Canadian Society for History and Philosophy of Science 2011
University of New Brunswick, Fredericton
May 29-31, 2011

Abstracts

Ben Almassi, College of Lake County
Peer Review in an arXiv Age
Session II.3A

The recent growth of the arXiv in physics and online "preprint" archives throughout the sciences presents historians and philosophers of science a good opportunity to critically examine the evidential significance of peer review. Traditionally peer review has been framed as a cornerstone of scientific objectivity, and the peer reviewed publication of results understood as crucial evidence of their rigor and reliability. Yet in physics and other fields the arXiv (formerly the Los Alamos National Laboratory, or LANL, Archive) lets users upload material without peer review filter; this unrestricted construction enables scientists to transit information and engage each other's work at a rate that makes waiting on peer reviewed publication quite unfeasible. But do the benefits of arXiv come at the expense of scientific objectivity? In answering this question, I drawn upon the philosophical literature on epistemology of peer review and to the history of the development and debates over arXiv, specifically in modern physics. I critically engage arXiv founder Paul Ginsparg's critique of traditional peer review, and I look to identify other indicators to which arXiv users turn, absent peer review, to gauge the rigor and reliability of posted work. I propose we distinguish between the corroborating and anti-biasing aspects of peer review's contribution to scientific objectivity; I argue that the development of the arXiv has been responsive on corroboration, but the anti-bias problem remains neglected. To this end, I advocate for arXival double-blindness.

Micah Anshan
Lessons from Insite: Disagreement between government, neuroscience and social science over Vancouver's Safe Injection Facility and effective drug addiction policy
Session I.3C

The Vancouver Safe Injection Facility (SIF) has been subject to rigorous scientific study which has shown it to be economically and medically effective. However, the Canadian federal government is still opposed to such harm-reduction measures regardless of their objective validity. Further, Robert Blank's characterization of neuroscience as opposing social science is unhelpful considering that he does not address harm-reduction in an article about public policy and addiction. Thus it seems that there is an inherent antagonism to harm-reduction in both governmental and neuroscience policy recommendations. This paper holds that this antagonism is not based on any legitimate study and should be abandoned in favour of a policy which has been shown to work, even if it seemingly contradicts widely-held societal beliefs about drug addiction and drug addicts. By putting our prejudices against drug addicts aside, it becomes clear that harm-reduction policy is best suited for the problem of drug addiction.

John Beatty, Philosophy, U. of British Columbia
Alfred Moore, Political Science, U. of British Columbia
Pluralism and Legitimacy in Scientific and Political Authority
Joint Session CPA-CSHPS

Riiko Bedford, IHPST, U. of Toronto
ADHD: Natural Kind or "Normalizing Judgment"?
Session III.1B

Philosophical accounts conflict about the relative importance of biological nature and constructed social meaning to the character of disease as it is assumed and applied by medicine. More specifically, these accounts disagree about the ontological status of diseases as "real" vs "constructed." Medicine currently proceeds as if diseases represent natural kinds; however, this approach raises issues in the realm of mental illnesses, as many feel these classifications to be strongly dependent on social convention. Rather than uncritically accepting diseases as natural kinds, it is fruitful to examine their essential qualities and ontological status, for implicit in conceptions of disease are assumptions about the character of the self which is afflicted. This paper examines allopathic medicine's unquestioning treatment of the commonly diagnosed mental illness attention deficit-hyperactivity disorder (ADHD) as a natural kind, and the social and existential consequences of this assumption. Further, it offers a suggestion for a conception of disease rooted in an understanding of the afflicted self as a dialectical subject that emerges through indissoluble interactions between biology and an array of constructed social meanings. Such a conception of disease resolves many of the common philosophical disagreements around the status of disease (as predominantly biological or socially constructed), and allows for a broader scope of the delivery of medical care; indeed, the expansion of the afflicted self to a dialectical entity calls for an analogous extension of the boundaries of psychiatry, and even medicine, to incorporate social, political, economic, and cultural factors into its research and practice.

Adriana Benzaquen, History, Mount Saint Vincent U.
Locke's Science of Childhood
Session III.1C

Although histories of developmental and child psychology typically highlight the significance and lasting influence of John Locke's ideas about childhood and children, not much has been written about the practical origin of those ideas. To what extend did Locke follow his own injunction, in the Essay concerning Human Understanding, to "Follow a Child from its Birth, and observe the alterations that time makes…"? This paper reconstructs Locke's "science of childhood" in action by examining his experiences with and observations of children. In particular, I will discuss Locke's relationship with the children of his friends Edward and Mary Clarke, for whom he wrote the letters that were later published as Some Thoughts concerning Education. I will argue that, even though Locke's science of childhood was very different, in its goals and methods, from the specialized child sciences—the Child Study movement, developmental psychology, paediatrics and psychoanalysis—that would emerge in Europe and North America in the late nineteenth century, it already makes visible some of the concerns and anxieties that would accompany the rise of these sciences. Drawing on Locke's published correspondence and the unpublished Clarke family letters and papers, I will show that Locke's science of childhood was, like the modern child sciences, relational (its data and generalizations were produced in the context of concrete relations between specific adults and specific

children), transformative (it created new roles and identities for adults in their relationships with children) and normalizing (observations of children were inextricable from educational and therapeutic interventions).

Joseph Berkovitz, IHPST, U. of Toronto
The Propensity Interpretation of Probability: A Re-evaluation
Session I.2C

The notion of probability is ubiquitous in science. In his book *Science and Hypothesis*, Henri Poincaré, the famous French mathematician, physicist and philosopher said of the probability calculus: "if [it] be condemned, then the whole of the sciences must also be condemned." While there is a broad consensus concerning the calculus of probability, its interpretation continues to be a controversial matter. Interpretations of probability can be divided into two categories. Subjective or epistemic interpretations, where probability reflects knowledge, ignorance or uncertainty; and objective interpretations, where probability reflects objective properties of the world. The propensity interpretations are among the main objective interpretations of probability. In the literature, there have been various objections to propensity interpretations – e.g. that they are at odd with the probability calculus, that propensities are metaphysical and unobservable, and that the relationship between propensities and long-run frequencies is ill defined. In this paper, I consider objections and argue that they are largely unjustified.

Agnes Bolinska, IHPST, U. of Toronto
The Epistemic Function of Misrepresentational Scientific Models
Session II.1C

In this paper, I argue that our understanding of the notion of scientific representation can be aided by examining the various ways in which scientific models misrepresent their target systems. I consider scientific models in terms of their role as tools for gaining knowledge about physical systems, which I take to be their primary role. I show that even on a conception of models as tools for gaining knowledge, fairly severe misrepresentations, including those that turn out not to have targets, like Maxwell's model of the electromagnetic ether, count as scientific representations nonetheless. I discuss the difference between unintentional and intentional misrepresentation: unintentional misrepresentation arises when scientists are fundamentally mistaken about the structure or function of a target system, while intentional misrepresentation usually involves some form of idealization. I argue that the epistemic role of both kinds of misrepresentation ought not be underestimated. Unintentional misrepresentations are useful for the investigation of a hypothetical system, which might in turn have consequences for actual systems, while intentional misrepresentations facilitate knowledge gain about aspects of a system that could not be understood otherwise.

Kristin Borgerson, Philosophy, Dalhousie U.
Useless, Repetitive, and Secretive? Assessing the Scientific Validity of Clinical Trials
Session II.3B

Clinical research ought to be scientifically valid. This ethical requirement is widely accepted and works its way into most contemporary ethical guidelines. It would seem, then, that it would be a good idea to have some account of what we mean by scientific validity (at least in the particular context of clinical research, if not more generally) as well as some way of distinguishing better from worse

standards of scientific validity. I do not believe that philosophers of science or bioethicists have adequate answers to these questions. As a result, clinical researchers and members of research ethics committees (RECs) alike share in a general confusion about the particular demands of scientific validity and the strength of those demands relative to the other ethical requirements of clinical research. In this paper I draw on recent research by clinical epidemiologists in order to identify and critique two particular assumptions underlying current conceptions of scientific validity: first, that the appropriate level of analysis when assessing scientific validity is the isolated individual clinical trial, and second that scientific validity should be assessed independently of the other ethical requirements of research. Each of these problematic assumptions stems from a general failure to appreciate the ways in which science is a social practice. A robust understanding of scientific validity requires that we attend to the ways in which research trials, like researchers, are not isolated and independent.

Frédéric Bouchard, Philosophy, Université de Montréal
There are no organisms, just complex multi-species individuals. So what are the bearers of adaptation?
Session I.3A

Organisms have always played a privileged role in our understanding of biological phenomena. They are assumed to be the bearers of the adaptations explained by the theory of evolution by natural selection, the focus of development and the causal nexi of ecological interactions. Explicitly or not, biologists and philosophers have assumed that what made organisms special contra other types of organization (e.g. genes, genomes, groups, species, etc) was their relatively high structural/material homogeneity (they are made of the same type of material and have a shared origin) and functional integrity (the organism is a functional whole acting as one system). Developments in microbiology and in symbiosis research weaken the appeal of these intuitions. Most organisms are in fact composites of multiple species (e.g. the gut microbiome allowing us to digest most of our food is part of us) and the apparent superiority of organisms over other types of organisation needs re-appraisal. In this presentation, I will examine how various philosophy of biology accounts of biological individuality (J. Wilson, R. Wilson, D.S. Wilson and Sober, J. Dupré) as they pertain to organisms fare against these developments. I will argue that the primacy of organisms needs to be replaced by the primacy of complex multi-species individuals and analyse the consequence of this for how we understand biological adaptations.

Ingo Brigandt, Philosophy, U. of Alberta
'Developmental Constraint' in the 1980s: Positive Explanatory Agenda or Mere Tool of Criticism?
Session I.3A

The concept of developmental constraint was at the heart of developmental approaches to evolution of the 1980s. It is well-known that this idea was used to criticize neo-Darwinian evolutionary theory, in particular adaptationism and the centrality of natural selection. Yet merely criticizing neo-Darwinism does not yield an alternative framework that would actually offer evolutionary explanations. In current evolutionary developmental biology ('evo-devo'), the concept of developmental constraint is of minor importance, with notions such as evolvability (and novelty) being at the center of attention. The concept of evolvability clearly defines an explanatory agenda in evolution, so that one could view the historical shift from 'developmental constraint' towards 'evolvability' as the move from a concept that is a mere tool of criticism to a concept that founds a positive explanatory project. However, by taking a look at how the concept of developmental constraint was employed in the 1980s by those who

introduced this idea, I argue that developmental constraint was not just seen as constraining, but also as permitting and facilitating morphological transformation in several ways. Accounting for the possibility of macroevolutionary transformation and the evolutionary origin of novel form was clearly part of the agenda of these developmental approaches to evolution. Therefore, the concept of developmental constraint was part of a positive explanatory agenda long before the advent of evolutionary developmental biology as a genuine scientific discipline. In the 1980s, despite the lack of a clear disciplinary identity this concept coordinated research among paleontologists, morphologists, and evolutionary developmental biologists.

Cameron Brown, Philosophy, Concordia U.
Aristotle and Evolution
Session I.2B

Was Aristotle an evolutionist? Some (e.g. Greene 1973, Ruse 1979) see Aristotle's biology as fundamentally at odds with evolution, for example because of his commitment to the fixity of species, or his use of teleological explanation. Others think that though he was not an evolutionist, this was merely contingent: Aristotle could have and, indeed, would have been an evolutionist had he not held, for instance, that the fixity of species was parasitic on the fixity of the celestial spheres (e.g. Henry 2006). And yet others have thought that Aristotle was in fact the "first evolutionist" (e.g. Nordenskiold 1928, and in a circuitous way, Gotthelf 1999). I suggest that this interpretive discord results not from confusion on Aristotle's part, but from the absence on the part of his interpreters of a univocal conception of evolutionary theory. Beginning with Darwin's partial reading of Ogle's translation of *Parts of Animals*, I show how changing historical judgments about the compatibility of Aristotle's biology with evolution are themselves demonstrative of the morphogenesis of evolutionary theorizing. Ultimately, by comparing Aristotle's embryological account of formal reproduction with Pere Alberch's "rules of development", I argue that evo devo offers a rich new perspective from which to assess Aristotle's biology.

James Robert Brown, Philosophy, U. of Toronto
What Do We See In a Thought Experiment?
Session I.1A

Einstein's special relativity involves a number of thought experiments. Most of these follow the usual pattern. Like other thought experiments they begin with a setup that we can visualize; we then observe what happens; finally, we draw appropriate morals. The observation, of course, takes place in our imagination, the mind's own laboratory, but the process is otherwise similar to a real experiment. Observers on the track and on the train in Einstein's thought experiment are typical, showing the profound result of the relativity of simultaneity. Though highly idealized, such observations seem quite realistic. Of course, the train has to go at a speed approaching the speed of light in order to measure the effect, but in the thought experiment we nevertheless see as we would see were the situation a real experiment.
There some thought experiments that play an important role in special relativity that are puzzling and lead to problems, if we try to characterize them this way. The car- garage (or the pole-barn) examples give us cases where, as normally presented, yield straightforward cases to be dealt with. Unfortunately, they violate the requirement that a thought experiment be as realistic as possible when it comes to what we would see. The fact is, we would not see Lorentz contraction (as is normally claimed in these cases), but rather we would see rotation. Instead of the car looking contracting to fit

inside the garage, it would look rotated and would slam sideways into the garage. The realistic thought experiment does not lead us to the right conclusion.
This talk will address this problem and offer an answer to what it is we actually do see in thought experiments such as the car-garage example. Metaphors such as "the view from nowhere" or "the view from everywhere," and so on will be found wanting.

Kimberly Brumble, U. of Calgary
Climate Models and the Spectrum of Virtual Experiments
Session II.1C

There has been skepticism in the past about experimental practices which utilize surrogate materials rather than the actual materials in which a particular phenomenon appears "in the wild." This skepticism shares features with much of skepticism regarding the robustness of climate models (particularly General Circulation Models) and their inferential power. Much of this skepticism derives from the concern that the use of surrogate materials in an experiment necessarily involves doing an experiment which may involve manipulation of materials very different from the 'target' materials and thus can involve substantial abstraction and "idealization." In this paper, I will address this skepticism by demonstrating that there exists a class of virtual experiments (that is, experiments done with surrogate materials and thus high degrees of idealization) which differ only in degree, rather than in kind, from experiments done in the materials of their target phenomena. I will argue that this class of virtual experiments forms a spectrum and that recognizing the existence of this spectrum suggests the success conditions for such experiments; I will argue further that this approach permits the development of success conditions even for virtual experiments that are as highly "idealized" as thought experiments and experiments involving simulations, models, and theory- dependant apparatuses, and that thinking about such experimental procedures as a class allows the lessons of the literature on each to be applied usefully to the rest. I will show how thinking about this spectrum of virtual experimentation quells anxiety about vast climate models by providing success conditions for semi-empirical computer simulations.

Christopher Chalmers, Dalhousie U.
The Role of Values in Definitions of Health and Disease in Psychiatry
Session II.3A

Theories of health and disease are typically divided into three types. Naturalist theories focus on biological normality in humans. Normativists understand health as states we desire, while diseased states are to be avoided, and so health and disease turn on value judgments. Hybrid theories combine naturalism and normativism suggesting that defining disease or disorder involves both objective criteria and value judgments. For mental disorders, hybrid theories typically provide a list of potential mental disorders which is then culled by value judgments. I briefly review the benefits and criticisms of each theory and alternative attempts to capture the benefits of the hybrid approach. These attempts fail because of the underlying assumption that there is a value free way of listing various behaviors or state descriptions which we can then apply our values to. Instead I hold that there are many different ways of describing the same behavior and this plurality of descriptions may not suggest a lack of objectivity, but instead may suggest that there are multiple equally objective ways of listing the behavior in question. Therefore values are involved one step earlier than hybrid theorists think as we will often require values to choose from the various ways we could describe our behavior before we even ask the

question of whether or not the behavior should count as a mental disorder. Clarifying the role that values play in describing behavior provides us with a more realistic and legitimate hybrid account of psychological health and disease.

Melissa Charenko, IHPST, U. of Toronto
Responses to Darwin's use of a Malthusian Metaphor in Canada
Session I.2B

In The Origin of Species, Darwin uses the Malthusian metaphor of a struggle for existence to explain how inter-specific and intra-specific competition result in the fittest individuals surviving and leaving progeny. My paper looks at historical responses to this metaphor in Canada, where the idea of a struggle for existence was not always understood in these ways and was either dismissed or accepted based on local environments, both physical and social, rather than the argument presented by Darwin. Some Canadians dismissed the metaphor because the idea of ruthless competition did not accord to their vision of the harmony within nature. Others felt that since a struggle for existence was related to natural selection and speciation, the metaphor must be false because it threatened their belief that species were immutable and created by God. At the same time, the idea of a struggle for existence was a common theme in Canadian literature. It also played a role in Canadian politics and visions of Canada's future. The practical sciences being performed across Canada also seemed to accept the notion of a struggle for existence in nature. By looking at formal reviews of The Origin, presentations before scientific societies, Canadian literature, political messages, letters, and scientific endeavors being undertaken, I examine the various responses to Darwin's use of the Malthusian metaphor in Canada and suggest the factors that led to the different reactions.

Sheldon Chow, Philosophy, U. of Western Ontario
What's the Problem with the Frame Problem?
Session II.1A

One of the most difficult problems that cognitive science has run up against is understanding how a cognitive system (such as that which humans possess) determines, from all that can possibly bear on a given task, what is relevant. In the cognitive science literature, this problem is more commonly referred to as "the frame problem". The frame problem was originally a problem for logic -based Artificial Intelligence (AI) research. Philosophers, however, have interpreted AI's frame problem as an epistemological problem for human cognition. As a result of this reinterpretation, however, specifying what the frame problem is has become a difficult task. In fact, as it is generally understood nowadays, the frame problem constitutes a set of closely related problems for computational cognition. To get a better idea of what the frame problem is, how it gives rise to more general problems of relevance, and how deep these problems run, I will expound six guises of the frame problem and explain their philosophical significance. I will then suggest that, contrary to the views of some philosophers, human cognition does not solve the frame problem in its epistemological guise, but I will explain how human cognition may avoid some of the epistemological worries.

Patrick Clipsham, U. of Western Ontario
Does Empirical Moral Psychology Rest on a Mistake?
Session III.1A

There are two dominant approaches that are employed in empirical investigations regarding the psychology of commitment to moral norms. The first takes certain token judgments as uncontroversially moral and investigates the psychological and neurological bases of these tokens. The work of Joshua Greene is typical of this kind of approach. The second begins with an articulation of what constitutes a moral judgment and then seeks to explain how empirical psychological investigations evolutionary models can inform metaethical debates about the nature of moral reasoning, moral judgment and moral motivation. This approach is most prominently found in the work of Sean Nichols, Don Loeb, Richard Joyce and Jesse Prinz. In one case, Nichols et al. begin an empirical investigation of moral motivation by presuming that the objects of moral judgments are all and only voluntary acts. Loeb and Joyce both argue that surveys of linguistic intuitions could resolve metaethial debates about whether or not moral properties are thought to be objective or subjective. Prinz and Nichols endorse a view of morality that construes moral judgments as authority-independent and as essentially involving the emotions of anger and guilt. I argue that Greene's approach to studying the psychological and neurological bases of moral judgments is more fruitful, as the other approaches are set up in such a way that they presume answers to important metaethical questions.

Tricia Close-Koenig, DHVS/LESVS, Université de Strasbourg
Catalogues, logbooks and atlases: Inventory management in 19th and 20th century pathological anatomy
Session II.3B

Pathological anatomy practices of the nineteenth century were natural history practices: collecting, describing, comparing, classifying, naming, and displaying. This meant that specimens travelled from the morgue to shelves of a cabinet in an anatomy theatre or a museum to the pages of a catalogue. The pathological anatomy collection at the Strasbourg medical school was renowned and, not surprisingly, eight catalogues were published between 1820-1863, and a centralized catalogue, Pathologisches Institut Strassburg Museal-Katalog, was maintained in the pathology institute until 1919.
Pathological anatomy practices of the twentieth century were notably laboratory practices: preparing and examining histology slides. From 1919, inventory efforts focused on recording histo-pathology examinations in laboratory logbooks. At this time, collecting was no longer a means but a side effect or an outgrowth of an emerging diagnosis service.
Strasbourg's pathologist institute director, Pierre Masson, was involved in publishing an Atlas du Cancer, an iconographic catalogue of microscopic tissue structure, between 1921 and 1930. An atlas evokes a return to, or perhaps a persistence of, natural history practices in the laboratory. I suggest that the means of organizing catalogue, logbook or atlas entries are telling of collection and specimen use in research and in diagnosis. The recording process was not unlike inventory management or bookkeeping, as I will illustrate. This paper presents one element of a historical case study examining a medical school pathology institute laboratory through a lens of business administration and economics to reveal underlying research and diagnosis dynamics.

Ken Corbett, History, U. of British Columbia
Chronic Anxieties: Railways, Telegraphs, and Punctuality in Victorian England
Session III.2B

During the late nineteenth century railway and telegraph networks in Britain presented new practical issues to their users. Fostered by passengers' emphasis on speed, regularity, and punctuality railways

presented the opportunity for users to develop a heightened awareness of clock time. Although railway companies sought out a rational time standard passengers did not always follow suit by making railway time their own. However, once passengers did adopt railway time they quickly understood that the standardization and coordination promised to them did not exist. They took to newspapers or the courts in attempting to expose asynchrony and unpunctuality. Through such efforts users of technology contributed to a public debate over cultural norms about time.
I argue that railways, telegraphs, and their accoutrements presented a landscape in which a tripartite problem developed for users. First, these technologies created the possibility for increased concern over clock time. Second, as a byproduct of this heightened concern for public time measures, passengers adopted the unified systems which railways and telegraphs promoted. Finally, and again a product of their increased sensitivity to timekeeping, passengers discovered that uniformity and coordination did not exist. These electro-mechanical networks simultaneously promoted synchrony and asynchrony, punctuality and unpunctuality.

Michael Cournoyea, IHPST, U. of Toronto
Untangling Complexity and Pluralism in Medical Explanations
Session III.1B

Complexity theory and explanatory pluralism have become central themes in the philosophy of medicine. Complexity theory offers an integrative and multidimensional approach for defining health and disease while explanatory pluralism recognizes the mutual importance of biomedical, phenomenological, and epidemiological etiologies. The goal is epistemic completeness: proponents argue that health and disease cannot be sufficiently defined or explained by any single etiological dimension, thus pluralistic integration is crucial. My talk addresses the epistemic coherence of these approaches while critiquing the plausibility of integrating complexity and pluralism into medical discourse. I focus on the underlying 'etiological stance' of these approaches, which emphasizes the centrality of causal explanations in disease recognition, treatment courses, and research directions. While an integrative and pluralistic etiological stance may help to reconcile the causal diversity of modern medicine, these approaches are insufficient when attempting to define the complexity of health. The uncertain place of the patient also remains obscure, as medicalized subject and object, even in the light of such pluralism. My aim is to reevaluate these epistemic trends and untangle the causal foundations of thesaristore integrative approaches to health and disease.

David Crawford, Philosophy, Duke U.
Biological Evolution and Statistical Mechanics: Re-placing Fisher's Analogy
Session I.3A

Statistical mechanical theory played a major role in Fisher's contribution to the birth of mathematical population genetics. Fisher had a significant background in physics and he saw in Boltzmann's work in statistical mechanics a counterpart to Darwin's work in evolutionary theory. Fisher often drew comparisons between his models and the Kinetic Theory of Gases, and he likened the role of his Fundamental Theorem of Natural Selection in evolutionary theory to the role of the Second Law of Thermodynamics in statistical mechanics. In this paper I elaborate how Fisher employed conceptual tools from statistical mechanics in his population genetics models. I argue that Fisher did so without an adequate understanding of the formal roles or the empirical justifications of those concepts and models in physics. Three factors make the formal basis of

Fisher's theory in Boltzmann's problematic: (a) the (in)sufficiency of basic model parameters for the systems studied (e.g., sample size, timescale, independence of units); (b) the empirical justification for idealizations (both idealized relations like Boltzmann's Stosszahlansatz and idealized models like ensembles); and (c) the different roles of 1st- and 2nd-order relations and properties in the hierarchical models. I argue that the shortcomings of Fisher's applications reveal important features of physical systems which make Boltzmann's idealizations useful and they demonstrate how theorists can misapply statistical mechanical principles across disciplinary boundaries when they fail to consider these empirical constraints. I conclude with a discussion of how Fisher's importation of conceptual tools from physics continues to affect contemporary discourse in evolutionary biology.

Michael Cuffaro, Philosophy, U. of Western Ontario
Many Worlds, the Cluster-state Quantum Computer, and the Problem of the Preferred Basis
Session III.2C

The source of quantum 'speedup' - the ability of quantum computation to achieve, for some problem domains, a dramatic reduction in processing time over known classical algorithms - is a matter of philosophical debate. On one popular view, the speedup stems from 'quantum parallelism': a quantum computer's purported ability to simultaneously evaluate functions for multiple values of their input. For many, this is evidence for the many-worlds explanation of quantum computation, an offshoot of the many-worlds interpretation of quantum mechanics. On this view, quantum computers literally perform multiple function evaluations simultaneously in different physical universes.

The many-worlds explanation is highly intuitive. For some, it is the only possible explanation of quantum speedup. In this paper, however, I argue that the many-worlds explanation of quantum computation is not licensed by, and indeed is conceptually inferior to, the many-worlds interpretation of quantum mechanics from which it is derived. While the decoherence basis fulfils its role, in the many-worlds interpretation of quantum mechanics, of determining the preferred basis for world decomposition with respect to macro experience, the criterion for world decomposition in the context of quantum computing is, I argue, ad hoc. Further, I argue that the many-worlds explanation of quantum computation is incompatible with the recently developed cluster-state model of quantum computation, where the determination of a preferred basis is precluded by the nature of the model. Since alternative explanations of quantum speedup exist that do not suffer from these problems, I conclude we should reject the many-worlds explanation of quantum computation.

Michael Da Silva, U. of Toronto
Community Mental Health in Central Ontario in the 1970s
Session I.3C

This paper focuses on the impact of the Community Mental Health Movement (CMHM) in Canadians institutions. The Peterborough Regional Health Centre, Mental Health Services' "Nicholls Building" is used as a case study in Community Mental Health in Canada. Following the rise of the CMHM in the United States during the 1960s and 1970s, a similar interdisciplinary movement occurred in Canada (see the McMaster Health Sciences network of mental health services). When the Nicholls Building became a mental health institution in 1978, the CMHM was well-established in mental health care. Specialized programs like the day hospitals appear to have some relation to the CMHM, but the extent of the facility's interdisciplinary work remains in question. This paper uses the Nicholls Building's commitment to the movement in the late 1970s as a possible barometer for the health of the movement in Ontario during that era. By the 1970s, articles like Israel Zwerling's "The Impact of the CMHM on

Psychiatric Practice and Training" were already focusing on the consequences of the movement. This project's local focus and historical distance, however, makes it unique.

Karl Degré, U. de Montréal
Ecosystems, communities and mechanisms
Session II.2C

The concepts of ecosystem and community are central to ecological explanations. However, since the introduction of these concepts, their ontological status is controversial. Taking as a starting point the mechanistic explanatory theories in philosophy of science, I suggest that ecosystems and communities are mechanisms. More precisely, I suggest that they are entities and activities organized in such a way as to exhibit regular and precise phenomenons (Machamer, Darden, Craver 2000). While entities are the components of the mechanisms (ex: species, ecological niche), activities are the causal relations that produce changes (ex: photosynthesis, predation). Finally, the interaction of the entities and the activities produce emerging properties that are unique to the community and ecosystem level (ex: pH, biomass). By using the manipulationniste theory of Woodward (2003) and the experimental results of Swenson and Wilson (2000), I argue that it is possible to identify the causal components of the ecosystems and communities and their relations to one another. Since it is possible to manipulate empirically and counterfactually ecosystems and communities, I conclude that they are real entities.

Robert DiSalle, Philosophy, University of Western Ontario
Explanation, explication, and interpretation of space-time
Session I.2A

Recent literature re-examines the meaning of Lorentz invariance and its relation to the structure of space and time. This has raised questions not only about the ontological significance status of space-time structure, but also about the distinction between fundamental and derived principles in physical theories, and the nature of theoretical explanation. I propose that space-time theories are particularly characterized by one particular aspect of principle theories: that they express " criteria" which natural processes " have to obey." The question I would like to consider is how certain principles come to have the force of criteria in this sense. Instead of a dynamical explanation of how processes or systems come to satisfy these criteria at the phenomenological level, or even a deductive-nomological explanation of how they follow from an underlying structure, I consider the sense in which these criteria are definitive, or constitutive, of fundamental physical properties of dynamical systems.

Alex Djedovic, IHPST, U. of Toronto
In Defense of Laws in Biology: Realist Laws and the Evolutionary Contingency Thesis
Session I.1C

It is widely presumed that biology is a "nomological vacuum" or "nomically inhibited"— i.e. there are either no laws in biology or only a handful (Rosenberg, 2001; Hamilton, 2007). This view is driven by the evolutionary contingency thesis (ECT), which uses the unavoidably historical nature of evolution as an argument against any universal generalization holding in biology across the span of evolutionary time (Gould, 1989; Beatty, 1995, 2006). In order to

clarify the debate, greater precision is needed on the concept "law of nature". This paper argues that the most fruitful approach is a reading that emphasizes necessity instead of universality. In addition, a close look at the ECT reveals that there are two components to the thesis: unpredictability and causal dependence. I argue, against Beatty (2006) that these aspects need not be seen as complementary. A reading that emphasizes the causal dependence aspect of ECT is not in conflict with a nomological account. An analysis of the ECT in terms of chaotic dynamical systems shows that unpredictability collapses into causal dependence. Therefore, if the causal dependence aspect of ECT is emphasized, and the necessitarian reading of "law of nature" is emphasized, there can be laws of nature operating in biology.

Emerson Doyle, Philosophy, U. of Western Ontario
Some Remarks on the Notion of `Empirical Fact' in Carnap's The Logical Syntax of Language
Session I.3B

The notion of a linguistic framework or language is essential to Carnap's rational reconstruction of the total language of science. Carnap's insight here is taken to be the recognition that questions of ontology, truth, and justification should always be relativised to within the context of some particular linguistic framework, as from without such questions can make no sense, and so land us in philosophical confusion. Likewise, it is only from within a framework that we can demarcate a clear analytic/synthetic distinction, and so distinguish the factual from the non-factual components of our knowledge. It has been argued, most directly and forcefully by Warren Goldfarb and Thomas Ricketts, that Carnap's relativisation of epistemological notions to particular languages carries right through to the notion of empirical fact. This is easy to suppose, as it seems concordant with the general spirit of Carnap's Principle of Tolerance, broadly construed. I contend however that this interpretation turns Carnap into quite an extreme relativist, and runs counter to his general attitude toward the methodology of both formal and empirical sciences as genuinely progressive enterprises. In fact, I will argue that a close reading of sections 51 and 82 of Logical Syntax show that Carnap must have some language-transcendent notion of empirical fact at work. Furthermore, my reading gives the Principle of Tolerance a much more confined role in Carnap's overall project, but locates its motivation and justification as methodological tool licensed by Carnap's analysis of the formal sciences.

Emma Esmaili, Philosophy, U. of British Columbia
Rationality's Evolution: Against the (Fine Descriptive) Grain
Session II.1A

According to Kim Sterelny (2006), general explanations that abstract away from the details of the organism and its environment are not likely to get at the evolution of rationality. Such an account argues that rationality evolved in response to a specific human informational environment, and thus requires a narrow, fine-grained level of explanation. I first present some ways of distinguishing between specific and general levels of explanation. I then claim that more general explanatory levels, in a particular sense, are not only useful but they are necessary for an explanatorily correct and an ontologically complete account of rationality's evolution. That is, more general explanations may not only lead us to more consistent, deeper explanations in terms of predecessor components, which narrow explanations treat superficially or overlook entirely, but they may also capture architectural components relevant to rationality's evolution that fall outside the purview of narrow explanations.

Yiftach Fehige, IHPST, U. of Toronto
Back To Kant? Marco Buzzoni on Scientific Thought Experiments
Session I.1A

As intriguing and thought-provoking thought experiments themselves is the ongoing discussion of their scientific merits. While thought experiments have been conducted since antiquity, it was not before Kant's Experimente der reinen Vernunft (experiments of pure reason) that first steps were made toward a philosophical investigation into their nature. Do they really work without the help of real-world experiments? If so, how do they work and by what criteria to tell apart good from bad thought experiments? The spectrum of answers is wide. Recently Marco Buzzoni has added a Kantian account. In his view thought experiments and real-world experiments need each other, like reason and experience in Kant. No real-world experiment without thought experiment, and no thought experiment without real-world experiment. I will first present Buzzoni's central argument to support this view, and subsequently show why it is indeed a good idea to go back to Kant in order to address the epistemological challenge that thought experiments pose. Yet, in a third step I will entertain a number of arguments why I am not convinced by Buzzoni's Kantian theory of thought experiments. Two mention here only two: It seems that Buzzoni conflates imagined real-world experiments with thought experiments. Second, Buzzoni's reading of the Kantian a priori makes it either unclear what exactly it is that carries the epistemic power of unexecuted thought experiments, or runs the risk of being trivial –trivial insofar as Buzzoni would simply mean to say that you have to think before executing real-world experiments.

Jill Fellows, U. of British Columbia
Objectivity and Trust: Downstrean of the Expects
Session II.3A

Naomi Scheman argues that one reason people concern themselves with objectivity is in order to gain the trust of others. Based on this, Scheman asks whether there are cases where the public is rational in distrusting institutions that claim to disseminate objective knowledge. Heidi Grasswick, building on Scheman's work, suggested a method for regaining and maintaining trust between institutions and the public. This suggestion relies on the willingness of institutions to share knowledge with the public, and to identify correctly who needs to know and what they need to know. In this paper, I will apply the work done by Scheman and Grasswick to the ecological example of marine-protected areas. The example of scientific and lay collaboration in developing and maintaining marine-protected areas generally provides support for Grasswick and Scheman's theories. However, the examples challenges some of Grasswick's assumptions, particularly when it comes to understanding who needs to know, and how knowledge is being disseminated. This examples leads me to modify Grasswick's suggestions. I will argue that marine ecology demonstrates that, in order for a scientist to maintain the trustworthy position of objectivity, he or she must be willing to listen to lay-knowledge, and not only disseminate scientific knowledge. The scientist must recognize that he or she cannot claim to be an expert in all areas, and must be willing to listen to lay expertise. I will argue that this new conception of what is required for objectivity, requiring inter-community dialog and trust, has both epistemic and ethical value.

Andrew Fenton, Bioethics, Dalhousie U.
Letitia Meynell, Philosophy, Dalhousie U.
Cognitive Ethology as Ecofeminist Methodology
Session III.1A

At first glance, the life sciences appear to be an unlikely place to find methodologies and ontological assumptions that are conducive to supporting a view of animal subjects that implicitly endorses an end to their oppression by humans. For example, the biomedical sciences have institutionalized invasive non-human animal trials, many of which inevitably end in 'premature' death (i.e., death earlier than typical in an animal's life cycle). Paradoxically, however, some of the most ethically troubling research (e.g., deprivation studies) have given human's reason to rethink our relationship with non-human animals and brought into question the very norms that judged the research morally acceptable in the first place. The paradox not merely challenges traditional researchers by suggesting substantive changes to current research practice, but also challenges those who oppose animal research by curtailing an important source of knowledge of animal minds.
Though we might be tempted to restrict animal research to only those animals with limited or no sentient capacities, another strategy is available. In this paper we will argue that cognitive ethology offers a method for scientifically investigating nonhuman animals within a framework devoid of a logic of domination and engaged with nonhuman subjects on their own terms. This relevantly resembles important insights in Ecofeminism about respectful engagement with animals other than humans. As a scientific methodology, cognitive ethology not only treats many nonhuman animals as subjects and agents, it conforms to feminist method as described by feminist philosophers of science. Cognitive ethology thus offers an ecofeminist methodology for understanding our nonhuman kin.

Curtis Forbes, IHPST, U. of Toronto
Two Kinds of Abstraction
Session III.2A

Several recent philosophical accounts of scientific representation (e.g. Cartwright 1989, Jones 2005, Chakravartty 2007, van Fraassen 2008) have been based around the idea that accurate representations are often achieved by employing two related but distinct types of misrepresentation: idealization and abstraction. Subsequent elaboration of these accounts has focused on either distinguishing different varieties of idealization (e.g. Weisberg 2006, 2007) or determining an epistemology of idealized representations (e.g. Cartwright 1999, Rol 2008, Pincock 2008, Sugden 2009). Relatively little work has been done, by contrast, on either analyzing varieties of abstraction or determining an epistemology for abstract representations. This is especially concerning given that many of those in agreement over the importance of abstraction and idealization for scientific representation (e.g. Chakravartty, Cartwright, Rol, Jones, Weisberg, etc.) often seem to have very different, even contradictory things to say about the nature of "abstraction"; Chakravartty (2007), for instance, claims that a particular representation's "degree of abstraction" can be measured simply by counting the number of abstractions involved, while Cartwright (1989) explicitly denies that counting abstractions in this way is even possible.
On my analysis, these apparent disagreements result from a failure to distinguish two distinct types of "abstraction," where one kind of abstraction necessarily involves generalization, and the other does not. By exploring the nature of these two kinds of abstraction, I argue, we can better understand the many ways that idealization and abstraction help us achieve more accurate representations in science.

Karine Fradet, Philosophy, Université de Montréal
Laws and contingencies: structural similarities in biological and anthropological explanations
Session I.1C

Because of their focus on human beings, social sciences are often treated apart from other, "harder" sciences. Many philosophers have argued that the human actions studied by social scientists will not fall under exceptionless laws of the type found in physics. Indeed, there always seems to be an exception to any generalization (Harris 1968). One such line of argument maintains that, on the basis of our free will, human actions are not constrained by any kind of laws (Dépelteau 2000), and some concluded that disciplines such as sociocultural anthropology should not be sciences in search of laws, but interpretive disciplines in search of meaning (Geertz 1973). I will argue that focusing on free will misses the mark and that the explanations provided by the social sciences are not different from those in the natural sciences. I will focus on often overlooked structural similarities between biological explanations and anthropological explanations. I will argue that historical contingency (Beatty 1995) and supervenience (Rosenberg 1994) do not make biological laws impossible and that the same argument applies for anthropological explanations and, arguably, for social sciences in general.

Lisa Gannett, Philosophy, St. Mary's U.
The 'New Systematics', Genetics, and Race
Session I.3A

Huxley's 1940 edited collection, The New Systematics, and Mayr's 1942 monograph, Systematics and the Origin of Species, introduced the "new systematics." The first section of the paper examines the reception of these works and what was new about the "new" systematics. The second section of the paper situates Dobzhansky's redefinition of race as a genetically distinct Mendelian population and introduction of the typological-population distinction within the context of the new systematics and surrounding debate about the legitimacy of infraspecific taxonomic categories. The third, and final, section of the paper explores the failure of the genetic race concept to attain hegemony despite extensive efforts made to promote acceptance of the "evolutionary synthesis" among anthropologists as well as biologists.

Paul Greenham, IHPST, U. of Toronto
The Lutheran Body and Textual Rhetoric: Philip Melanchthon's Understanding of the Body as Rhetorical Text
Session I.1B

Philip Melanchthon added intellectual clout to the Lutheran reformation through his use of Aristotelian philosophy and Renaissance Humanist educational reform in the Wittenberg curriculum. Melanchthon wrote not only on theology, however, but delved heavily into anatomy in his commentary on Aristotle's treatment of the soul, the Liber de anima (1553). Melanchthon's views of the body reveal interesting similarities to his rhetorical understanding of scripture. This talk presents the possibility that Melanchthon used specific features in his understanding of scripture as part of his presentation of the body, demonstrating the effect his biblical hermeneutics (theory of interpretation) had on his understanding of the body. These similarities come out in an analysis of the Liber de anima. Melanchthon's presentation of the body in the Liber de anima mirrors his rhetorical hermeneutic as it includes an analysis of specific terms or grammatical phrases, an attempt to read the message of the whole and to discern the primary argument from

the structure of the whole, and a conception of that main argument as clear and discernable to those with a basic rhetorical training.

Ernst Hamm, STS, York U.
Classifying and Depicting the Earth and its History
Session II.1B

The decades around 1800 have been described as witnessing the transition from ahistorical natural history to the history of nature. For the sciences of the earth this has been described as the move from mineralogical classification to historical geology and, by implication, the move from a nascent science to one that is more fully fledged. Besides having a very strong teleological character, such an account glosses over the important ways in which classification continued to be important, even in geology. This paper will examine particular kinds of mineralogical drawings and pictures and the way they were employed around 1800, especially in connection with the Freiberg Mining Academy, to show that classification did not go away or even diminish in importance, rather that it continued and was very much a part of new ways of thinking about the earth.

Sarah Hogarth, Philosophy, U. of Western Ontario
Charles Darwin's Reading of Sir John S. Sebright
Session I.2B

Charles Darwin's use of the analogy between artificial selection (i.e., the breeding of domesticated species) and natural selection, both in the *Origin of Species* and elsewhere, has been the subject of much scholarly debate. This paper focuses on the relative significance of early-nineteenth-century breeding pamphlets in the development of Darwin's analogy between artificial and natural selection. In his paper 'Charles Darwin and Artificial Selection' (1975), Michael Ruse discusses two breeding pamphlets (one by John Sebright and the other by John Wilkinson) and the ways in which they present significant analogues to the theory of evolution by natural selection. Ruse demonstrates that Darwin read these pamphlets, makes a thorough survey of the ways in which they implicitly present artificial selection as analogous to natural selection. Ruse argues from this that Darwin was heavily influenced by these pamphlets as his own theory developed. This paper examines what Ruse has neglected in his study: namely, the ways in which artificial selection, as presented by Sebright and Wilkinson, is *dis*analogous with Darwin's theory of natural selection. As supporting evidence, this paper appeals both to the content of the pamphlets themselves, and to the marginalia in Darwin's own copy of the Sebright pamphlet, arguing that Ruse has failed to properly assess the evidence they provide, and that his conclusion is therefore inadequately supported.

Jennifer Hubbard, History, Ryerson U.
(Dis)figuring Models: Heuristic Devices or Perceptions of Reality in Fisheries Science?
Session II.1C

Mathematical modelling in fisheries biology began in the 1930s and continues to be an integral component of fisheries science and fisheries management. A complex of different approaches and understandings of the power of mathematical models developed as models became increasingly mediated through the use of ever more powerful computers. This paper will distinguish between the two major functions of models produced by fisheries biologists: viz. models designed to test known

parameters and find the best fit for a given management problem, versus the use of models to forecast the future of fisheries for sustainable management. The past use of models to forecast fish populations has proved to be highly problematic, and raises questions as to the extent to which fisheries scientists have used their models purely as heuristic devices, or whether numbers of scientists have instead come to understand them as reflecting reality, as somehow reified in nature. A historical examination of the manifold sources of problems dogging the use of population models for forecasting sustainable fisheries offers compelling insights into how the understanding of models affects scientific perception in fisheries biology, and also offers important parallels for current controversies over the use of climate models.

Robert Hudson, Philosophy, U. of Saskatchewan
Carnap and 'Ecosystem'
Session I.3B

After years of neglect, the philosophy of Rudolf Carnap is receiving a greater focus from contemporary philosophers of science. One central issue in the scholarship surrounding Carnap's work is the role he assigns to his principle of tolerance in addressing traditional philosophical problems. My task is to provide an 'empiricist' interpretation of this principle and to apply it to a key debate in the philosophy of the environment, that surrounding the definition and ontological status of the notion of an 'ecosystem'. I argue that this 'empiricist' application of tolerance captures an important methodological train of thought in the work of practicing ecologists in their attempts to define what an ecosystem is. I also argue that this interpretation gives an overall preferable perspective on how we should address metaphysical problems (such as the problem concerning how we should view ecosystems). Particularly, it avoids a form of relativism that I claim afflicts 'non-empiricist' interpretations of the principle of tolerance, a relativism that I assert would detract from the attractiveness of Carnap's philosophy.

Molly Kao, Philosophy, U. of Western Ontario
From Foundation to Function: Rethinking the Role of Data in Science
Session III.2A

In Empiricism and Experience, Anil Gupta proposes an empiricist epistemology in which the rational contribution of experience to knowl- edge falls in the logical category of a function. I propose to apply this framework to scientific theorizing. The specific problem is that justifying the use of certain measuring devices seems problematic because often, the construction of theories requires reliable data, but acquiring reliable data requires the use of some theory to construct an accurate measuring device. I argue that we should think of the role of the data obtained in experiments as allowing scientists to move from a provisional view to particular judgments that then force a revision of those original views. Entitlement to judgments is then relative to a starting view and the data acquired. However, a series of data will often force the convergence of different views to the same conclusions about specific content, at which point scientists are entitled to those claims simpliciter. This model shows that although certain theoretical commitments may be invoked when designing measurement devices, agreement can legitimately ensue, eliminating the vicious circularity. Thus, if we conceive of data not as a foundation on which to build further theory, but as a function mapping views to judgments thus generating a revision process, its role becomes unproblematic. To support this claim, I examine in detail an early debate in thermometry concerning the reliability of various substances for constructing thermometers.

Jeff Kochan, Philosophy, U. of Kostanz
The Scope and Limits of Scientific Integrity
Session II.3A

A preoccupation with the integrity of the scientific enterprise brings with it specific commitments about what science is, and what it ought to be. Indeed, too strong an emphasis on scientific integrity may distract us from other important, perhaps essential, aspects of successful scientific practice. The American National Academy of the Sciences has defined ‘ scientific integrity’ in terms of both individual and institutional adherence to honest and publicly verifiable methods. I test this definition against a specific case: the decision-making process which preceded the 1986 Space Shuttle Challenger disaster. With this case, I hope to demonstrate two complementary points: first, that a too-strong focus on integrity may blind us to the possibility that reliable science may also depend upon methods which are honest but not publicly verifiable; and second, that some commentators have judged the integrity of science against standards which are overly idealized and inappropriate, thus promoting an image of science which fails to capture its reality as an extraordinarily complex, fallible, messy, but for all that wonderfully reliable, enterprise. These considerations point to further questions about the nature of scientific expertise and its relation to the concept of scientific integrity. We need to know what expertise is, and whether or not it can be adequately explicated in terms of publicly verifiable rules. This difficult question will have consequences for our understanding of, as well as the importance we place upon, the concept of scientific integrity, and also for the way we navigate over the rough waters running between successful science, on the one side, and democratic accountability, on the other.

Alexei Kojevnikov, U. of British Columbia
Space-Time and the Russian Revolution
Session III.2B

The paper describes the cultural background that inspired the concept of non-stationary Universe (or Big Bang cosmology, in contemporary terminology), pioneered in 1922-1924 by the mathematician Alexander Friedman in Petrograd. It explores the general reception of the Einstein-Minkowski ideas on relativity and the four-dimensional world in revolutionary Russia, and the intellectual turmoil they provoked among the educated public, including avant-garde painters, futurists poets, mystical philosophers, experimental physiologists, and pure and applied mathematicians. Revolutionary concepts of space and time stood at the center of the perceived affinity between the new art and the new science. Interactions and cross-fertilization between scientific and artistic inspirations during the utopian era unleashed an avalanche of weird theoretical hypotheses. In the words of a contemporary observer, "the bold flight of scientific creativity can only be compared to the fantasy of a poet, and the great novel achievements that apparently contradict common sense are growing upon the ruins of what was thought to be the unshakable truth." A comparative analysis of the theoretical speculations by an archeologist turned biophysicist Alexander Chizhevsky, a poet and historical numerologist Velemir Khlebnikov, a journalist turned theosopher Petr Ouspensky, and a mathematician and religious philosopher Pavel Florensky, helps reconstruct the common existential experience and cultural crucible that gave rise to the idea that Universe(s) too, can die and be reborn.

Cory Lewis, IHPST, U. of Toronto
Downward Explanation in Rayleigh-Benard Convection
Session I.2A

Some authors have argued that developments in the study of self-organization justifies the claim that properties of non-linear dynamical systems as integrated wholes help to explain the behaviour of their parts. I will argue that none of the major accounts of scientific explanation in the literature can be squared with this claim. Taking Rayleigh-Benard convection as my primary example, I will focus on the analysis developed by Hermann Haken of self-organizing systems. Haken's 'Synergetics' is cited by the above authors as one of the most important representations of complex dynamical systems, and the one which should lead us to believe that wholes can explain parts. After briefly introducing the phenomenon of Rayleigh-Benard convection and some of the ways it is represented scientifically, I will survey the major accounts of scientific explanation. I will try to show that, by the strictures of each account, while it may be right to say that system-level properties cause or explain other system level properties, it cannot be maintained that system-level properties cause or explain the dynamics of the elements of that system. My survey of accounts of scientific explanation will be divided into causal and non-causal accounts, and will include all of the standard ways of thinking about explanation (Deductive-Nomological, Statistical Inference, Unification, mechanical and causal/Kairetic). If my argument is sound, I will have shown the incompatibility of our best attempts to understand scientific explanation with the possibility of downward explanation.

Bernard V. Lightman, Humanities and STS, York U.
Science at the Metaphysical Society: Defining Knowledge in the 1870's
Stillman Drake Lecture

Debra Lindsay, History and Politics, U. of New Brunswick, St. John
From American Woodsman to Ornithologist: How John James Audubon Became a Scientist
Session III.1C

The legend of Audubon as a roguish genius equally comfortable in the halls of the London Royal Society as in the wilds of Labrador first emerged in 1868 from The Life and Adventures of John James Audubon the Naturalist, edited, from materials supplied by his widow by Robert Buchanan and then subsequently in 1869 in The Life of John James Audubon, the Naturalist, a corrective to the book credited to Buchanan but based on Lucy Audubon's original manuscript. In addition to disabusing readers who might believe the unflattering characterizations in the Buchanan edition, Lucy Audubon also redirected their attention to the fact that Audubon's claim to fame was as a naturalist not an adventurer. Almost thirty years later, Audubon and His Journals, with Zoological and other Notes by Elliott Coues, (2 vol., 1897) by Maria Rebecca Audubon, a granddaughter, appeared. Eschewing the life-and-letters genre adopted by contemporaries to memorialize family members in favour of a style more commonly associated with exploration journals such as the Lewis and Clarke Expedition (Coues, 1893), Maria Audubon published these volumes along with two more intimate biographical essays to draw attention to Audubon's scientific contributions. Raised in a household revolving around projects such as The Viviparous Quadrupeds of America and octavo editions of both Quadrupeds and Birds, Maria saw her grandfather as a scientist rather than as an adventurer (or perhaps even a naturalist), and she sought to preserve his pivotal role as an ornithologist for posterity. Many strategies, including obtaining assistance from influential members of the scientific community such as Ruthven Dean and

George Brown Goode, were employed as Maria Audubon introduced the idea that her grandfather was one of the most important ornithologists of the nineteenth century.

Stefan Lukits, U. of British Columbia
Information Theory and Bayesian Epistemology
Session I.2C

Information is a more basic epistemological concept than probability. It can be used to create the foundations for probability. It also delivers substantial epistemological results that cannot be obtained by using probability theory without the use of information theory. The paper shows in what ways information theory and probability theory are equivalent, and in what ways information theory is epistemologically prior to probability theory. A measure-theoretic proof is provided that Bayesian updating and the principle of minimal discrimination (using the Kullback-Leibler Divergence) are compatible. The paper considers Kolmogorov Complexity, Chaitin's incompleteness theorem, and their implications for an epistemology based on information theory.
As a consequence, while Bayesian epistemology largely proceeds by making use of the results of probability theory, there is a field of inquiry surrounding Bayesian epistemology which is not directly accessible to probability theory. The paper examines the way in which information theory and probability theory correspond (coming to some skeptical and to some positive conclusions) and which inferences between the two theories are therefore legitimate. Depending on the answer to this question, Richard Avenarius' Prinzip des kleinsten Kraftmasses (1876), Ernst Mach's economy of thought in physics (1890), Jaynes' principle of maximum entropy (1957), Solomonoff's theory of inductive inference (1960), Wallace's theory of minimum message length (1968), Akaike's information criterion (1974), and Rissanen's theory of minimum description length (1978) may assume considerable significance in developing Bayesian epistemology.

Dan McArthur, Philosophy, York U.
Marc Champagne, Philosophy, York U.
Addressing The Stance Stance; Voluntarism in Realism and Empiricism
Session II.1A

In his book the Empirical Stance and other more recent writings van Fraassen contends that his brand of empiricism is not a set of beliefs but rather that it is a “stance” or attitude that one has toward beliefs. The empiricist and the realist possess different epistemological values about what science can be and how it ought to proceed. Although one of van Fraassen main arguments is that stance choice is voluntary, he contends that empiricism is the superior stance relative to its rivals such as scientific realism and stances that he dubs “analytic metaphysics”. On the realist side of the debate Ross and Ladyman have recently adopted the “stance stance” as well. However, in their case they argue for the realist stance while sharing Van Fraassen's voluntarism. Like Van Fraassen, they also wish to reject stances that would count as what Van Fraassen calls analytic metaphysics. In this paper we will address the “stance stance” and its attendant voluntarism. We will argue that while there are some conceptual difficulties that attend the stance stance in general, both stance-realists and stance-empiricists make a similar mistake. This, we will claim, is to seek global stance to the realism question. We will argue that the realism question can be addressed more satisfactorily by adopting certain deflationary views that permit local rather than global solutions to the realism question. We will also try to show that when such a position is taken, the reasons for adopting realism or empiricism in a given situation can be more compelling than the voluntarism of the stance stance.

Duncan Maclean
Propensities and Rational Belief
Session I.2C

Objective chance constrains rational belief. The fact that a coin is fair constrains our belief to think it has a 50% chance of landing heads when tossed. Some philosophers take this to express a principle of reason: David Lewis (1994) dubs it the "Principal Principle". Lewis thinks his reductive theory of chance explains PP and that a theory of irreducible propensities cannot. For Lewis, statements about chances are statements about occurrences, since the former supervene on patterns in the global distribution of properties. Statements about future events coming to pass are also about occurrences. Lewis thinks statements about occurrences entail other statements about occurrences, so chance constrains rational belief via an entailment relation. But propensity theory cannot show how chance constrains rational belief, since propensities and occurrences are distinct sorts of entity. Statements about the one are thus logically independent from statements about the other. My paper will (1) critique Lewis's reductive account of PP and (2) formulate a reductive account of PP for propensity theory. The latter will be done by focusing on the real causal connections that obtain between propensities and their manifestations. While acknowledging that statements about propensities and occurrences are not logically connected, propensity theorists believe that propensities are metaphysically connected to occurrences as the causes of their manifestations. I examine the possibility of piggybacking the connection between statements about propensities and occurrences on the real causal connection between propensities and manifestations. The law of large numbers will prove helpful here.

Gordon McOuat, HSTP, U. of King's College
Are the life sciences truly revolting? Classifying, natural kinds and the birth of modern biology
Session II.1B

In his germinal *Life Sciences in the Twentieth Century*, historian of biology Garland Allen introduced the notion of a "revolt from morphology" as the chief characteristic in the birth of the "new biology" at the beginning of the twentieth century. In the face of blistering criticism, Allen quickly modified his notion to be a "revolt from natural history" ("Allen I" vs "Allen II", according to Fred Churchill's taxonomy). The critics weren't placated: the problem seemed to lie in "revolt" and discontinuity. The critics won: the consensus is, now, that continuity (conceptually, institutionally, theoretically) rather than "revolution" marks the changes in 20th century life sciences, and that any reference to revolt was merely rhetorical. This paper revisits the debate and calls for a revitalisation of Allen II, this time reconsidering the engagement with natural history and experimental biology in terms of "natural kinds" and "styles of reasonings". The consequences for the place of natural history and classification in modern life sciences are then explored.

Alexandru Manafu, Philosophy, U. of Western Ontario
Multiple Realization: Some Lessons from Solid-State Chemistry
Session II.2C

In the past decade or so, some of the philosophical discussions about reductionism and emergence have shifted from the philosophy of mind to the philosophy of physics (see, e.g., Batterman 2000; 2002). Many of these new treatments center around the idea of physical properties that are multiply

realized (same macro-level property being carried out by di?erent micro-level properties). The present paper aims to contribute to these discussions by presenting and examining the philosophical consequences of what I take to be a powerful example of multiple realization, one which comes from solid-state chemistry. I look at the phenomenon of piezoelectricity (i.e., the ability of certain materials to generate a surface charge when subjected to mechanical stress) and argue that piezoelectricity is best understood as a multiply-realizable functional property: being a piezoelectric is de?ned by the behaviour of the material in certain conditions, not by a single micro-level feature that is possessed by all these materials and only by them. I consider the origin of piezoelectricity in various materials and argue that the lower-level mechanisms involved are truly diverse: ionic displacement in crystals, dipole rotation in polymers, streaming current in interface materials like cement or bone. I explore the philosophical consequences of this fact with respect to the issue of inter-theoretic reduction and argue that the existence of multiply realizable properties like piezoelectricity supports an anti-reductionist attitude about solid-state chemistry in relation to the underlying micro-physics.

Benjamin Mitchell, STS, York U.
Occult Correspondences: W.T. Stead, the Community of Borderland and the Brahmins of Science
Session II.2B

The tension which exists in occult thought between the exoteric knowledge of the many and the esoteric, secret knowledge of the few could only have been exacerbated in an age of mass communication. Such was the state of Britain at the end of the nineteenth century. Improvements to the printing press and the boom of popular journals meant that audiences that had hitherto remained disparate or untapped were now being given a new sense of solidarity and common cause, provided that their interests were successfully maintained by some canny editor. These concerns came to a dramatic front in the pages of Borderland, edited from 1893 to 1897 by the radical journalist William Thomas Stead (1849-1912). The fact that such a journal received world wide circulation testifies to the existence of multiple occult communities in England and abroad. These groups coalesced around Borderland through Stead's creation of an occult library for all subscribers, his appeals to a participatory readership and the promise of legitimacy and protection from scorn that the community provided. Yet despite this, competing interest groups such as the London-based Society for Psychical Research (SPR) and "commercial occultists" with different ideals for what constituted the scientific study of the occult meant that an easily discernible Borderland community could not be maintained without the journal that defined it. Stead planned the journal's suspension to be a temporary hiatus, but without its presence, the public it had helped to define dissipated once again into factional and methodological isolation.

Lisa Mullins
The Diary of a Scientific Institution: Technological Revelations in the Meeting Minutes of the Académie Royale des Sciences, 1699-1730
Session III.1C

Much scholarly work has been done on the Parisian Académie Royale des Sciences, arguably the foremost institution of natural knowledge in the eighteenth century. Scholars are familiar with the Académie's practices and significant controversies, including the Académie's vicious debates on the new calculus and the shape of the Earth. However, little scholarly work has been done on the mundane, day-to-day existence and tasks of the Académie. What was a regular meeting like? How much time and effort was spent discussing papers? Doing experiments and demonstrations? Taking

care of administrative business? The first part of this paper, in the Annales tradition, answers these and related questions about the Académie in the first three decades of the eighteenth century; it does so through a detailed reading of the procès-verbaux (the minutes of meetings), from which a statistical overview is produced.
The majority of this paper is a discussion of an unexpected statistical result: the amount of technological consultation demanded of the Académie. In their 1699 statutes, the Académie was charged with examining and judging all new machines and inventions related to the sciences that were submitted to the Crown for a patent or privilege. The Académie received about forty such submissions a year, and each had to be examined by an ad hoc committee of academicians, and a report written. Perhaps most striking about this demand for expert judgement is the fact that the majority of academicians had no training in the 'crafts' or applied sciences of their time. This paper explores how the Académie became an 'expert' body on technological as well as scientific matters, and some of the problems academicians encountered in their role as arbiters for the Crown.

Taylor Murphy, Philosophy, U. of Alberta
Homology meets the imagination
Session III.1A

In the past 10 years, many philosophers such as Matthen and Griffiths have argued for a substantial revision in the way psychological traits are individuated. Since psychological traits are a subcategory of biological traits, these authors maintain, they can be individuated according to the biological classificatory scheme of 'homology' rather than 'shared function' or 'analogy'. Homology—which individuates traits according to common descent—is superior as it individuates traits that share 'deep causal commonalities', rather than for instance shared function, where the shared causal properties are often relatively superficial. Much of the philosophical interest in homology and psychological traits has centered around the emotions (e.g. Griffiths 2003, Clark 2010). However, the imagination is another topic of philosophical interest that is ripe for an analysis based on the notion of homology. I use motor imagination as a case study to argue that our capacities for imagination and pretence may be generally related by way of homology to our capacities to perceive and believe. More specifically, I argue that they are serially homologous—a special type of homology within one and the same species—using criterion informed by Love (2007) and Ereshefsky (2007). The upshot of such an analysis is that it, if correct, genuinely explains why pretense and belief are so similar, which Nichols (2004, 2006, 2008) has attempted to explain but whose explanation thus far arguably lacks sufficient explanatory force (Goldman 2006).

Kathleen Okruhlik, Philosophy, U. of Western Ontario
Mill and the Merchants of Doubt
Joint Session CPA-CSHPS

Dustin Olson, Philosophy, McMater U.
On the Passage and Perception of Time with Bertrand Russell
Session II.2B

In this paper I reveal an advance in the philosophy of time made by Bertrand Russell later in his philosophical career (1927-48). Here Russell develops a theory of spacetime order, specifically addressing the t variable in special relativity and how from this we can account for order and time

sequence, and as I argue the passage of time. This move by Russell addresses and, with a little work, accounts for problems still being discussed in the current literature, most relevant here being whether or not time's passage is compatible within a four-dimensional spacetime manifold. This paper has two goals: first, to advance a theory of time that rejects and ignores the traditional A/B distinctions while also accounting for becoming in four dimensions—this can be done by localizing and relativizing time; secondly, by using his approach as a model on which to base this project, I hope to reveal Russell's advanced theory of time as developed in his later period, which has been virtually ignored to this point. Furthermore, I argue that Russell's theory anticipates and, in some respects, is superior to Howard Stein's localized theory of time, and closely reflects more current localized accounts such as one finds in the work of Richard T. W. Arthur. It is my hope that we can advance the "time" debate beyond McTaggart's hackneyed A/B distinctions while recognizing Russell's continuing relevance, especially concerning his untapped later works in epistemology, science, and metaphysics.

Ray op'tLand, U. of Calgary
A Revised History of Computing in the Eighties
Session III.2C

The development of the personal computer in the 1980s gave rise to one of the more transformative technologies of the modern era. But the role that culture played in this development has been marginalized in many of the historical accounts of the period. In order to highlight the influence of culture on personal computing, I've constructed a time-line of the decade of the 1980s, charting the various actors and events that were involved, from computing science through to the arts. This historiography looks at how several of the key innovative technologies that made up the personal computer as an artifact were influenced by parallel developments in both the scientific and cultural spheres. Using an evolutionary perspective, I will demonstrate how the changes in computing in the 1980s cannot be reduced to a normative view of the people, businesses, or technology. Rather, a broader perspective must be engaged to provide a complete picture of the development of the now- ubiquitous computer.

Danielle Pacey, York U.
Organs at War: Eugen Steinach's "Battle of the Gonads", Sex, and Fin-de-Siècle Experimental Biology
Session II.3B

What circumstances make it possible to think of two organs as being at war with one another? Based on reciprocal transplantation experiments carried out in test animals, Viennese physiologist Eugen Steinach (1861-1944) found the action of male and female sex hormones to be antagonistic. He described the hormone associated with the testes as inhibiting the development of feminine secondary sex characteristics, while the hormone associated with the ovaries inhibited the development of masculine secondary sex characteristics. This led him repeatedly to describe sex antagonism as "the battle of the gonads" (*der Kampf der Gonaden*). Even as the theory – and the experiments which had shaped it – faced harsh criticism in the United States and England, Steinach's work was heartily taken up in the context of German sexology as a basis for theories of sexuality. I focus on the intellectual and institutional shaping of the metaphorical "battle of the gonads". I attend especially closely to the influence of developmental embryologist Wilhelm Roux, whose method of disrupting natural systems and bodies to deduce their normal functions – the "causal-analytical" programme – was embraced by the Institute for Experimental Biology (*Biologische Versuchsanstalt*) where Steinach carried out most of his work.

Valérie Racine, Philosophy, U. of Western Ontario
Evolutionary Explanations of Complex Adaptations: How do they get their explanatory force?
Session I.1C

In this paper, I address the challenge of how evolutionary explanations of complex adaptations get their explanatory force. To be precise; in what sense do they provide robust scientific explanations rather than mere plausible descriptions of (putative) adaptations? Philosophers and biologists, such as Brandon (1990) and Sinervo & Basolo (1996), have correctly indicated that explanations that appeal to natural selection must be testable in a way that meet certain specific requirements. Their checklist approach offers a reasonable way in which to conceive of and investigate prospective adaptations. I take a different approach in order to focus on the different types of causal relations within these explanations. I argue that these explanations get their explanatory force from the degree to which their components exhibit what Woodward (2010; 2001) calls invariance - i.e. the stability of a (causal) relationship under some set of interventions. I consider explanations of adaptations to be aggregates of adaptive and historical explanations. This conceptual division reveals the different kinds of causal relations involved in explanations of adaptations and this, in turn, is crucial for making explicit both the comparative and the historical rigour involved in explaining adaptations. I illustrate these claims with examples from studies on the thermo-regulatory behaviour of ectotherms. I conclude by arguing that this explication of evolutionary explanations - i.e. this method of approaching the initial question - is a valuable addition to the "checklist approach" because it allows for the evaluation of the explanations without appeal to a list of necessary and sufficient conditions.

Isaac Record, IHPST, U. of Toronto
Technological Possibility
Session II.2A

This paper is part of a larger project to understand the relationship between technology and scientific practice. In this paper, I will argue that technology is a constraint on scientific knowledge. The paper is divided into two parts. In the first part, I discuss "technological possibility," which depends on the availability of material and conceptual means to bring about a desired state of affairs. It follows from my definition that physical possibility and conceptual possibility are necessary but insufficient conditions for technological possibility. In the second half of the paper, I consider the relationship between technological and epistemic possibility. I first distinguish weak and strong notions of epistemic possibility. "Weak epistemic possibility" turns on what an agent can rule out on the basis of his or her current mental state, while "strong epistemic possibility" obtains only when requisite investigations have been undertaken. Scientific knowledge claims typically involve epistemic possibility in the stronger sense, as when experimental investigations are needed to establish the claim as legitimate. Moreover, the "requisite investigations" of science now typically depend on available technology. A more precise way to put the point is that technological possibility is a necessary but insufficient condition for strong epistemic possibility. I conclude by suggesting that some other constraints on knowledge-seeking activities (ethics, economics) can be understood in a similar way, and furthermore systematic relationships hold between some of these constraints.

Andrew Reynolds, Philosophy & Religious Studies, Cape Breton U.

Singing the Cell Electric: electronic engineering metaphors in the science of cell communication
Session II.3B

The recognition that cells within plant and animal bodies communicate with one another by means of electrical and chemical signals has been of monumental significance for the life sciences. The study of cell signaling pathways, the often elaborate causal routes by which cell morphology and behavior is influenced by reception of messages from other cells and external environmental cues, has left no area of biological and biomedical research untouched. As in other fertile areas of science, cell communication research is heavily reliant upon metaphor and analogy, in this case drawn from the field of electronic and computer engineering (e.g. 'signal transduction', cell 'circuits' etc.). But to what extent can these metaphorical models be taken as accurate reflections of cellular reality? Would the science be better off without the metaphors altogether? Recent publications suggest that scientists are split on the appropriateness of these metaphors (e.g. Bhalla 2003 for a pro- opinion and Pigliucci and Boudry 2010 for a contrary opinion.) I will discuss the positive and negative roles of metaphors in this research, what they suggest about the nature of knowing and understanding in science, and whether the centrality of metaphor here is compatible with a realist or an instrumentalist interpretation of the scientific theory involved.

Adam Richter
"Unrefined and Undisciplined Masses": Copernicanism, Esotericism, and Divine Accommodation
Session I.1B

The notion of divine accommodation, which suggests that God accommodated the language of Scripture to suit human understanding, was employed by several early modern thinkers in support of the Copernican theory. These writers, who range from Georg Joachim Rheticus in the early to mid-sixteenth century to Isaac Newton in the 1690s, argue that Copernicanism is not in conflict with passages of Scripture that suggest that the earth is stationary and the sun moves around it. Rather, Scripture has two meanings: an exoteric meaning that is accommodated to the understanding of its original audience (because, for the ancients, it did appear that the earth was at the centre of the universe) and an esoteric meaning, accessible only to those with sufficient education, that in fact reflects the heliocentric model of the universe. This paper argues that Copernican astronomers emphasized this exoteric/esoteric divide – or epistemological dualism – partly to place themselves in a privileged group with a unique ability to interpret Scripture.

Jonathan Simmons, U. of New Brunswick in Fredericton
Martin Seligman's Positive Psychology as a Scientific/Intellectual Movement
Session I.3C

Psychology has long struggled with fragmentation and unification, resulting in separatist impulses that transform theories into social groups rather than epistemic objects. These impulses derive from dissatisfaction with the methodological basis of psychology, providing an explanation for the emergence of Positive Psychology as a discrete approach to human behavior within the social sciences.

Positive Psychology, like any other scientific/intellectual movement (SIM) is political in that it constitutes a collective effort to pursue a research program in the face of resistance from others in the scientific community. In response to this resistance, the dominant message of Positive Psychology is one of separatism. In addition to distancing themselves from clinical psychology, positive psychologists have marginalized their intellectual forebears, disregarding humanistic psychology for being antiscientific and failing to develop a strong research tradition.
The purpose of this paper will be to develop an understanding of the micro-processes of Positive Psychology, bringing into focus the local institutional conditions and settings that might have played a role in its emergence as a SIM. I will argue that under the leadership of Martin E.P. Seligman, Positive Psychology is perhaps the clearest contemporary example of institutionally designed science and is fueled by Seligman's self-concept as a scientific warrior and light bringer.

Stephen Snobelen, HSTP, U. of King's College
Genesis and the Systema naturae: the theological correlates of the Linnean neologism homo sapiens
Session I.1B

When referring to the taxonomical designation *homo sapiens*, scientists commonly cite the tenth (1758) edition of the *Systema naturae*—the edition of Carolus Linnaeus' masterwork where this well-used term is introduced. Yet although this term enjoys widespread use both within and without science, it is doubtful that many have examined the section of the *Systema naturae* where the coinage appears. It is probable that even most scientists who cite the 1758 edition of this work refer to the title and date of the book merely by convention. The Latin of the *Systema naturae* also limits access of its contents to specialist historians. Nevertheless, for so common a term surprisingly little has been written on its origin. This paper details one element of the coinage: the theological correlates that appear in and around the entry for *homo sapiens* in the *Systema naturae*. Linnaeus places the coinage within a theological discussion of God's creation of and purpose for humans. This discussion draws on passages from Isaiah and Psalms and glosses the meanings of both elements of the term *homo sapiens* with allusions to the *imago Dei* (Genesis 1:27) and the first human's creation from the earth (Genesis 2:7). In so doing, Linnaeus articulates a fundamental paradox of humanness: the God-like mind subsisting with a humble body of organic origin. This paper both explicates Linnaeus's use of the Genesis Creation in his coinage of *homo sapiens* and sets this biological neologism within the wider theological themes of his work in natural history.

Mark Solovey, IHPST, U. of Toronto
Mike Thicke, IHPST, U. of Toronto

Social Science Indicators in Action: U.S. Senator Walter Mondale's Initiative to Create a Council of Social Advisers
Session I.3C

Between 1967 and 1974, U.S Senator Walter Mondale (Minnesota) put forth legislation to create a Council of Social Advisers (CSA) that would advise the President and produce an annual social report of the nation, based largely on data from social indicators. The proposed CSA was explicitly modeled after the Council of Economic Advisers. Mondale believed the CSA should both emulate and counterbalance its precursor's ability to measure, monitor, and manage the nation's welfare. Shortly

after Mondale stopped pursuing this initiative, it faded from view. It has also been overlooked by historians of the social sciences and American science policy. Yet this episode offers an excellent opportunity to explore conflicting views about the scientific foundations and policy relevance of the social sciences.
Our talk will describe Mondale's efforts, the involvement of social scientists and their views for and against his proposal, and the links between the political process and the social indicators movement. From a theoretical standpoint, by analyzing the various roles of ignorance in this episode, we suggest that the story of the CSA offers a useful way of extending the study of "agnotology" (as presented in Londa Schiebinger and Robert Proctor's recent edited volume on this topic) to include the history of the social sciences.

Ian Stewart, HSTP, University College
Natural Histories, Classification and Francis Bacon
Session II.1B

In the 16th and 17th centuries, the activity of classification of flora and fauna received a new impetus through, in part, the burgeoning genre of natural history, both in printed form and as a set of collecting/presenting practices. This paper will survey contemporary scholarship's recent interest in the quite varying practices and theoretical underpinnings of natural history at play in the period. It will focus primarily on the natural histories of Francis Bacon and their role in his 'Instauration' of the sciences, with particular attention on the consequences in that 'Instauration' for the very character and function of classification itself.

Brooke Struck, U. of Guelph
Scientific explanation and understanding
Session I.2A

Carl Hempel's D-N model of explanation, which relies on the logical structure of deducing the explanandum from the explanans by modus ponens, suggests that scientific understanding is articulated in terms of sufficient conditions. Several objections to the account suggest that scientific practice is not only focused on sufficient conditions, but rather on necessary conditions as well. In order to take this into account, I articulate the mD-N model of explanation, with the appropriate adjustments to the corresponding account of understanding and the "Why?" question associated with scientific investigation. With the newly articulated model in hand, I then return to those same objections raised against the D-N model, and show mD-N's success in overcoming these difficulties. The success of mD-N turns out to be reliant not only on the introduction of the component of necessary conditions to the explanatory model, but also on the reformulated "Why?" question and the definition of scientific understanding, these three aspects working in concert to assemble a coherent picture of scientific investigation. In closing, I discuss a distinction between two types of questions in science (following Philip Kitcher) and how they relate to mD-N, as well as drawing parallels between the mD-N model and the scientific method, and finally discuss similarities between mD-N and other models of scientific explanation (principally that of James Woodward).

Mike Stuart, IHPST, U. of Toronto
What Can Philosophy Learn from the Clock-in-the-Box Thought Experiment?
Session I.1A

John D. Norton believes that all thought experiments can be reconstructed as arguments. Michael Bishop challenged this notion in 1999, arguing that the clock-in-the-box thought experiment presented at the Solvay conference in 1930 by Einstein cannot be reconstructed as an argument. Norton responded to this challenge with a paragraph in 2004, and since then Bishop has been silent on the issue. However, this thought experiment continues to yield new results in quantum mechanics to this day. Also, it is allegedly something Bohr worried about all his life, since he sketched his solution on his blackboard the day he died. It has been cited as a forerunner of the EPR paper, and physicists agree that Einstein and Bohr both missed the point. Such an interesting case surely deserves more attention from philosophers of science.
Recently, James R. Brown has discovered a new type of thought experiment called a counter thought experiment, which he argues cannot be faithfully reconstructed as an argument. I show that the clock-in-the-box is a perfect example of this new kind of thought experiment, and then I use this result to explore and elaborate Brown's notion, and evaluate it as a challenge to Norton's account. First, I conclude that cases like these, in which a thought experiment continues to yield new results over generations, present special difficulties for the identity and success conditions of thought experiments generally. Second, I entertain some counter arguments on Norton's behalf, and conclude that Brown's argument, if valid, is not decisive.

Shelley Tremain, Wilfrid Laurier U.
Impairment and the Diagnostic Style of Reasoning
Session III.1B

Genealogy, the technique of investigation that Michel Foucault introduced in his late writings, has proven to be one of the most fruitful ways to articulate the mutability of the categories of gender and race, among other allegedly natural human characteristics. Until recently, discussions within disability studies and philosophy of disability have represented impairment as a transhistorical, biological entity, that is, as diachronically continuous, notwithstanding the fact that some theorists have allowed that the shape and character of this entity are subject to historical and cultural influence. A genealogy of impairment would trace the elaboration of the idea and materiality of impairment by and through popular, philosophical, medical, juridical and other discursive practices in order to demonstrate that impairment is not a transhistorical and value-neutral human characteristic, but rather is the naturalized and materialized outcome of a classification initially generated in medical and juridical contexts to facilitate normalization. In particular, a genealogy of impairment would show how impairment is naturalized and materialized by and through a cluster of historically-emergent biotechnologies such prenatal testing and embryonic stem cell research (among other social objects and events). These biotechnologies (and others) are products and effects of a certain "style of reasoning" (Hacking 1992, 1; 2002) that has brought into being new types of objects, individuated with the style, that had not previously been noticeable among the things that exist (Hacking 1992, 10–11). In my presentation, I argue that the "diagnostic style of reasoning" (as I call it) has brought into being a new type of object called "impairment" whose objective existence is authenticated by the style itself.

Dana Tulodziecki, Philosophy, U. of Missouri-Kansas City
The Epistemology of Scientific Practice
Session II.2A

In this paper, I want to suggest that there are aspects of scientific practice that make a central contribution to the epistemic standing of our scientific theories and hypotheses, such as methodological rules and principles, experimental procedures, and our engagement with scientific instruments. The purpose of this paper is to outline a meta-philosophical programme detailing what such a project would involve. Specifically, I will explain what is required in order to support the following four inter-related points: (i) that these different factors really do make epistemic, not just pragmatic, contributions to our theories, (ii) how it is that they make these contributions, (iii) that claims about the epistemic nature of these factors are, at least in principle, testable, and, lastly, (iv) that we can actually test for them by engaging in historical-empirical work. In this paper, I will focus specifically on scientific methodology (with the eventual aim of developing similar accounts for other aspects of scientific practice) and outline an account of our methodological principles according to which these principles are robust both epistemically and empirically, with a special emphasis on principles that abound in actual science. The main goal of this paper is to show how – given the imperfect epistemic predicament that comes with doing empirical science and the enormous variety of scientific practices, many of which change over time – we could ever provide, even in principle, an epistemic justification for any of these methodological strategies.

Jaipreet Virdi, IHPST, U. of Toronto
A Crisis of Identity and a Need for Medical Authority: Aurists and Aural Surgery in 19th Century London
Session II.2B

During the 1830s, as aural surgery—a branch of medicine focusing on ear diseases and deafness—became a "fashionable" trend amongst aristocratic households, British aurists fiercely competed with each other for positions, status, and patients. These exchanges played a pivotal role in shaping public perceptions of aural surgery, constructing it as a "profession of quacks" and undermining the authority of prominent and popular aurists such as William Wright (1773- 1860), John Harrison Curtis (1784-1852), and James Yearsley (1805-1869). This paper narrates how public opinions made it difficult for aurists to maintain their surgical and medical authority, by focusing on the case of the London-based aurist Alexander Turnbull (c.1794-1881) during the summer of 1839. In two separate instances, two of Turnbull's patients died following medical catheterization to relieve deafness, leading to an inquiry commenced by Thomas Wakley (1795-1862) and a jury of tradesmen. The public outcry over Turnbull's supposed negligence and incompetence was spread throughout London periodicals and discussed in The Lancet, drawing much attention to the shortcomings of aural surgery and criticizing the nature of aurists' authority as medical and surgical practitioners.

Daniel M. Weinstock, Philosophy, U. de Montréal
Catastrophic Risk Management and the Role of the Democratic State
Joint Session CPA-CSHPS

Amy Wuest, Philosophy, U. of Western Ontario
Formal Truth as a Means of Clarification in the Realism/Anti-Realism Debate

Session I.3B

The epistemic status of scientific theories, as either true or something else, has been hotly debated by realists and anti-realists since the fall of the received view. While this is not a new topic, and much time and effort has been spent trying to clarify this issue, there still seems to be a deep, lingering confusion at the heart of the debate concerning the status of scientific theories as either true or not. Both realists and anti-realists, in many cases, define truth simply as a correspondence to reality. However, I will suggest in this paper that this definition of truth has proven to be unhelpful in resolving this core problem for both realists and anti-realists. In order to clarify the conception of truth, I suggest taking a step backwards, to the 1930's, and looking at Tarski's The Concept of Truth in Formal Languages and Carnap's account of analyticity in the Logical Syntax of Language (LSL). Even though Carnap's project was meant to be purely syntactical in the LSL, when it is juxtaposed to Tarski's account of formal truth, it is clear that the two projects have much in common. The result of both projects is a clear, understandable conception of truth that could serve as a satisfying middle-ground between the realists and anti-realists.