostimulation to prevent a seizure).

Given cultural connections between the brain and identity, as well as worries about privacy and 'neurohacking' (to name a few examples), significant attention has been rightly paid to the ethics of BCIs. However, in this paper, I want to raise a question that I contend has yet to receive sufficient attention: what are the ethical and political implications of the way we *measure* BCIs?

There exists a subset of the population who, despite training, are unable to use BCIs. This failure is usually due to problematic translation between brain activity and action, e.g. the BCI cannot 'read' the brain signals produced by the individual, usually for unknown reasons. BCI researchers call this phenomenon 'BCI illiteracy' and report that it affects 15 – 30% of BCI users (Thompson, draft). I argue that the use of 'BCI illiteracy' as a metric for success encodes a problematic model of human-technology interaction. In particular, it places responsibility for the 'failure' on the individual BCI user, as opposed to the technological system. This has implications for how the BCI user perceives herself in relation to the technology, and on how neuroscientists and engineers understand and engage in their work, and thus on how the technology develops. As such, the case of the BCI illiteracy metric illustrates how instruments and practices of measurement serve as sites for the interaction of science, technology and values.

Simon DeDeo (Carnegie Mellon University)
"The Microstructure of Scientific Revolutions"

Large-scale shifts in the nature or subject of scientific inquiry are topics of perennial interest and historical analysis tells us a great deal about the sources of change in intellectual life. Much less is known about the smaller revolutions that might occur during what appears in between, leading us to undervalue the creativity and dynamism hidden in "normal science." Drawing on recent tools used for the analysis of political debate, I present a large scale analysis of the text of papers on the arXiv preprint server. This information theoretic study enables us to compare and contrast the development of empirical sciences, such as observational astrophysics, with largely theoretical endeavors such as string theory. And it reveals a continuum of microrevolutions in a range of subjects that can also form the basis for more qualitative and conceptual analysis.

Jaana Eigi, (University of Tartu) [w/ Katrin Velbaum, Endla Lõhkivi (University of Tartu)]
"Using interviews when studying epistemic impacts of the work conditions in academia"

In the presentation, we show how analysing interviews may contribute to philosophy of science. Using interviews with humanities researchers to explore research evaluation (Lõhkivi et al 2012), we argue that it does not match the output valued in humanities and thus diminishes their credibility. Analysing interviews with early career researchers in humanities and social sciences (Eigi et al 2014; Eigi et al 2018), we show how extremely project-based science policy poses challenges both for them and their supervisors.

Our analyses contribute to the understanding of the epistemic impact of social organisation of research (Biddle 2014), such as evaluation and funding. They also contribute to taking science practitioners' concerns seriously (Douglas 2010) – in our case, concerns about work conditions in academia. Thus, they help to advance several projects in contemporary philosophy of science.

Brady Fullerton (University of Guelph)
"Computational Approaches to the Multivalence of 'Junk DNA'"

Within the discipline of genomics there is currently a debate over how to define "Junk DNA." Some researchers even argue that this term serves primarily as a rhetorical device and should therefore be avoided altogether (Graur et al., 2012). Sparking this debate was the highly publicized finding by ENCODE (2012) which claimed to have identified a "biochemical function" for as much as 80% of the human genome (Eddy, 2012). The media surrounding this project interpreted this result as "the eulogy for junk DNA" (Pennisi, 2012). Critics, however, pointed out that ENCODE was using "junk" in a rather loose sense to describe any genomic region that lacks some minimal biochemical effect. This debate raises questions about how many different conceptions of "junk DNA" are currently floating around within the various sub-disciplines of genomics, and what exactly are the rhetorical or epistemic roles that each conception plays.

In this poster I present the methods and results of an approach that relies on the computational procedure of topic modeling to identify the most common "topics" or themes that emerge out of some large corpus. Working with a corpus supplied by JSTOR I make use of Latent Semantic Analysis to better understand terms that group with "Junk DNA" in both synonymous and alternative usage. Furthermore, I have trained a Latent Dirichlet Allocation (Blei et al., 2003) on this same corpus in order to better understand the topics surrounding "Junk DNA." The most basic finding of testing this trained model is that there are multiple conceptions of "Junk DNA" within this corpus, many of which appear to be associated with a particular time period. Hence, the term appears to be undergoing a rapid evolution in its meaning, raising questions about the kinds of "selective pressures" that might be driving these changes.

Nicolas Kleinschmidt (University of Münster)
"When Should Philosophers of Science Conduct Empirical Research? The Explication of 'Empirical Aesthetics' as a Paradigmatic Case of Necessary-Empirical Philosophy of Science (NEPoS)"

Over the last 50 years, academic activities have paved the way for the (re-)establishment of what today is called "empirical aesthetics". However, empirical aesthetics is a topic of controversy within the scientific community – not only between empirical aestheticians and other scientists, but also between empirical aestheticians themselves. It has developed within a wide range of different humanities and sciences. And although it seems to foster a great deal of innovative research in all of those different disciplines, the concept of empirical aesthetics is far from clear. Even academics who think of themselves as representing this way of approaching aesthetical questions do not offer a satisfying definition of their own discipline.

Up to the present day, only a few historical and no sociological research on empirical aesthetics has been conducted. Following Rudolf Carnap's philosophical method of explication, philosophers are, however, in need of at least some empirical data to answer their questions. In order to explicate the concept 'empirical aesthetics', a philosopher of science therefore seems to be challenged to conduct her own empirical study on empirical aesthetics.

In my poster presentation, I display my explication of the concept 'empirical aesthetics' as a case study for, at least partially, empirical research in the philosophy of science and as a paradigmatic case where philosophers of science need data which they have to gather through empirical research themselves, which I call "NEPoS" (i. e. "Necessary-Empirical Philosophy of Science"). Second, from that paradigmatic case I abstract the underlying "NEPoS"-type of cases in philosophy of science. Third, I argue that in "NEPoS"-type cases, philosophers of science should conduct empirical research for not only rational reasons, but also reasons of research ethics.

Kate MacCord (Marine Biological Laboratory)
"The McDonnell Initiative at the Marine Biological Laboratory"

In order to highlight the impact that empirical approaches to HPS has on both HPS practitioners and scientists, I layout an initiative underway at the Marine Biological Laboratory (MBL) in Woods Hole, MA, called the McDonnell Initiative. Funded by the James S. McDonnell Foundation, this initiative facilitates collaborations between historians and philosophers of science and life scientists with the aim of transforming the research of both fields through interactions and the co-production of knowledge. In order to accomplish this goal, the initiative funds workshops, working groups, and laboratory embedding experiences, all held at the MBL. I discuss the different activities that the initiative has employed to seed these collaborations, the feedback from both disciplines about the impact of these activities on the ways in which participants think about their work, and conclusions about the nature of future activities that can further enhance working relationships between scientists and historians and philosophers of science.

Jaimie Murdock (Indiana University, Bloomington) [w/ Colin Allen (University of Pittsburgh), Simon DeDeo (Carnegie Mellon University)]
"The Development of Darwin's Origin of Species"

From 1837, when he returned to England aboard the HMS Beagle, to 1860, just after publication of The Origin of Species, Charles Darwin kept detailed notes of each book he read or wanted to read. His notes and manuscripts provide information about decades of individual scientific practice. Previously, we trained topic models on the full texts of each reading, and applied information-theoretic measures to detect that changes in his reading patterns coincided with the boundaries of his three major intellectual projects in the period 1837-1860. In this new work we apply the reading model to five additional documents, four of them by Darwin: the first edition of The Origin of Species, two private essays stating intermediate forms of his theory in 1842 and 1844, a third essay of disputed dating, and Alfred Russel Wallace's essay, which Darwin received in 1858. We address three historical inquiries, previously treated qualitatively: 1) the mythology of "Darwin's Delay," that despite completing an extensive draft in 1844, Darwin waited until 1859 to publish The Origin of Species due to external pressures; 2) the relationship between Darwin and Wallace's contemporaneous theories, especially in light of their joint presentation; and 3) dating of the "Outline and Draft" which was rediscovered in 1975 and postulated first as an 1839 draft preceding the Sketch of 1842, then as an interstitial draft between the 1842 and 1844 essays.

Vincenzo Politi (UNAM)
"The Role of External Analogies in Scientific Change"

In a number of ethnographic studies of science, Kevin Dunbar has investigated scientific reasoning by looking at actual scientists at work. Dunbar found that *analogical reasoning* is a common features of the actual practice of science

Analogical reasoning, however, poses a problem. If scientists solve problems by trying to make them similar to a set of established exemplars, how is scientific change possible? Where do new ideas and problem-solving techniques come from?

The aim of my talk is to solve the problems with analogical reasoning by expanding on some observations contained in Dunbar's own work. The work of Dunbar is considered to be influential in the 'cognitive science of science', but is yet to find a proper application in the history and philosophy of science. My talk will provide an opportunity to show how some of Dunbar's ideas can illuminate some crucial episodes in the history of science.

Dunbar explains that scientists make 'local analogies' when trying to solve experimental problems. These 'local analogies' work in a way which is similar to models and exemplars. At the same time, he also speaks of 'external analogies', through which particular problems are solved by drawing analogies with problem-solutions coming from other fields and disciplines. External analogie: 1. are not as frequent as the local analogies; 2. are used especially when facing more theoretical or conceptual problems (Dunbar, 2001, p. 316).

On the basis of Dunbar's 'local'/'external analogies' distinction, and of the characteristics of the latter, I make the hypothesis *H*:

(H) 'External analogies' may play a crucial role during episodes of radical scientific change.

To test my hypothesis, I consider a well-known *case study*: the so-called Black-Body Problem.

Raphael Scholl (University of Cambridge)
"Scenes from a Marriage: On the confrontation model of history and philosophy of science"

According to the "confrontation model," integrated history and philosophy of science operates like an empirical science. It tests philosophical accounts of science against historical case studies much like other sciences test theory against data. However, the confrontation model's critics object that historical facts can neither support generalizations nor genuinely test philosophical theories. Here I argue that most of the model's defects trace to its usual framing in terms of two problematic accounts of empirical inference: the hypothetico-deductive method and enumerative induction. This framing can be taken to suggest an unprofitable one-off confrontation between particular historical facts and general philosophical theories. I outline more recent accounts of empirical inquiry, which describe an iterative back-and-forth movement between concrete (rather than particular) empirical exemplars to their abstract (rather than general) descriptions. Reframed along similar lines, the confrontation model continues to offer both conceptual insight and practical guidance for a naturalized philosophy of science.