Communiqué

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CANADIAN SOCIETY FOR THE HISTORY AND PHILOSOPHY OF SCIENCE

No. 75 Spring/Printemps 2010

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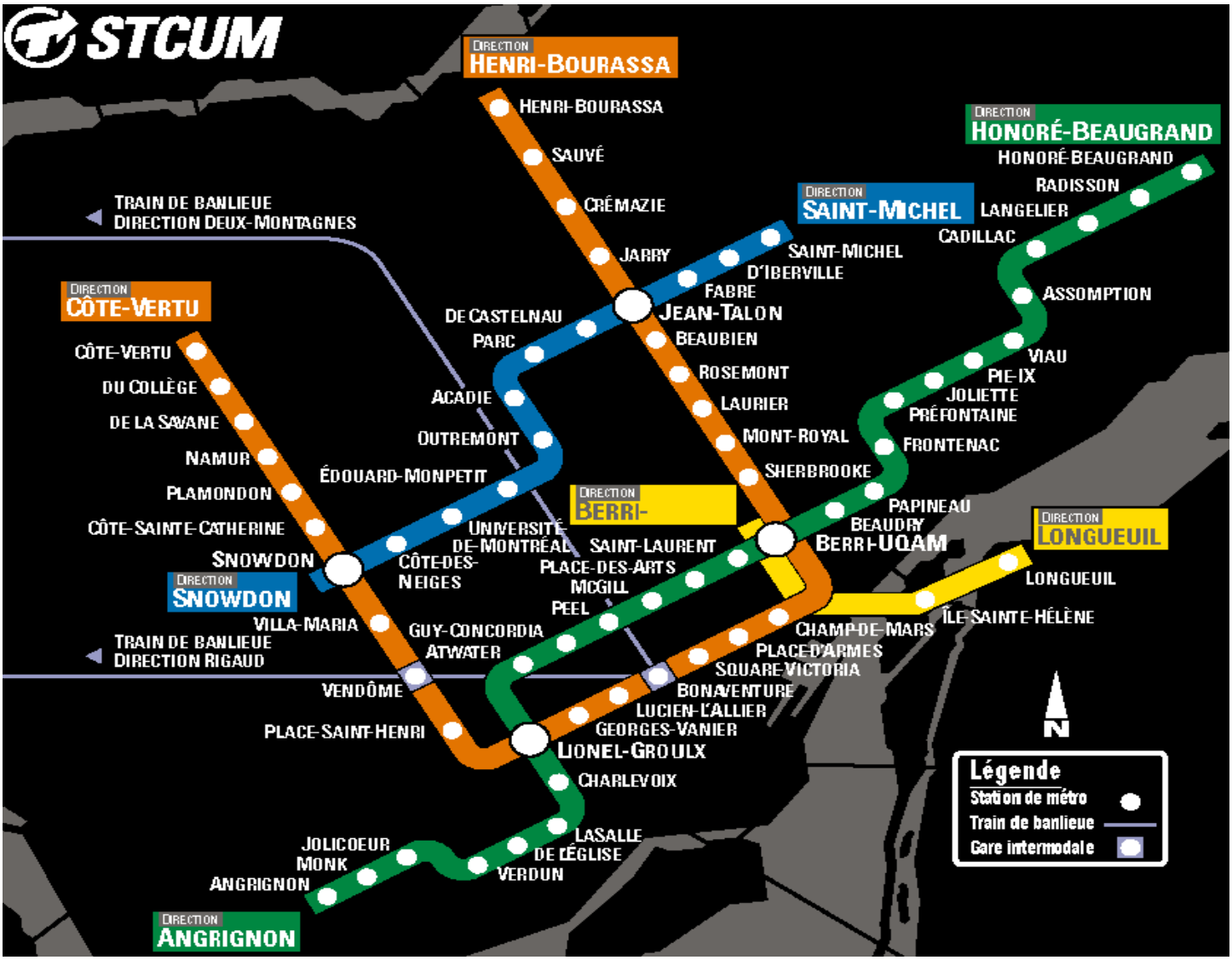

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Please direct submissions and inquiries to Aryn Martin or Sofie Lachapelle, preferably by email (details below). Please note that submissions can be sent in either official language. The editors are grateful to York University for assistance in printing and mailing costs, and to the University of Guelph for providing the necessary software.

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CSHPS/SCHPS 2010 MONTREAL:

ABSTRACTS

(organized alphabetically, session number cross referenced to master program)

Michal Arciszewski (marcisze@uoguelph.ca)
The Dauer Larva
Session G1
One of the most extensively studied model organisms in biology is the nematode worm Ceanhorabditis elegans which exhibits a specialised alternative developmental stage, the dauer larva, in response to environmental stress. This response which is highly adaptive for many species, is induced by what researcher call the dauer choice, an integration of environmental signals and developmental cues into a complex network of neural and genetic pathways. The relevant molecular parameters involved in the dauer choice have been experimentally dissected with such detail that, I argue, we have an illuminating reduction. What makes this a philosophically interesting reduction lies in two main features: we have a reduction of a highly complex phenomenon to one molecular model and, in doing so, have, for the first time, a significant reduction linking the central concepts employed in the distinct domains of evolutionary, developmental and behavioural biology. The reduction of the dauer choice can serve as a simple model for the reduction of a wide range of highly complex biological phenomena.

Joseph Berkovitz (joseph.berkovitz@utoronto.ca)
The world according to de Finetti: On the instrumental and verificationist foundations of de Finetti's subjective theory of probability
Session D2
Bruno de Finetti is one of the founding fathers of the subjective school of probability, where probabilities are interpreted as rational degrees of belief. De Finetti's interpretation of probability has been highly influential in science, and it has recently gained popularity in the interpretation of probabilities in quantum mechanics. De Finetti's work on the relation between the theorems of probability and rationality is among the corner stones of modern subjective probability theory. De Finetti maintained that rationality requires that a person's degrees of belief be coherent, and he argued that the theorems of probability theory could be derived from these coherence conditions. We argue that de Finetti held that the coherence conditions of degrees of belief in events depend on their verifiability. Accordingly, the familiar coherence conditions, as reflected in discussions of subjective probability, only apply to sets of degrees of belief in events that could in principle be jointly verified. While the most obvious explanation of de Finetti's verificationism is the influence of positivism, we argue that it could be justified by his instrumental concept of probability. We then explain the implications of this interpretation of de Finetti to a subjective interpretation of quantum probabilities.

Steven Bland (sbland@uwo.ca)
Carnap's Scientific Philosophy
Session D3
One of the most prevalent objections to Logical Positivism charges that its central philosophical theses must constitute either nonsense or freely chosen linguistic conventions. However, if verificationism and conventionalism are in fact linguistic conventions (this is, presumably, the more attractive horn of the dilemma), then Logical Positivism is a self-refuting position. Carnap, being the most highly regarded of the Positivists, has been attacked on exactly this count. In addition to canvassing what I see as being some of the inadequate responses on Carnap's behalf, I will argue that a careful study of Carnap's philosophy of logical syntax and his mature theory of scientific theories reveals what is wrong with the objection. This paper will show that verificationism and conventionalism, far from being pragmatically motivated conventions, are actually the result of Carnap's attempt to explain the essential features of our theoretical knowledge by providing a rational reconstruction of mathematical, philosophical, and scientific theories. Thus, while these theses may be non-empirical, they are indispensable to a scientific philosophy like Carnap's. Moreover, I claim that Carnap's methodology makes clear how it is that philosophy can be crucially informed by science without itself being an empirical discipline.

Ingo Brigandt (brigandt@ualberta.ca)
Beyond Armchair Intuitions and Experimental Philosophy: Lessons from Biological Concepts
Session E2
Philosophers aim at analyzing central philosophical concepts, e.g. 'knowledge', 'reference', or 'causation'. A widely used method to arrive at such accounts is the use of one's intuitions as to how the concept applies to various imagined situations. Recently, experimental philosophy emerged and claims to do away with the armchair analysis of philosophical concepts by using questionnaire data. However, I argue that both approaches are based on the erroneous assumption that we already possess an appropriate

concept of e.g. knowledge, and simply have to make it explicit by consulting an individual's intuitions or by surveying the intuitions of a population. Scientific concepts show why this assumption is wrong. At their introduction, biological notions such as the gene concept or the homology concept embody many misconceptions. In the process of clearing those up, concepts and even the basic definitions of biological terms change. Rather than explicating the current use of a concept (as many philosophers do for philosophical concepts), scientists aim at improving the concept. Based on examples I explain that biological concepts are introduced for a certain scientific purpose, and that the revision of concepts and their definitions can be justified relative to this purpose. In analogy, I suggest that philosophical terms are technical terms introduced for a (philosophical) purpose, and that any philosophical account (of knowledge, reference, causation) is to be justified not by how well it conforms to intuitions but by how well it serves this purpose. This provides a more fruitful philosophical methodology than armchair and experimental philosophy.

Kimberly Brumble (kcbrumbl@ucalgary.ca)
How to Diagnose a Metaphoric Model in the Field
Session H1
Cognitive linguist George Lakoff and philosopher of mind Mark Johnson argue in their work that there exist preconscious structures that they term "cognitive metaphors." Lakoff and Johnson assert that there is expanding evidence from neuroscience, neuropyschology and linguistics that attests to the existence of what they call preconscious visceral frameworks. These frameworks shape our thinking about the world and make much of, if not all of, thought metaphoric in a particular sense. The metaphoric quality of thought is particularly prevalent in, and useful for, analyzing overt forms of metaphor like scientific modeling. Before reducing, changing, or improving on scientific models, we ought to consider how and why cognitive metaphors are in operation and how they effect our models. If we do not take into account the particular ways models fail we may import those failures into future models. Following Lakoff and Johnson, and responding to the recent discussion in Philosophy of Biology of the Adaptive Landscape Model, I suggest that the value of particular metaphoric models in science can be productively evaluated by considering the presence of cognitive metaphors. Using the recent debate regarding the adaptive landscape model, I will argue that, before reducing or otherwise altering models, we ought to consider the cognitive metaphors at work within our present models.

Mario Bunge (info.philosophy@mcgill.ca)
Is quantum mechanics indeterministic?
Session B4

Nearly everyone believes that the quantum theory is indeterministic. The reason is that its state function is interpreted as a probability density. However, there are reasons to believe that this conclusion was hasty and simplistic. The aims of this paper are to argue for the following theses. First, the birth of statistical physics in the nineteenth century suggested that, although causal determinism à la Laplace is no longer tenable, a broader conception of determinism, as lawfulness plus non-creation ex nihilo, is defensible. Second, standard quantum mechanics has two aspects on the same footing: a causal aspect symbolized by the hamiltonian, and a stochastic aspect represented by the state function. For example, any model of a scattering experiment includes both the force that scatters the incident particles, and the state function that describes the resulting scattering. Third, Bohm's 1951 theory was not thoroughly causal because it retained the state function as basic or primitive. One moral of this story is that the world is objectively half-causal and half-random. Another moral is that the determinism problem must be distinguished from the question of realism. Even if causality were totally absent, as Hume thought, the external world would exist on its own: if it did not, it would be senseless to explore it.

Alison Butler (abutler@stfx.ca)
Finding a Science: The Transformation of Victorian Occultism
Session C1
This paper examines how scientific development affected the evolution of Victorian occultism by examining how occultists attempted to make their field more "scientific" in response to the threat of scientific naturalism, and how occultism's resulting refashioned form had more in common with the emerging sciences of the mind. This paper assesses the openness of these new sciences to this refashioned occultism and addresses attempts by occultists to ally their interests with psychology and psychoanalysis. This inquiry begins by exploring how scientific naturalism and a cultural emphasis upon materialism provoked a fundamental transformation of modern magic. This transformation involved an emphasis upon individual spiritual development and evolution of the self into a higher being, thus positioning Victorian magic as better suited to the sciences of the mind. This complimentary pairing of occultism with psychology was the result of magic's failure to align itself with naturalism, and of theoretical changes within occultism. Such changes involved how occultists

accounted for the existence of mind and consciousness. This paper also explores the role of psychologists in the intersection of their field with that of occultism. Did they seek out occult influences in opposition to naturalism, or in recognition of apparent similarities between the subject of study in both occultism and psychology?The findings will shed light on the nature of the relationship between science and magic. They will further the issue of how scientific advancement affects belief, by suggesting that rather than destroying belief systems, the march of science can provoke the refashioning of competing worldviews. In examining the role of occultism in the development of psychology, this research also demonstrates how occultism has left its mark on science.

Anne-Laurence Caudano (a.caudano@uwinnipeg.ca)
"Heaven has the shape of a cube or is stretched like a skin:"Cosmology and the Byzantine Church in the Comnenoi Period (1088-1185)
Session F4
While Aristotelian studies flourished after the reorganization of institutions of higher learning in Constantinople in the 11th century, the Byzantine State and Church were openly attempting to control philosophical knowledge and its orthodoxy. A century later, Manuel II Comnenos embarrassed himself in a debate to defend astrology, a discussion that he ultimately lost to the monk Michael Glycas. These two incidents are well known to Byzantinists. Lesser known to scholars, however, is the evidence pointing to a revival of Antiochene descriptions of the world. These cosmological theories, elaborated in the 5th and 6th centuries by Church Fathers of the School of Antioch, described the universe as a box at the bottom of which the earth rested flat. Sun, moon and stars circulated around a mountain situated at the northwest. By the time the Comnenoi sat on the imperial throne, such ideas, or so it seemed, had been long forgotten. These strange views about the world, exclusively based on Biblical quotations, survived in chains of commentary to Genesis, however. More interestingly, they also found their way into the work of a 12th-century author, Peter the Philosopher. Two texts remain from him: a letter addressed to Patriarch Luke Chrysoberges (1157-1169), which condemned the prelate for dabbing into astrology, and a (yet unpublished) astronomical treatise that explicitly defended the Antiochene cosmos. Although little known, Peter the Philosopher is in fact a remarkable representative of the colourful scientific discussions that characterized the reign of the Comnenoi emperors.

Anjan Chakravartty (anjan.chakravartty@utoronto.ca)
Positing Powers
Session H3
The most basic argument offered by philosophers of science (e.g. Bird 2007, Cartwright 1999, Chakravartty 2007, Ellis 2001) in favour of realism about powers (dispositions, capacities, tendencies, etc.) is that by positing their existence, we gain explanatory power. Why do types of objects and systems investigated by the sciences, sharing certain properties, figure in the same sorts of causal relations, behaving in highly similar ways in highly similar, counterfactual circumstances? What grounds the regularities described as laws of nature? Realist answers to these questions appeal to explanatory force: if there were powers, they would furnish explanations of these and related phenomena – explanations that would be lacking otherwise – and this is rendered as evidence for their existence. Conversely, the Humean tradition is typified by a rejection of such demands for explanation. I consider the ways in which powers have furnished candidate explanations that would otherwise be unavailable, and reflect on the question of how far demands for explanation should take the realist. On one hand, maintaining that explanatory power is an epistemic virtue is an important tenet of realism. On the other hand, there are clearly limits, lest the realist find herself explanatorily infallible and consequently worthy of Humean scorn, able to explain all natural phenomena at will, simply by positing the existence of a relevant disposition. This paper considers the delicate balancing act this suggests, and the resultant constraints that operate on realism about powers driven by the putative power of explanation in the context of scientific knowledge.

Christopher Chalmers (chris.chalmers@dal.ca)
Categorizing Evolution Explanations in Psychiatry
Session B1
Evolutionary explanations have been largely absent in the history of psychiatry, however there have been recent calls for an integration of evolutionary theory into psychiatric medicine (Baptista, Aldana, Angeles, & Beaulieu, 2008). Dominic Murphy has written extensively on the appropriate use of evolution in the psychiatric context (Murphy, 2000, 2004, 2005, 2006). He has distinguished three categories for evolutionary explanations of mental disorders. Murphy argues that there is a significant distinction between the first category of evolutionary explanations that involve the breakdown of an evolutionary function and the second and third categories of evolutionary explanations that involve currently adaptive traits or formerly adaptive traits operating in mismatched environments. If our explanations fit into the latter two categories then for these explanations nothing

in the mind is malfunctioning as the traits in question are operating "as natural selection intended" (Murphy and Stich, 2000). I argue that Murphy has mischaracterized the three categories of evolutionary explanation as mutually exclusive when in fact they are not. Some evolutionary explanations of mental disorders do not fit into any single category while other explanations are impoverished when we cannot integrate explanatory resources from more than one category. In order to develop complete evolutionary explanations for a number of mental disorders we need to allow for the possibility of this integration.

Mielle Chandler (miellechandler@gmail.com)
Rewriting Creation: Towards a Developmental Systems Critique of Biotechnical Property
Session G1
In its capacity as a legal and scientific construct, biotechnical property is conceptually grounded on 'man's' jurisdiction to act upon the lifeworld. While intellectual property law addresses jurisdiction through codifying the act (as an 'inventing' or 'making') as giving rise to a new creation, developmental systems theory suggests that the epigenetic contextual and surrounding elements are critical factors in the generation of new life. Privileging action as the approach that brings an actual plant or animal into instantiation from the given potentiality of genetic information serves to shore up both normative conceptions of creation as issuing from a cognitively controlled volitional propulsion—an act of will and intelligence—and, according to populist critics, the economic interests of biotechnical companies.This paper examines how the ownership of biotechnical property rests, conceptually, on the privileging of human cognition, action, and agency—and the corporate technological mobilization of this cognition, action, and agency—over other critical developmental systems. If we look further to Rheinberger's scholarship on the intersection of science and language the ontological question becomes: if genetic information is 'written' into existence by developmental systems without cognizing agents, then how is it that the corporate technological 'rewriting' of genetic codes confers the legal entitlement of ownership?

Mathieu Charbonneau (mathieu.cote-charbonnea@umontreal.ca)
When Ideas have Weight: Material Knowledge and the 1953 DNA Model
Session E2
In his Nobel lecture of 1954, the chemist Linus Pauling claimed that the configuration of complex molecular structures, especially organic ones, are best understood by building material models of them. A year before, Watson and Crick discovered the DNA structure by using such methods. Their discovery consisted in the construction of a metallic model representing the DNA molecule's basic structure and in measurements on that model to show that it complied with previous empirical findings concerning DNA (viz. Franklin's spectrographic readings, Chargraff's rule, etc.). I will argue that the 1953 model was no mere demonstration of the proper DNA structure, but that it should be considered as the first instantiation of the scientific knowledge that DNA structure was a double-helix, etc. Although considering material artefacts as knowledge seems counterintuitive, I will show that by using a functional account of knowledge some material models can and should be understood as knowledge simpliciter. Such models can be justified true beliefs, and the 1953 DNA model was.In his Thing Knowledge, Davis Baird argued that material models should be considered as genuine objective knowledge, that is, knowledge shared by the scientific community, rejecting the possibility that artefacts could be subjective knowledge. My account differs by showing that a material model can be understood as subjective knowledge, i.e. as a genuine belief of some individual scientist. This thesis will be illustrated by the subjective cognitive role of the materiality of Watson' and Crick's 1953 model.

Sheldon J. Chow (schow53@uwo.ca)
Alain Ducharme (aducharm@uwo.ca)
Keeping Darwin in Mind
Session B2
Darwin was explicit that evolution via natural selection explains all biological phenomena, including the complex and mysterious nature of the human mind. However, the application of his theory of natural selection to the mind is the most ill-received facet of Darwinism. Darwin received much opposition on this point in his own time, and the controversy is still alive today. We argue that human mind and cognition should not be excluded from an evolutionary analysis. A recent popular application of Darwin's ideas to human mind and cognition is the programme of Evolutionary Psychology, according to which the human mind is the product of a vast number of adaptations from our ancestral history. But Evolutionary Psychology has come under harsh criticism, mainly having to do with its accommodation and use of evolutionary concepts such as fitness and adaptation. Yet this is not an indication that Darwin was wrong. Our main thesis is that applying Darwinian evolution through natural selection to the human mind remains a tenable research programme. Focusing on Darwin's work we show that Darwin himself saw the potential of how his own ideas may bear on the human mind, and through careful exegesis

we tease out methodological pointers from Darwin to suggest the beginnings of a modest evolutionary psychology (sans capital letters) which avoids the downfalls of Evolutionary Psychology (with capital letters).

Samantha Copeland (sam.c@dal.ca)
Methodology in the History of Psychopharmacology: A Look at Clinical Expertise
Session B1
In the introduction to his collection of interviews, The Psychopharmacologists, David Healy suggests that the early science of psychopharmacology was not, "the kind of science that…proceeded by conjecturing and refuting or by classic experiments. It has tended to be an enterprise where people have stumbled on observations and built theories post hoc." This is partly due to the fact that new technologies in psychopharmacology—that is, new drugs or drugs with new indications--were introduced and became widespread before theory could account for or predict their effects. This paper proposes to look at some of Healy's interviews with scientists who played key roles in the development of the study and practice of psychopharmacology. More precisely, this paper will examine the scientific methods these psychopharmacologists employed. The discussion briefly engages the construction of psychopharmacology by industry, psychiatry and government, in so far as the pragmatic goals of research were made to supersede theoretical goals in the interests of marketing and making treatment available to psychiatric patients. I argue, however, that this does not sufficiently account for the attitudes of the scientists themselves toward their research. That is, the methods employed by the scientists of early psychopharmacology are also a reflection of the difficulty in drawing a line between research and practice in the human sciences. It is this aspect of the scientific method that will be my focus in this paper, and my analysis will suggest possibilities for a general theory of clinical practice as scientific method in medical research.

Ken Corbett (ken.corbett@dal.ca)
Constituting Time: Technology & Philosophical Attitudes Towards Time in Britain 1870-1900
Session A3
This research forms the basis of a chapter in my Master of Arts thesis entitled "Technologies of Time: Time Standardization and Response in Britain 1870-1900." It examines the influence of technological systems on philosophical notions of time. Nineteenth-century systems of communication and transportation such as the railway and telegraph increased the complexity of social interaction characterized by a higher degree of time awareness. In addition, these technologies presented the possibility of conceiving time in new ways that were manifested in philosophical debates on the nature of time. This project situates late-nineteenth-century philosophical notions of time alongside the heightened demands of coordinating one's own time with that of the community. It examines the discussion of time in such British periodicals as Mind and The Proceedings of the Aristotelian Society as well as other published works such as those of Shadworth H. Hodgson, Bernard Bosanquet, and Bertrand Russell. I argue that the philosophical problem of time in the late-nineteenth century is a manifestation of the new experience of time produced by interactions with industrial technologies. Evidence of this is present in the use of technology to express the experience of time and the conception of time as a form measurement in philosophical debates.

Michael Cournoyea (michael.cournoyea@utoronto.ca)
Symptomatic Assumptions: Examining the Foundations of Evolutionary Medicine
Session G2
Evolutionary medicine (EM) is an emerging field of medical studies that aims to transform our understanding of medical causation by reconceptualizing human physiology and behaviour in terms of evolutionary adaptations. The proponents of EM hope to provide medical educators, policy-makers, and researchers with a robust and comprehensive explanatory framework that acknowledges the importance of evolution to medicine. This paper takes a critical approach to the epistemological foundations of EM, critiquing the use/misuse of evolutionary thinking in modern medicine. EM has yet to address three of its tenuous assumptions: its adaptationist approach to human evolution, its faith that evolutionary explanations will prove useful to clinical practice, and its narrow biomedical view of medicine. These assumptions are implicit and unacknowledged in the EM literature but remain central to EM's programme. This project is the first to critically engage and challenge these assumptions in order to assess the integrity of EM's explanatory framework. With an expanding literature and growing number of active proponents, EM is well positioned to influence medical education, public health policy, and clinical research. Drawing on work in the philosophy of biology, health, and medicine, this project challenges the value of EM's growing influence.

Michael Cuffaro (mcuffaro@uwo.ca)
On Kant and Non-Euclidean Geometry
Session F3

Kant argued for the synthetic a priori status of geometry, but his views were generally taken to have been refuted by the development of non-Euclidean geometries. Recently, however, some philosophers have argued that, far from refuting them, the development of non-Euclidean geometry has only confirmed Kant's views, for since a demonstration of the consistency of non-Euclidean geometry depends on a demonstration of its equi-consistency with Euclidean geometry, one need only show that the axioms of Euclidean geometry have `intuitive content' in order to show that both Euclidean and non-Euclidean geometry are bodies of synthetic a priori truths. Michael Friedman has argued that this defence relies on a modern (polyadic) conception of logic that was foreign to Kant. According to Friedman, Kant held that geometrical reasoning (not merely geometrical axioms) is constructive in nature, but that this is simply wrong in light of modern analytical methods. Friedman commends Kant for comprehending the implications of the limitations of his own (monadic) logic. But as a consequence, Friedman argues, non-Euclidean geometries are logically impossible for Kant. Friedman's characterization of Kant's views on geometrical reasoning is correct. I argue, however, that Kant's views do not imply the logical impossibility of non-Euclidean geometries. I show that constructive axiomatizations of both Euclidean and non-Euclidean geometries exist, and that in spite of the abstract symbolization techniques involved, they do (unlike Hilbert-style axiomatizations) capture Kant's views on the essentially constructive nature of geometrical reasoning well.

Lucia Dacome (lucia.dacome@utoronto.ca)
Balancing acts: weighing perspiration in the long eighteenth century
Session C3
This paper reconstructs the historical fortune of an image that throughout the seventeenth and eighteenth centuries became a landmark of the medical doctrine of static medicine advanced by the physician Santorio Santorio (1561-1636). The image depicted a man weighing himself on a scale that measured changes in insensible perspiration, an imperceptible excretion of the skin that was considered to be of critical importance for the pursuit of health. Well into the eighteenth century, the image of the weight-watching man underwent a great success. It appeared in a variety of medical works, navigated across competing medical theories and different medical genres (such as the commentary, the aphorism and the experimental report) and survived harsh debates on competing models of the body (such as the mechanical versus the humoural). This paper will examine the success and the historical agency of this image, and accordingly reconstruct its role in the early modern medical world.

Richard Delisle (richard.delisle@uleth.ca)
Ernst Mayr's Philosophy of Science: Its Connections With Logical Empiricism and the Unity of Science Movement
Session G2
Ernst Mayr is a major figure of the neo-Darwinian movement in the twentieth century. His contribution to this movement largely centers on his development of a strong organismic biology which resists the reductionistic conception of a gene- and molecular-based evolutionary biology. Perhaps less well known, however, are Mayr's significant and sustained efforts to construct a genuine philosophy of science which would take into account the specificities of organismic biology. Unsurprisingly, this endeavor prompted him to build a case against the physicalistic epistemology of the logical postivists and others, which they hoped would serve as a universally valid basis for philosophy of science. Mayr's goal consisted in finding the proper place for biology within science, a quest which he especially pursued in relation to the physical sciences. It is argued in this paper that Mayr's philosophy of science should be taken seriously, since it provides a way of envisioning what a scientific revolution deprived of its physicalistic components might look like. In contradistinction to Mayr's rhetoric and self-understanding of the situation, however, it will be further argued that his philosophy of science achieves its aims only because Mayr himself shared a number of epistemological commitments originally conceived within the context of a physicalistic scientific revolution: a plea againts metaphysics, a theory-based approach to science, a reductionistic and corpuscular philosophy, and the search for an unified knowledge. Mayr's connections to such epistemological commitments are made more obvious when a rapprochement is established between his view of science and the one promoted by the logical empiricists. It seems that these four commitments are neutral enough, epistemologically speaking, to encompass both the physicochemical and the biological realms. Although focused on epistemolgical issues, this paper is an explicit contribution to the encounter of history and philosophy of science.

Eric Desjardins (ericdsjrdns@gmail.com)
Ecological Management and Historicity
Session E1
In the 1960s-1970s, biology took an "historical turn" and increasingly recognizing the importance of historical contingencies in explaining the state and distribution of life on earth. This paper will examine the implications of

the historical turn in community ecology and ecological management. Projects of ecological conservation and restoration often rest on the assumptions that nature, if left alone, will progress towards a stable equilibrium (an harmonious, self-sustained assemblage of species), and that we can control nature and return it to some "pristine" state. I will argue that these ideals fail to recognize "historicity," which entails essentially the existence a relationship of causal dependence between the "assembly history" of a community (i.e., the order in which particular species arrive in a community) and the outcome (i.e., structure and composition of community). It follows from "historicity" that a given set of conditions may very well lead to multiple outcomes (some less predictable than others), and that reversing the trajectory of ecosystems may be virtually impossible. So, embracing "historicity" invites us to revise certain expectations and objectives in ecological management, and it stresses the need to get a better understanding of the relationship of causal dependence for different types of ecological communities. Acquiring this body of knowledge will take some time, but it is in line with the increasingly popular approach called "adaptive management," which consists of treating policies as hypotheses and management actions as experiments.

Benoît Dubreuil (dubreuil.benoit@uqam.ca)
Retracer les mécanismes de l'évolution de l'esprit
Session B2
La plupart des philosophes s'étant intéressé à l'évolution de l'esprit l'ont fait dans le cadre du paradigme de la psychologie évolutionniste. Cette présentation s'intéresse à un autre paradigme n'ayant pas à ce jour retenu l'attention des philosophes : l'archéologie cognitive évolutionnaire (ACE). L'ACE met en relation l'archéologie et les neurosciences cognitives afin de retracer l'évolution cognitive chez les homininés. Je montrerai que la méthodologie de l'ACE présente des similitudes avec celle de la psychologie comparée, mais également des difficultés propres. Les comportements des populations disparues ne peuvent pas être étudiés dans un environnement contrôlé, mais doivent être reconstruits sur la base d'indices fragmentaires. Contrairement à un point de vue influent, je soutiendrai que la production d'hypothèses valables en ACE ne dépend pas seulement de la capacité d'établir des liens explicites et plausibles entre le comportement des homininés et des fonctions cognitives bien établies (Botha 2008, 2009; Wynn et Coolidge 2009). Je soutiendrai plutôt que, pour retracer les changements cognitifs les plus plausibles, l'ACE doit nécessairement utiliser 1) des méthodes lui permettant d'identifier les variables non cognitives susceptibles d'expliquer les variations de comportement et 2) des méthodes permettant de retracer des changements cognitifs et neuronaux chez des espèces disparues, notamment, la paléoneurologie et les neurosciences cognitives comparées et du développement. En somme, je montrerai que le succès de l'ACE dépend de sa capacité à intégrer des approches permettant de retracer les transformations de mécanismes causaux à plusieurs niveaux (neuronal, cognitif, social et écologique).

Yiftach Fehige (yiftach.fehige@utoronto.ca)
Thought experiments, Computer and Blended Simulations
Session H1
Scientific thought experiments haven been subject of serious and appropriately widespread systematic scrutiny since James R. Brown proposed his highly controversial Platonic account at the end of the 1980. Independently of this recent development in philosophy of science a growing number of philosophers show a strong interest in the nature and use of simulation and models in science. Most recently, first attempts have been made to look at the relationship between thought experiments and simulation or thought experiments and models. As for the relationship between thought experiments and simulation, the question has been raised if thought experiments and computer simulations are just aiming for the same end by different means. Thought experiments and computer simulations seem to share many features both in their underlying logical structure and in their epistemology, and these features do not appear of secondary role in the purported cognitive effectiveness. Assuming that some thought experiments in science and the humanities have cognitive efficacy, I would like to argue for the view that thought experimenting and computer simulations aim not for the same end by different means and require different criteria to assess their quality. However, I will entertain the idea that the cognitive efficacy of thought experiments can be accounted for in terms of blended simulation, which blends in nicely with a naturalistic account of intuition. A thought experiment is cognitively efficacious iff it plays an indispensable role in theory revision, which is derivative to the indispensable role intuitions play in knowledge acquisition. While my contribution has the status of a general remark about the relation of thought experiments and computer simulations in terms of their principal purpose, I use Einstein's clock-in-the-box thought experiment to illustrate my point. The central line of my argument that supports the difference between thought experiments and computer simulations runs as follows: (1) Thought experiments aim for the exploration of intuitions. (2) The aim of computer simulations is to assess whether or not a model is consistent with observation by implementing the model to explore the behavior of the model and its predictions. (3) To explore intuitions is not a matter of assessing the consistency of

a model with observation. (4) Ergo, thought experiments and computer simulations do not share the same end by different means. One of the main problems in accounting for the cognitive efficacy of thought experimenting is the problem of informativeness: assuming that theory tells us something about the real world and that thought experimenting can support theory, the question arises how come thought experiments can exercise that cognitive power if they remain unexecuted in the real world. Accounting for thought experimenting in terms of intuitions allows for the following naturalistic explanation: intuitions as mental propositional attitudes manifest as a result of causal relations with the natural and social environment. One of the problems of this naturalistic account is the identification of the cognitive mechanisms that would help to illuminate what it means that the natural and social environment shape intuitions. I propose that the notion of blended simulation might help to further the understanding with regard to this aspect of thought experimenting. Recent developments in cognitive science support the view that human cognition is more than the manipulation of abstract symbols on the basis of their syntactic forms according to clearly formulated rules. Cognition also reflects modality specific simulations of embodied experience. The respresentation of knowledge typically involves unconscious simulation or blended simulation of associated experiences within the corresponding modes of human embodiment (visual, tactile, auditory, kinesthetic). Modal theories of knowledge of cognition may help to understand how blended simulations facilitate the manifestation of intuitions that guide the thought experimenter. The contemplation of thought experimenting helps to draw particular attention to the importance of imagery that serves to elicit intuitions. Thought experiments are very often the only way to elicit intuitions that guide a scientific debate. They make intuitions explicit and enable a critical examination of them. Intuitions are not only fiction but tell us about the real world as they emerge by means of blended simulation. In this sense thought experiments do not substitute real world experiments or computer simulations in order to evaluate the consistency of models, theories, etc. and observation. Matters of consistency with observation are not of primary concern when it comes to thought experiments. Einstein's clock-in-the-box thought experiment, for example, does not try to assess consistency with observation but aims for a reinforcement of the intuition that conjugate variables of physical objects have a determined value. Einstein's thought experiment is powerful because the underlying intuition is powerful. Powerful enough to have him forgetting his own formula of gravitational redshift. It takes the intuitive distance that was present in Bohr to interrogate the thought experiment's guiding intuitions in order to detect a contradiction with the gravitational red-shift formula. In consequence, thought experiments and computer simulations require different criteria to assess whether or not they are of good quality. Einstein's clock-in-the-box thought experiments suffers from underdetermination in terms of relevant background assumptions and leads therefore intuition astray. Observation does not play any role in Bohr's successful version of the same thought experiment which refutes Einstein's.

Andrew Fenton (andrew.fenton@gmail.com)
Chimpanzee knowledge and some implications for analytic naturalized epistemology
Session B2
Contemporary primatology and animal cognitive research continue to shed light on the capacities of chimpanzees to actively engage with both objects and conspecifics in natural and nearly natural environments. Active cognitive engagement implicates both learning and increasing goal-directedness that reflects the past experience of the animals in question. I propose to critically examine the epistemological implications of seeing chimpanzees as epistemic subjects who engage in recognizable epistemic activities (e.g., evidence gathering). To accomplish this, I will first briefly examine why certain studies of chimpanzee behavior should persuade us that these animals are usefully regarded as epistemic subjects who engage in recognizable epistemic activities. This, I will then argue, ought to have implications for analytic, particularly naturalized epistemology, whether highlighting its assumptions about knowledge production, epistemic activity or the nature of epistemic subjects. To illustrate, I will seek out a possible role for an individualist epistemology in understanding the knowledge of chimpanzees, perhaps providing an application of individualism that escapes recent attacks from those who see social epistemology as a more accurate and normatively tractable framework for theorizing about human knowledge or human epistemic activities. I contend that exploring more biologically plausible examples of individualist epistemic activities might shed light on what it means to be a human social knower. This nicely flips 'Cartesian elements' of contemporary analytic epistemology on its head, requiring us to re-see at least some animals as epistemic subjects, and their epistemic activities as integral to understanding human epistemic subjectivity and activities.

Nicolas Fillion (nicolas.fillion@gmail.com)
Robert Moir (rob.h.c.moir@gmail.com)
Modeling and Explanation: Some Lessons from Modern Error Theory

Session H2

Philosophical theories of scientific explanation have had notorious difficulty accounting for the extensive success of idealization in science. Recent works have shown the need for a more mathematically informed model of explanation, viz. a model that accounts for the prevalence of complementary descriptions, is able to handle both single events and universal phenomena, and involves counterfactual robustness. We argue that two deep and powerful concepts, namely backward error analysis and conditioning, provide a precise general framework into which these features may naturally be incorporated. Together these concepts provide a method to effectively analyze classes of models (as opposed to single models), an element we consider essential for the refinement of current models of explanation. Perhaps surprisingly, these concepts, which originated in the pioneering mathematical works of James Wilkinson, have not yet been discussed in the philosophy of science literature. We will provide a number of examples that will illustrate their wide-ranging application to mathematically posed physical problems. This will reveal that a key aspect to mathematical modeling, as prescribed by this method, is to exactly solve nearby problems. Since this framework enables nearness of a problem to be measured objectively (i.e., independently of our knowledge of particular methods to solve given problems), it provides epistemological grounds for a criterion to determine when an idealized model is just as good as the one originally specified. Finally, we conclude by indicating the way in which an understanding of this method contributes to the aforementioned research programme to refine the concept of explanation.

Curtis Forbes (curtis.forbes@utoronto.ca)

Reinstating Chakravartty's Stalemate

Session H3

In The Empirical Stance (2002) van Fraassen outlines and defends a meta-epistemological account of rationality that deems someone's epistemic practices "rational" so long as they are not a) self-defeating by their own lights or b) logically inconsistent. As emphasized by Chakravartty in A Metaphysics for Scientific Realism (2007), this view of rationality actually prevents van Fraassen from maintaining any "radical" critique of scientific realism, for the realist's practice of engaging in metaphysical speculation is neither clearly self-defeating nor logically inconsistent. In short, metaphysical speculation and anti-metaphysical empiricism must be deemed equally rational. An important corollary of this account of the scientific realism debate is that choosing between a realist interpretation of science and an anti-realist one becomes largely a function of our choice of values, entirely independent of any considerations of "rationality." Thus, various scientific realisms and anti-realisms can be equally rationally tenable, all at the same time. In this sense the scientific realism debate has been rendered moot, a state of affairs I call "Chakravartty's Stalemate." In Scientific Representation: Paradoxes of Perspective (2008), van Fraassen claims that scientific realism is actually untenable, given the scientific adequacy of an empiricist version of quantum mechanics. This makes it appear as if Chakravartty's Stalemate has been broken, but I argue it has not, i.e. that several mature scientific realisms remain fully tenable. While van Fraassen's (2008) characterization of scientific realism is more elaborate than his previous characterizations, I argue that it nevertheless fails to adequately characterize many contemporary brands of scientific realism.

Doreen Fraser (dlfraser@uwaterloo.ca)

Emergence in Statistical Mechanics and Quantum Field Theory

Session D1

Bob Batterman has argued that thermodynamic properties furnish a genuine example of emergent properties (see, e.g., The Devil in the Details). This case study has the nice feature that the relationship between the theories in question—thermodynamics (TD) and statistical mechanics (SM)—is known and can be clearly expressed in mathematical terms. Renormalization group (RG) methods are the mathematical formalism that is employed for this purpose; in general, this formalism is well-suited to the task of discerning cases of emergence and reduction because it relates theories at different scales. RG methods have also found applications in other areas of physics, most prominently in particle physics (i.e., quantum field theory). In the context of quantum field theory (QFT), the higher level theory (i.e., analogue of TD) is an "effective" QFT incorporating short distance cutoffs that, to a very good approximation, is empirically adequate at some low energy scale; the lower level theory (i.e., analogue of SM) is a continuum QFT without short-distance cutoffs that applies to arbitrarily high energy scales. Physicists and philosophers have claimed that effective QFTs describe emergent properties. This raises the question of whether an analogue of Batterman's argument for emergence can be run for QFT. I argue that the QFT analogue of Batterman's argument does not go through because both the explanatory goals and the idealizations invoked differ from the TD-SM case. This case study is of general interest for the study of emergence and reduction because it sheds light on the issue of whether the formal relationship between theories is sufficient to determine if a given case is one of emergence or reduction or whether the interpretation of the formal relationship is also required.

A. Gwyndaf Garbutt (gwyndaf.garbutt@utoronto.ca)
The Authority of Evidence and the Evidence of Authority: Exploring the construction of natural knowledge in the De animalibus of Albertus Magnus
Session D4
Medieval natural philosophy is often seen as dominated by the authority of ancient philosophers and disconnected from direct observation of the natural world. Some historians even suggest that Medieval scholars were interested exclusively in interpreting the knowledge of previous authorities and uninterested in connecting that knowledge to the physical world. My paper will show that, at least in the case of Albertus Magnus (~1200-1280 C.E.), this view is wholly inaccurate. Albertus Magnus's De animalibus (~1246 C.E.) is one of his greatest works on natural philosophy, compiling and assessing information from a wide variety of sources about the physiology and behavior of animals. Peter Harrison has argued that Albertus's inclusion of 'fantastic' animals in this work is evidence that he was more concerned with compiling information taken from ancient texts than representing physical reality. Such a reading drastically misrepresents Albertus's project. A close examination of De animalibus reveals instead a careful evaluation of evidence associated with each animal via observation, reason, testimony, and experimentation, all employed to determine the reality of claims concerning animals in distant lands. These 'fantastic' animals are not symptoms of a natural investigation dominated by ancient thought, but products of a careful evaluation of evidence and an acknowledgement of the limits of Albertus's own experience. Such a reading of De animalibus leads us to a more complete understanding of the creation of natural knowledge in the works of Albertus Magnus and provides the first step toward a reevaluation of the methods of Scholastic natural philosophy.

Yvon Gauthier (yvon.gauthier@umontreal.ca)
L'appareil analytique et ses modèles
Session A2
L'idée d'associer le concept d'appareil analytique ou ensemble des structures logiques et mathématiques aux modèles d'une théorie physique est nouvelle dans la littérature de la philosophie de la physique. Les historiens comme Corry, Jammer ou Mehra et Rechenberg ont bien identifié la source chez Hilbert de l'appareil analytique que certains appellent « machinerie analytique », mais on n'a pas encore fait l'hypothèse d'une relation (syntaxique et sémantique) entre appareil analytique et modèles chez les historiens et les philosophes de la physique. Je veux montrer que la relation des modèles d'une théorie physique à son appareil analytique est première par rapport à la relation qu'entretiennent la théorie physique et son appareil expérimental ou encore le domaine empirique de la théorie. Dans cette voie, je veux contraster l'approche de van Fraassen d'un perspectivisme structuraliste avec le point de vue constructiviste logico-mathématique pour radicaliser la thèse de la surdétermination théorique de la réalité physique. T. Ryckman dans son important ouvrage de 2005 The Reign of Relativity a couvert la période 1915-1925 de la philosophie de la physique, mais bien qu'il défende un point de vue (transcendantal) proche du constructivisme logicomathématique, il s'intéresse surtout à la théorie de la gravitation de la relativité générale et n'aborde pas la question de l'appareil analytique chez Hilbert et von Neumann en mécanique quantique.

Sebastián Gil-Riaño (sebastian.gil.riano@utoronto.ca)
Culture as an ontology for international development: Unesco and Anthropology in the 1950s
Session D4
What kind of ontology grounds contemporary discourses of international development? In the aftermath of the Second World War, the United Nations Education and Scientific and Cultural Organization (Unesco) sought to undermine scientific racism by appealing to scientific evidence for racial equality, and by citing examples of racial harmony around the world. For Unesco, scientific racism and its emphasis on fixed biological differences was an ontology that posed a threat to the Universalist values of the UN and its cosmopolitan hope for a world community. Consequently, Unesco launched a campaign that sought to undermine the scientific validity of the concept of race while promoting the concept of culture as the best way to understand human differnces. In my paper, I examine the concept of culture and its application during the first fifteen years of Unesco's existence. I do so by focusing on the writings of three anthropologists involved in Unesco's activities during the 1950s: Alfred Métraux, Margaret Mead, and Claude Levi-Strauss. I argue that Unesco's initial repudiation of race paved the way for the elaboration of a developmental discourse, which positioned the culture of the industrialised West as the pinnacle of human achievement and as a model for the economic development of other societies. By describing human differences as cultural, Unesco's social scientists adopted the view that human differences are malleable. This malleability, in turn, provided the justification for economic development programmes where social scientists, and in particular anthropologists, saw themselves as the experts best suited to guiding local cultures through the perils of modernisation.

Ari Gross (ari.gross@utoronto.ca)
Pictures and Pedagogy: the role of diagram in Feynman's early lectures, 1949-1950
Session A2
Over the past decade historians and philosophers of science have developed an interest in Feynman diagrams, the small, yet immensely powerful visual representations of subatomic events developed by Richard Feynman in the 1940s. However, while historical works like David Kaiser's Drawing Theories Apart and Adrian Wüthrich's Feynman's Struggle and Dyson's Surprise Revelation have told how Feynman diagrams were developed and propagated, there has been little focus on how Feynman himself used (and conceived of) his diagrams, especially shortly after their invention. Furthermore, the focus on "Feynman diagrams" at the expense of the numerous other types of diagrams used by Feynman has narrowed debates over the role of diagrams in scientific thought and the nature of visual reasoning. This paper aims to give a substantive account of how Feynman used diagrams in the first lectures in which he was tasked with explaining his new approach to quantum electrodynamics. A critical examination of unpublished course notes to lectures given in 1949/50 shall demonstrate how Feynman used a spectrum of diagrams in a variety of ways: heuristically, computationally, and as an aid to understanding the mathematical details of his approach. We shall see how Feynman modified each individual diagram to suit particular needs within the context of the lecture, yet how a general visual style allowed him to move between morphologically distinct diagrams with ease. Furthermore, key biographical themes such as the evolving role of diagrams in Feynman's work and the changing types of diagrams drawn throughout his career will be discussed.

Kristen A. Hardy (khardy@yorku.ca)
Civilizing Knowledge: Victorian Science in The Calcutta Review
Session C1
The broad range of strategies of inquiry and modes of knowledge production which Western societies have come to group under the rubric of 'science' encompasses multiple sites for investigating the ideological and practical contingencies of metropole and colony. As historian and social scientist Kapil Raj points out, changing patterns of education and governance within both Britain and its colonies led to a situation in which "science became organized so as to serve as the grammar of an increasingly globalized industrial society, as its ideology, its set of rules, and, indeed, its working metaphysics" (Raj, Relocating Modern Science, 139).The Victorian era, which encompassed a dynamic period of relations between Britain and its most-prized colonial possession, India, witnessed an unprecedented proliferation of scientific knowledges, practices, institutions, and discourses, diverse in their objects and methods. Whether undertaken in the metropolitan centre or the colonial periphery, research and theorizing were carried out in a sociocultural context undergirded by Britain's status as a large and growing colonial power. Science was an important component of the colonial agenda and the imperial performance of power, serving as a vehicle (with varying degrees of success) for the assertion and legitimization of British authority in the eyes of Indians, Britons themselves, their European rivals, and other colonized populations throughout the world.

Spencer Phillips Hey (heyspencer@gmail.com)
Robustness and Group Selection
Session E3
Richard Levins (1966) sketches a provocative account for the role of robustness in biological modeling. This paper builds on his and later work (Griesemer and Wade 1988, Levins 1993, D'Arms et al. 1997, Wimsatt 2007) by drawing a key distinction between three different conceptions of robustness: (1) Stability under changes in initial conditions; (2) stability under changes in parameters or equations; and (3) stability across a variety of independent modeling assumptions. This three-fold distinction is then used to analyze the group selection controversy from the history and philosophy of biology, arguing that the inefficacy of group selection was a robust result under the first two, mathematical conceptions of robustness, but not the third---a heuristic conception. The different conditions needed to establish robustness under each conception, and avoid the dangers of pseudo-robustness, therefore requires that when robustness is invoked, the relevant conception and degree both need to be specified, accompanied by an argument or proof for why the result is robust in the appropriate sense.

Michelle Hoffman (michelle.hoffman@utoronto.ca)
Science for babes and sucklings: the rise of general science in the secondary schools of Quebec and Ontario
Session A4
The early decades of the twentieth century saw a marked change in the way science was taught in high schools across much of the United States and Canada. This change, which targeted the lower grades of high schools, involved a gradual move away from a discipline-based approach (by means of separate courses in subjects like chemistry, physics, botany, and zoology) to a broad-based thematic course in general science. The general science movement was initiated in Chicago around 1903 amid concern about declining

enrolments in science courses and gradually made inroads into Canadian education. As John Rudolph ("Turning science to account," 2005) has shown, the general science course was founded on carefully articulated claims about the universal applicability of scientific reasoning and was fuelled, furthermore, by a technological enthusiasm that manifested itself in a proliferation of applications to machinery and engineering. This paper examines how the pedagogical ethos of general science was filtered and rearticulated by educationists in Quebec and Ontario, where demographic pressures were prompting a new outlook on the purposes and goals of a secondary school education that culminated in significant changes to the science curriculum in the late 1930s.

Laurent Jodoin (laurentjodoin@hotmail.com)
Mesure quantique et entropie / Quantum measurement and entropy
Session B4
L'entropie thermodynamique implique une compatibilité macroscopique entre différents états microscopiques (Tolman 1938). L'entropie quantique est liée au couplage avec l'environnement, c'est-à-dire l'appareil de mesure (Jancel 1963). Mais la réponse apportée à l'interaction système-appareil de mesure par la thermodynamique, d'une part, et par la mécanique quantique, d'autre part, est différente : l'une prédit une diminution et l'autre une augmentation de l'entropie. Ainsi, l'identification de l'entropie thermodynamique et de l'entropie quantique selon l'interprétation de von Neumann est mal fondée, voire erronée (Shenker 1999). En effet, cette identification implique la possibilité (absurde) d'une machine à mouvement perpétuel. Or, la conception de l'entropie quantique selon l'interprétation de von Neumann implique la matrice densité r qui représente « toute l'information disponible sur le système » – et cette information ne peut être « ignorée ». Il est soutenu ici que cela pose des problèmes à l'interprétation subjective de la théorie de la mesure en mécanique quantique.

Jeff Kochan (jwkochan@gmail.com)
Magicians, Miners, and Magnetical Experiments: Zilsel and Henry on William Gilbert
Session C2
Edgar Zilsel famously argued that modern science emerged as the Renaissance prejudice against manual labour subsided and educated scholars began to appropriate the practical techniques of miners, metallurgists, and mechanics. One hero in Zilsel's story is the late 16th-century savant William Gilbert, known for his magnetical studies. According to Zilsel, Gilbert acquired his experimental expertise from labourers and craftsmen. John Henry has recently disputed this claim, arguing that Gilbert instead acquired his expertise from the elite tradition of natural magic, where experimental techniques had long been used in alchemical investigations. Henry supports this claim by citing Gilbert's animism, that is, his belief that lodestones were living beings. Zilsel had rejected Gilbert's animism as an elitist frivolity irrelevant to his experimental practice. Henry counters that Gilbert's animism fit hand in glove with the experimental culture of natural magic. According to Henry, Gilbert predates the radical, early-modern divergence of empiricism and animism.I this paper, I argue that Zilsel and Henry are both right, and both wrong. Zilsel is right that Gilbert acquired his experimental techniques from mechanics, and Henry that he acquired them from magicians. Probably he got them from both. On the other hand, both historians are wrong in supposing that Gilbert's animism must have come from elite magicians. There is ample historical evidence suggesting that animism was also common among the Renaissance labouring classes. Furthermore, animism is not magic. If natural magicians were animists, then they were so independently of their elite education not because of it.

Brandon Konoval (drkonoval@telus.net)
"Je me supposerai dans le lycée d'Athènes": Rousseau, Aristotle and the Discourses of Nature
Session F4
In the Discours sur l'inégalité (1755), Rousseau presents himself as declaiming in "the Lyceum of Athens": why should Rousseau's arguments be 'heard' in such precincts? Rousseau's response to the question of the Dijon Academy is fundamentally conceived as a response to Aristotle's philosophy of nature in general and to the Politics in particular. The emergence of complex social arrangements per accidens, the incommensurability between early and more developed forms of society, the mendacious character of those who initiate the establishment of civil order, the moral depravity and psychological distortion attendant on members of civil society, and the perverse translation of slight differences in natural human potential to vast differences in social and political power are a point-by-point refutation of central tenets of an Aristotelian correlation between 'natural' and civil order. On this reading, what has been held to be 'natural'—as manifest in modern civil society or its citizens—is but the mask of artifice and the mark of social and moral decay. The challenge Rousseau thereby presents to the discourse of nature and society is directly addressed in Kant's Conjectural Beginning of Human History (1786) and implicitly in his Idea for a Universal History with a Cosmopolitan Intent (1784), where a Newtonian concept of a balance of attractive and repulsive social forces seeks

to restore the prospect of a 'natural' social progress and a constructive moral vocation for humanity.

Alex Koo (alex.koo@utoronto.ca)
Mathematical Explanation in Science: Arguments for Mathematical Realism
Session A1
The use of mathematics in scientific explanation has motivated a new Enhanced Indispensability Argument (EIA) for mathematical realism. Proponents of the EIA depend on the claim that mathematical objects are indispensable in scientific explanations, and hence inference to the best explanation tells us that we should be mathematical realists. A new criticism has surfaced which argues that the EIA relies on an unjustified and naïve view of the actual role of mathematics in scientific explanation. This criticism, which I will call the Indexing Argument, claims that the role mathematics plays in scientific explanations is merely to index physical facts. Even though mathematics may be an indispensable feature of scientific explanations, mathematical objects do not play any real explanatory role and thus the conclusion of mathematical realism cannot be had. This paper will argue that the indexing argument is fundamentally flawed as it undermines itself; it too relies on an unjustified and naïve view of explanation. What results from this undermining is a deadlock between the mathematical realist and nominalist camps that, I claim, is unlikely to be resolved in favour of the nominalist. Even worse, I will show that there is good reason to reject the indexing argument in principle as it depends on the assumption that mathematics maps on to the physical world; an assumption that we have good reason to believe to be false.

Sarah Kriger (sarah.kriger@utoronto.ca)
The Illusion of Intelligence: Technology, Conjuring, and "Psycho"logy in Nineteenth-Century London
Session C1
Just as the deftest sleight-of-hand fails to garner applause if its presentation falls flat, the most ingenious mechanical conjuring apparatus does not astound its observers through the operation of its mechanism alone. A good stage illusion must not only physically resemble the object or phenomenon it is meant to represent, it must also take advantage of social and theatrical conventions both to direct spectators' attention to its salient features and to convince audiences to recognize it as representing the desired entity. One particularly illuminating example of such a technology is Psycho, a purported automaton exhibited by the illusionist J. N. Maskelyne at the Egyptian Hall, London beginning in 1875. For over four thousand consecutive performances, Psycho played cards with audience volunteers, and, to this day, no one can say with complete certainty how it did so. Straddling the line between legitimate technological ingenuity and deliberate deception, Psycho was promoted as advanced technology—a machine that could reason like a human being—but displayed as a magic trick. Without its mechanism, the automaton would not have functioned—but the mechanism on its own would not have achieved the success it did. The various contexts in which Psycho's creators both purposely and serendipitously placed it, including those of the scientific community and the contemporary presentation of the human body onstage, helped it to fix itself in Victorian minds as a thinking machine, and modern historians of technology can fully analyze Psycho only by examining its place within those contexts.

James Justus (jrjustus@gmail.com)
Biological Information as Game-Theoretic Information
Session E2
Accounts of biological information flounder on its two putatively distinguishing features: arbitrariness and intentionality. Shannon information's independence from physical signaling media resembles the type of arbitrariness needed, but it fails to capture the required intentionality. Teleosemantics attempts to capture this intentionality by claiming genes possess functions selected for in their evolutionary past which, like functions of designed linguistic objects, establish genes semantically code for phenotypes. This view has nontrivial philosophical costs: a substantive function concept and permissive semantics. Deflationists legitimately question whether such function reification obscures more than biological information illuminates, and pragmatists balk at accepting a semantics without the clarity or fruitfulness of, say, Tarski's work. Information concepts found within sender-receiver signaling games constitute relatively unexplored explicative candidates. Genes are represented as sending signals through transcription, translation, and other processes to phenotypes, the receivers. The evolutionary problem is reaching a stable coordination system that (probabilistically) maximizes sender-receiver payoffs (fitness here) given selective forces impinging on phenotypes. Genes' selective history influences, but does not determine, what signals are sent (hence these are iterated games), and fitness payoffs are functions of environment-phenotype accord. I argue this account offers precise, plausible characterizations of arbitrariness and intentionality. The multiple stable equilibria signaling games typically have provide the former. In this sense signal content is arbitrary, much like codons are said to be. The

appropriately impoverished signal meanings generated in such games provide the latter. If genes have meaning, it is presumably rudimentary compared with natural language, and signaling game meaning displays exactly this.

Lucien Lamoureux (llamour@uwo.ca)
Van Fraassen's Reconstruction of Scientific Activity
Session H3
If someone were to ask van Fraassen "What is Science?" he would construe the question as "asking what is the point, the telos, of that activity" and reply "saving the phenomena" or, more precisely, "Science aims to give us theories that are empirically adequate; and the acceptance of a theory involves as belief only that it is empirically adequate". This answer – constructive empiricism – is puzzling. Although it uses the term 'belief', it "is not a doctrine about epistemology, but about the aim of science". What, exactly, is a "doctrine of aim" as a philosophical theory? And why should the question "What is Science?" elicit "saving the phenomena" as its answer? These fundamental issues come to the fore with van Fraassen's latest work, Scientific Representation: Paradoxes of Perspective, where makes a new distinction between phenomena and appearances: phenomena are observable entities whereas appearances are the contents of observation or measurement outcomes. If, as he argues, saving the phenomena is not the same as saving the appearances, can "saving the appearances" provide a better answer the question "What is Science?" After characterizing a doctrine of aim accurately as a proffered rational reconstruction of scientific activity and making explicit specific criteria of assessment, this paper shows how "saving the appearances" provides the better answer. Its purpose is to suggest that either saving the phenomena is the same as saving the appearances (in successful scientific practice) or van Fraassen has misconstrued the question "What is Science?"

Nicolas Leclair-Dufour (leclair.nicolas@gmail.com)
Aristote et l'évolution de la théorie mathématique des proportions
Session F3
Avec son article Archimedes and the Pre-Euclidean Proportion Theory, W. R. Knorr a éveillé un nouvel engouement chez les chercheurs, d'abord stimulé par les Eudoxos-Studien d'O. Becker, face à l'évolution de la théorie pré-euclidienne des proportions - en particulier celle d'Eudoxe, que l'on retrouverait chez Archimède, Pappus et dans le livre XII des Éléments d'Euclide -, théorie qui contrasterait avec celle exposée au livre V de ces mêmes Éléments. Outre le type de preuve et les indications explicites d'Archimède, ce sont, entre autres, les témoignages d'Aristote qui appuient la thèse de Knorr et plus particulièrement, permettent à Becker de reconstruire l'évolution de la théorie des proportions, en ce qu'Aristote affirme explicitement qu'il a été témoin, de son vivant, de la généralisation des preuves de cette même théorie, de particulières qu'elles étaient d'abord. Notre but ici est, non pas de rechercher chez Aristote des preuves mathématiques de type eudoxéennes, comme l'ont fait plusieurs chercheurs à la suite de Knorr, mais d'évaluer l'apparente contradiction entre la théorie générale du livre V et son application particulière au livre XII, à la lumière de la théorie aristotélicienne de la démonstration scientifique et des types de preuves mathématiques qu'il utilise dans ses écrits physiques. Il s'agit donc de justifier deux types de preuves et d'axiomes - ceux d'Archimède (Eudoxe) et d'Euclide (Éléments, V) -, à partir d'une théorie générale des mathématiques et de la démonstration scientifique, sans pour autant dévaluer les apports de Knorr, mais bien en les justifiant par une dimension fondationnelle que nous croyons trouver chez Aristote.

Susanne Lettow (lettow@iwm.at)
Heredity, Race, and Reproduction. Genealogical Thinking in the Nature Philosophies of Kant and Schelling
Session G2
The nature philosophies of Kant and Schelling contribute, each in a specific way, to the development of biology as a discrete area of knowledge. Of central importance are their articulations of genealogical concepts – heredity, race and reproduction – that in the context of the temporalisation of natural history acquired specific biological meaning. In Kant's theoretical reflections on knowledge of nature, the concept of heredity first became more than a juridical term, and also 'race' began to have a fixed meaning. In addition, in the second half of the eighteenth century, in the context of the transformation of the synchronically oriented natural history into a diachronically and genealogically oriented biology, the concept of reproduction emerged as a reference to processes of generation as well as regeneration.

Eleanor Louson (elouson@gmail.com)
Nature, Real to Reel: Why HPS should care about wildlife films
Session A4
Wildlife documentaries are a major source of the public's exposure to wild animal behaviour and biological explanations for this behaviour, yet historians and philosophers of biology have yet to seriously examine this promising medium. Drawing on a brief history of the genre, I propose an explanatory schema of the viewer's mediated encounter with nature, with a focus on the framing of wildlife films. Examining recent wildlife films, I will argue

that the genre's mediating contexts have significant consequences for the public's perception of animal behaviour. Wildlife documentaries represent nature, but their vision is selective and highly edited. These films are produced under multiple constraints, including research orientation, entertainment value, and external goals such as conservation. Modern wildlife films are the result of the genre's changing standards of authenticity, multiple reimaginings of the documentary form and changing ideas about what counts as genuine animal behaviour. The early staging of animal interactions and faked displays of combat have given way to more subtle mediating practices to obtain desirable vignettes. Even noninterventionist filming distorts nature for the screen, creating a more active, spectacular, violent or family-friendly nature, depending on the aims of the film in question. HPS scholars have much to gain from wildlife films; like theories of animal behaviour in biology, they are as much a result of the changing human perspective on nature as they are a representative depiction of animal life. Any account of biology's impact on the public can only benefit from a closer look at the screen.

Alexandru Manafu (alexmanafu@gmail.com)
The British Emergentist View on Chemistry
Session C3
In 1923, C.D. Broad delivered the Tarner Lectures in the philosophy of science at Trinity College, Cambridge. In the subsequent couple of years, Broad produced The Mind and its Place in Nature, a monograph based on these lectures. Broad's book is considered representative for the current of thought that came to be known as "British emergentism". In recent years, Broad's work has received an increased amount of attention from authors concerned with emergence and reduction in all disciplines, and especially in the philosophy of mind. While it is true that Broad's motivation for the development of what he calls the "theory of emergence" was to account for the existence or appearance of secondary qualities such as colours and odours (which seem resistant to a purely mechanistic explanation), Broad's primary example of emergence is that of chemical compounds. According to Broad, in order to learn about the properties of chemical compounds such AgCl (silver chloride), one must study samples of this substance, as they cannot be deduced, even in principle, from the properties of silver and those of chlorine taken separately or in other combinations. The purpose of this paper is twofold: first, to examine and make explicit Broad's views on chemistry as the primary example of emergence; second, to investigate whether Broad's position regarding chemistry could still be upheld today, after more than eight decades of scientific developments in theoretical and computational quantum chemistry.

Dan McArthur (djmc@yorku.ca)
Mark Champagne (gnosiology@hotmail.com)
On Structural Realism's Optimistic Meta-Inductio
Session B3
Structural realists have basically made their case in two ways. First, they have highlighted telling historical examples of mathematical expressions surviving radical theory-change. Second, they have proposed that this is so because "structure" enjoys (epistemological or metaphysical) primacy over "nature", such that a vindication of scientific realism can focus on relations at the expense of their transient relata. Although the bulk of the discussion has thus far centered on making this segregation tenable, we argue that historical evidence of "theory-constancy" suffices, and that such an inductive appeal is more genial to naturalism than any armchair account of structure's resiliency.

Nicolas McGinnis (nmcginni@uwo.ca)
Simplicity and the Russell-Poincaré Debate
Session A3
Poincaré's appeal to the notion of simplicity in his articulation of conventionalism has been a major subject of debate, particularly in light of his claim that we should always prefer Euclidean geometry over its alternatives on such a ground. Poincaré himself never offers an account of simplicity (or indeed what generally constitutes a proposal being the more 'advantageous') save in the broadest possible terms. In what follows I will first sketch out Poincaré's 'conventionalist' views, particularly in contrast to the logicist program—exemplified here by Russell—and then proceed to examine an account of 'simplicity' derived from algorithmic information theory. It is the task of this paper to argue that the notion can be made into a more rigorously defined aspect of the inferential process, which in Poincaré includes sundry non-logical devices. An adequate formalization of simplicity directly bolsters Poincaré's anti-logicism by providing adequate grounds for its use as a methodological constraint without contravening the central theses of his conventionalism.

Francesca Merlin (francesca.merlin@gmail.com)
La notion d'évoluabilité en biologie : limites et perspectives
Session G3
Durant les vingt dernières années, la notion d'« évoluabilité » a été appliquée à plusieurs phénomènes biologiques impliqués

dans la régulation de la variabilité chez les organismes vivants (par exemple, l'augmentation du taux de mutation dans des conditions de stress, Radman et al 1999 ; l'accumulation de variations génétiques neutres et leur expression en réponse aux changements de l'environnement, Rutherford 2003). Cependant, il n'y a pas de consensus quant à la définition de cette notion qui, au sens littéral, désigne la « capacité à évoluer ». En particulier, le désaccord porte sur le système biologique auquel on doit attribuer cette capacité évolutionnaire (un génome, une population, une lignée). La plupart des définitions proposées caractérisent l'évoluabilité par rapport au processus de sélection, comme la capacité d'un système biologique à produire de la variation héréditaire favorable (par exemple, Wagner & Altenberg 1996, Kirschner & Gerhart 1998). Nous mettrons en évidence les limites de ces définitions que nous qualifions d'« adaptationnistes » : elles consistent à n'attribuer d'importance qu'aux conséquences évolutionnaires adaptatives. Ensuite, nous proposerons de définir l'évoluabilité comme la mesure des potentialités évolutionnaires (morphologiques et/ou fonctionnelles) d'une population naturelle, indépendamment de leur valeur adaptative – qui varie en fonction du contexte particulier où ces potentialités se manifestent – et donc indépendamment du processus de sélection. Nous montrerons le bien-fondé et l'intérêt de notre définition par l'analyse des phénomènes biologiques mentionnés ci-dessus, que les biologistes considèrent comme des facteurs d'évoluabilité. Nous terminerons par quelques réflexions au sujet du rôle de la notion d'évoluabilité en biologie.

Nora Mills Boyd (nboyd@uwaterloo.ca)
Truth and the Shape of Space: Trouble for Deflationists in the Application of Mathematics
Session A1
Abstract: In this paper I bring the debate between advocates of deflationary and correspondence theories of truth to bear on a particular example of the application of mathematics in physical science: the topology of the universe. Responding to Penelope Maddy's challenge that the only notions of truth and reference that are needed to "say the things we want to say" are disquotational T and R-type sentences, I argue that this application of mathematics demonstrates that there are at least some important things that the deflationist cannot say. I employ strategies introduced by Patricia Marino to highlight some problems that the deflationist faces. If we think that mathematical geometry and physical geometry are separated by a difference in kind, then none of the statements of applied mathematics are true simpliciter. Insofar as these are statements that we want to assert, a deflationary account of truth proves to be insufficient. However, we might think that the shape of the universe is properly a mathematical object. In this case, a deflationist still faces a problem in that mere T-sentences cannot account for the non-repeating truth conditions inherent in the observational statements of physical cosmology. I also argue that my example is safe from some of the responses that the deflationist might want to make against two of the examples currently in circulation in this debate.

Benjamin Mitchell (B.D.Mitchell@Dal.ca)
Deep Things: History, Mythology and Deep Sea Biology.
Session C2
If something is large, and from the darker corners of the deepest oceans, it is called a sea monster, or, if it is vaguely tubular in shape, a sea serpent. With our early attempts to explore the deep places that these legends call home, we find to our surprise that the fallacies and fancies of bygone eras, if not exactly true, do bear a striking resemblance to entities in the natural world. Where they fail to bear a resemblance to actual creatures, they instead show us a telling picture of how ancient and medieval peoples interpreted what life was like in the unknown oceans. This paper will examine how people throughout history have tried to interpret the meaning of deep-sea life. No creature in the animal kingdom represents the various dimensions of human experience with the deep sea and sea monsters as well as the giant squid, or Architeuthis dux, which will be given particular attention as the preeminent crossroads connecting history, mythology and biology. In the modern world, the marine biologists' response to ancient and medieval accounts of sea monsters has varied significantly from the 1800s onwards. Around the time of the "Darwinian Revolution", new credence was lent to the possibility that such creatures may actually exist. As recently as the 1950s this credence was powerful enough to help launch the Danish Galathea expedition, with the sea serpent as one of its driving factors.

Amir Mohammad Gamini (amirm_gamini@yahoo.com)
The Golden Age of Science in Islamic Period and the Role of non-Ptolemaic Models
Session F4
According to the 'Classical Narrative', al-Ghazali and ash'arian anti-philosophy attack ended what is commonly called 'Golden Age of Science' in Islamic period. But the discovery of non-Ptolemaic Models and their important role in development of the Copernican models of universe raised serious doubt concerning the validity of this claim. It showed that not only astronomical traditions in Islamic period did not finish in the thirteenth century, but also they contributed immensely to the intellectual heritage of that period. Technical innovations and complex mathematical

models used by astronomers at this time made this period golden age of science. As recently argued by Saliba and some other historians, contrary to what the 'Classical Narrative' states varieties of methods and techniques used by Islamic astronomers during this period demonstrate high level of creativity and innovations. This paper consists of two parts. In the first part, I will consider these models in details and will explain complicated and intelligent mathematical methods used in them. Using Kuhn's terminology, I will describe this period as a crisis time in which different solutions to anomalies have been offered by Islamic astronomers to rescue the original paradigm. These models are originated and developed from a philosophical and theoretical tension within the Ptolemaic-Aristotelian framework. On the other hand, some known points connect these models with the Copernicus's heliocentric models.In the second part, I will raise the some questions. Having raised these questions, I will express my reservations regarding whether it is appropriate to call that period 'Golden Age of Science'.

Shari Monner (slmonner@ucalgary.ca)
Context Relativity of Biological Character Identification
Session G3
'Character' can be loosely defined as any feature of a species that is picked out as distinctive. 'Has scales', 'lacks forelimbs', and 'has over 300 vertebrae', are all characters of snakes. Characters are the data biologists use to determine the evolutionary history and inter-relation of species. 'Has over 300 vertebrae' can distinguish snakes as a group from the group of all lizards, while 'has scales' can provide evidence for a closer evolutionary relationship between snakes and lizards than between snakes and mammals or lizards and mammals. The accuracy of the description that the set of all a species' characters provides thus determines the accuracy of the inferred relationships between that species and any other. While biologists have a principle to determine the most appropriate evolutionary classification from a single character set, there is no agreed on principle to identify the character set of each species. In 2003 Richard Richards looked at a variety of principles for identifying characters and found all of them to be defective. He concluded that character identification and, consequently, evolutionary inferences are arbitrary. In this paper I will argue that character identification in biology is not arbitrary, but context relative, and that such relativity is required for adequate character identification.

Deirdre Moore (deirdremoore@gmail.com)
Herophilus' Pulsating Medicine
Session D4
In the second century AD, Galen, who often heaped scorn on his predecessors, wrote of "a man who is known by everybody to have surpassed the great majority of the ancients, not only in width of knowledge but in intellect, and to have advanced the art of medicine in many ways". The man who received Galen's praise was the anatomist and physician Herophilus of Chalcedon. Herophilus lived and practiced medicine in Alexandria, sometime between 330 and 250 Herophilus wrote a substantial work on pulses incorporating a constellation of contemporary Alexandrian scholarly influences including medicine, mechanics, philosophy and music. Alexandrian understandings of time and timing in these fields figure centrally in Herophilus' work on pulses. Herophilus' development of pulse theory in relation to musical rhythms was highly influential in the ancient world. Nor were his discoveries restricted to exploratory ventures. Keenly interested in the diagnostic applications of his findings, he allegedly constructed a pulse timing clepsydra (water-clock) derived from Alexandrian innovations of this instrument. Herophilus was responsible for developing sphygmology (study of the pulse) to a greater degree than any other figure in the Hellenistic or classical period. In his work on pulses Herophilus adapts and integrates the diverse worlds of knowledge around him. Herophilus applied contemporary Alexandrian methods of time reckoning and measurement in their practical mechanical and musical forms to the movement of the pulses in the different seasons of human life. Under Herophilus' scheme the external technology of measured time becomes internal to the human body.

Rebecca Moore (r.moore@utoronto.ca)
Categorization and Controversy: Redefining genetically modified organisms (GMOs)
Session G1
Developed in the early 1970s in the United States, recombinant DNA (rDNA) techniques allow for the transfer of specific genes from one organism to another, usually to confer a desired trait, with unprecedented speed and without the restrictions of previous breeding methods. All agricultural cultivars developed using recombinant DNA technology are frequently grouped together to create the category of GMOs (genetically modified organism) or GM (genetically modified) crops, and understood to carry the same characteristics and risks. This contributes to a popular misunderstanding of what constitutes a GMO, but the real danger of the uniform characterization of GMOs is that it has been used to inform international policy decisions. One need look no further than the 2002 famine in Zambia when food aid was rejected by the Zambian government on the grounds of it being 'genetically modified' for an example of

the strength of the GMO category. In this paper I argue that there is very little consistency among GMOs and objection to the entire category is, in most cases, inappropriate. I will illustrate that the misunderstanding of what constitutes a GMO and how to appropriately characterize the category has contributed significantly to the debate regarding the appropriate use of rDNA technology in agriculture, and has led to inappropriate policy frameworks that treat the products of recombinant DNA technology uniformly. I put forth a new schema for understanding the agricultural products of rDNA technology to help clarify the debate and make more nuanced policy decisions without ascribing inappropriate characteristics to all rDNA-derived plants.

Kathryn Morris (Kathryn.Morris@DAL.CA)
Dreams, Demons, and Astronomy: Copernicanism and Kepler's Somnium
Session F1
Johannes Kepler's Somnium is a strange and fragmented work. The core of the text, written as a student assignment in 1593, provides a detailed lunar astronomy. Sixteen years later Kepler added a framing narrative, in which the protagonist falls asleep while reading a history of Bohemia, only to dream of a young astronomer who is transported to the moon with the help of a group of friendly demons. The Somnium has attracted the interest of literary theorists, who have claimed it as an early work of science fiction. Other commentators have focussed their discussions on the astronomical content of the text. It is clear, however, that the scientific and literary aspects of the Somnium were connected for Kepler, as he states in a note that "the purpose of my Dream is to use the example of the moon to build up an argument in favour of the motion of the earth, or rather to overcome objections taken from the universal opposition of mankind." In this paper I will discuss the ways in which Kepler uses fiction to support his Copernicanism. In particular, I will argue that he uses imaginative literature to bridge mathematical astronomy and natural philosophy by allowing the reader to "virtually experience" a moving moon. I will also compare the Somnium to other pre-and post-Copernican moon voyages in order to show how Kepler shaped the genre to the particular needs of his scientific project.

Taylor Murphy (taylorsmurphy@gmail.com)
Between basic and applied science: the conflation underlying the linear model of science
Session B3
In 1945, Vannevar Bush delivered a hallmark report to U.S. president Roosevelt, where he gave an account of scientific research and its relation to society. In this report, he argued that there were two types of research: basic and applied science. This view provides a two-step model of scientific research: basic science provides a wealth of scientific information that applied science subsequently draws on (dubbed the "linear model"). Without constant basic scientific research, applied science will run out of resources. This distinction and view of research remains popular in philosophical discourse, as well as in science policy in North America.Importantly, it is supposed that there is a trade-off between doing basic and applied scientific research, the latter being immediately beneficial and the former an investment for future benefits. Drawing on the research of Pasteur, Bohr and Edison, as well as the work of Carrier (2007) and Stokes (1997), I argue that the linear model conflates two distinct dimensions of scientific research. What emerges from this view is that there is no such trade-off between basic and applied research, and that this allows for the best of both basic and applied science to be pursued in tandem. However, it still remains that a type of research devoid of any immediate value appears necessary, but it is neither basic nor applied science as traditionally conceived. I then discuss the implications on philosophical accounts of scientific innovation and with relation science funding policy, where the linear model is ubiquitous.

Gemma Murray (ggrm2@cam.ac.uk)
Is Quantum Mechanics About Quantum Information? Bub's Information-Theoretic Interpretation of Quantum Mechanics
Session D2
In this paper I provide a reading of an interpretation of orthodox quantum mechanics (QM) as a principle theory about quantum information advocated by Jeffrey Bub in several recent papers. Bub bases his interpretation on his previous derivation (with his co-authors Clifton and Halvorson) of the basic features of QM from three fundamental information-theoretic constraints. He construes this theorem (CBH) as a principle theory of QM, in the sense used by Einstein to characterise special relativity, in order to interpret QM as a theory about quantum information. My reading supplements Bub's account primarily through providing an analysis of the nature of the constraints in CBH and revealing the empiricist motivations which underlie his interpretation. I draw upon Bub's analogy with special relativity to show that some principle theories constrain corresponding constructive theories to a greater degree than is generally understood – a consequence of (i) some facts about the world being empirically unascertainable and (ii) concerns about 'idling cogs' in our scientific theories. I argue that Bub's claim that QM is about quantum information can be taken as a minimal claim, not postulating information as a worldly stuff. I defend this construal of Bub's interpretation

through consideration of the way the interpretation seeks to reveal rather than reduce the characteristic features of QM, and by revealing how his approach demonstrates the impossibility of such a reduction.

Wayne Myrvold (wmyrvold@uwo.ca)
Maxwell and a Third 2nd Law of Thermodynamics
Session D1
It has long been recognized that there are two distinct laws that go by the name of the Second Law of Thermodynamics. The original says that there can be no process resulting in a net decrease in the total entropy of all bodies involved. A consequence of the kinetic theory of heat is that this law will not be strictly true; statistical fluctuations will result in small spontaneous transfers of heat from a cooler to a warmer body. The currently accepted version of the Second Law is probabilistic: spontaneous transfer of heat from a cooler to a warmer body is not impossible, merely improbable. There can be no process whose expected result is a net decrease in total entropy.According to Maxwell, the Second Law has only statistical validity, and this statement is easily read as an endorsement of the probabilistic version. I argue that a close reading of Maxwell, with attention to his use of "statistical," shows that the version of the second law endorsed by Maxwell is strictly weaker than our probabilistic version. According to Maxwell, even the probable truth of the second law is limited to situations in which we deal with matter only in bulk and are unable to observe or manipulate individual molecules. Maxwell's version does not rule out a device that could, predictably and reliably, transfer heat from a cooler to a warmer body without a compensating increase in entropy. I will discuss the evidence we have for these two laws, Maxwell's and ours.

Kathleen Okruhlik (okruhlik@uwo.ca)
John Stuart Mill and the Democratization of Science
Session F2
As calls to "democratize science" become more urgent and widespread, some philosophers of science have turned to John Stuart Mill for help. In some ways this is unsurprising, since Mill is not only famous for his political theory but also unusual in the attention he pays to the social dimensions of science. From another perspective, however, some of these appeals to Mill are at least prima facie bizarre insofar as they turn to On Liberty for arguments in favour of democratic control of scientific inquiry. Mill wrote that essay precisely because of his belief that democracy tends to promote the tyranny of the majority—and thus to undermine free inquiry and progress toward truth. Not all these appeals to Mill by philosophers of science take the same form; some seem more Millian than others. In this paper I examine Paul Feyerabend's appeals to On Liberty during his anarchistic and relativist periods; Philip Kitcher's arguments in Science, Truth, and Democracy; and Helen Longino's use of Mill in her development of a theory of science as social knowledge.

Flavia Padovani (padovani@interchange.ubc.ca)
Probability between Fiction and Reality: Reichenbach's Correspondence with Paul Hertz
Session D3
The possibility of tracing back the difference between causality and probability to two specific forms of lawfulness and the analysis of their interrelation are among the main issues tackled in Reichenbach's work, as well as the central question addressed in his doctoral thesis, The concept of probability in the mathematical representation of reality (1915). In this work, Reichenbach still interprets the principles of probability and causality within a deterministic framework. The laws of nature are mathematical representations of necessary (causal) connections between events, but these representations presuppose certain probabilistic assumptions allowing us to rely on the approximate results of our measurements and finally elaborate physical laws on a firm footing. In some autobiographical notes of 1927, while recalling the positive results of his dissertation, Reichenbach refers to some objections to his early interpretation initially raised by Kurt Grelling, and later taken up by Paul Hertz. My paper analyses these objections in the context of the barely mentioned 1920—1921 correspondence between Hertz and Reichenbach and shows how these discussions, centred on the crucial problem of applicability of probability statements, undoubtedly influenced Reichenbach in his shift towards considering probability as primitive with respect to causality. Probability as a (methodological) fiction is no more than a useful tool for mathematical representation; as a hypothesis about reality, instead, it is a much stronger, if not the strongest assumption within a system. Probability cannot be based on an assertion of certainty, but rather must be rooted in probability itself as a principle of knowledge: the most fundamental one.

Kent Peacock (kent.peacock@uleth.ca)
Three Faces of Ecological Fitness
Session G3
This paper takes it that fitness is most usefully understood not merely as a parameter descriptive of reproductive success, but rather as those traits of organisms that are explanatory of survival in the face of the challenges posed to those organisms in their ecological contexts. On this view there are three sorts of "ecological" fitness: the well-

documented ability to compete, the ability to cooperate (as in mutualistic symbiosis), and a third sense of fitness that has received insufficient attention in evolutionary theory, the ability to construct. (Recent contributions from Odling-Smee and coworkers on niche construction have gone some way to fill this gap in evolutionary theory.) Following Lotka, constructive fitness can be understood thermodynamically as the ability to maintain or enlarge the energy-circulating capacity of an ecosystem. An organism that is constructively fit could end up with its gene frequency constant through time but its probability of survival protected or enhanced since it would have sustained or increased the total carrying capacity of its ecosystem. (Such organisms would then exhibit differential persistence, which has been suggested by Bouchard and others to be a measure of evolutionary success that is at least as important as differential dominance at a given time.) Photosynthesizers and other autotrophs are obvious candidates for constructively fit organisms, but any organisms, including heterotrophs, can exhibit constructive fitness if they have mechanisms for channeling free energy into their ecosystems. I will examine the prospects for the human species in the light of these considerations.

Makmiller Pedroso (makmiller@gmail.com)
Essentialism, History, and Biological Taxa
Session E3
Recent writers are committed to a new brand of essentialism called "historical essentialism" (Queiroz[1995]; Griffiths[1999]; LaPorte[2004]). According to this brand of essentialism, relations of common ancestry are essential features of biological taxa. The main argument used to back up this assumption, due to Griffiths[1999] and LaPorte[2004],is that the dominant school of classification, the cladistic school, defines biological taxa in terms of common ancestry. The goal of my talk is to show two difficulties with this argument: (1) Arguments that appeal to a consensus of a group are convincing only if such a consensus exists. However, the dispute between pattern and process cladists compromises the consensus necessary for justifying historical essentialism: unlike process cladists, pattern cladists do not define biological taxa in terms of ancestor-descendant relations. Hence, citing the cladistic school alone does not justify historical essentialism. Cladistics supports historical essentialism only if additional premises are provided. In particular, historical essentialists need to argue that process cladistics is right, pattern cladistics is wrong. (2) Both Griffiths[1999] and LaPorte[2004] argue that sharing a certain ancestor is an essential attribute of biological taxa. This assumption is in disagreement with cladistics, however. For although synapomorphies provide membership conditions for a clade, they do not distinguish which taxon is the most recent ancestor of a clade. Synapomorphies only provide evidence that two taxa are more related to each other than a third taxon. Hence, cladistics does not in itself produce any evidence for the existence of historical essences.

Vesselin Petkov (vpetkov@alcor.concordia.ca)
Realism and explanation – the necessary common ground for a genuinely fruitful interaction between science and philosophy of science
Session B4
The collaboration between scientists and philosophers of science is not what it should be. Although there are various reasons for that I suggest that sharing an explicit realistic view on the nature of scientific theories and a comprehensive view on scientific explanation can provide the minimal common ground for collaboration between scientists and philosophers of science that can produce tangible results. It is usually assumed that scientists are overwhelmingly realists but this is not always the case, especially in fundamental physics, since physicists often doubt whether theoretical concepts reflect anything real. This tendency in theoretical physics is best demonstrated in a recent article by N. David Mermin in which he insisted on not considering the "most successful abstractions to be real properties of our world." To demonstrate how productive the interaction of scientists and philosophers of science can be, I will discuss two examples: (i) endurantism versus perdurantism (how physicists can help philosophers resolve a debate), and (ii) the nature of the quantum object (how philosophers of science should abandon their secondary role in the pursuit of scientific knowledge and start actively participating in the advancement of science by carrying out rigorous conceptual analyses of open questions in science).

Josipa Petrunic (j.petrunic@ucl.ac.uk)
What George Peacock's "Principle of the Permanence of Equivalent Forms" (1830) can tell historians, philosophers and sociologists about the generation of mathematical knowledge
Session F2
The use of historical case studies in the philosophy of mathematics has recently become a matter of wide-ranging methodological debates. In particular, Donald Gillies' edited collection, Mathematical Reasoning and Heuristics (2005), highlights the varied concerns that arise when historical case studies in mathematics—and especially those stemming from pre-20th century developments—are given a more central role in philosophical analyses of the generation, legitimization and dissemination of mathematical knowledge. Similar concerns are also reflected in the works

of contemporary sociologists of scientific knowledge, many of whom see scientific knowledge as historically contingent and culturally shaped, but many of whom also stop short of applying these views to mathematical claims. Historians of mathematics play a special role in mediating between these philosophical and sociological debates. In this paper, I will discuss the development of symbolical algebra as defined by George Peacock's "Principle of the Permanence of Equivalent Forms"—a philosophical and mathematical view that Peacock issued in 1830. I will argue the "Principle" was an output of industrialization in Britain as well as local, political and social influences prominent in Cambridge in the early 19th-century. I will then use this case study to reflect upon the philosophical and sociological question of how it is that mathematical knowledge gets generated? By comparing and contrasting responses that contemporary philosophers and sociologists of mathematics have recently offered, I will use the Peacockian case study to demonstrate that mathematical knowledge claims are, indeed, profoundly historical and contingent—a fact that both philosophers and sociologists must take better account of in the future.

Stéphane Plante (s.plante@umontreal.ca)
Conciliating Knowing and Doing The Material Use of Scientific Models
Session H1
My aim in this talk will be to articulate the interaction between abstract knowledge and know-how in science. I find my inspiration from Keller who argues that an adequate conception of scientific models must take into account not only the conceptual use of models by scientists but also their material use in technological applications. I therefore explore the possibility of extending our conception of scientific models to include this material aspect. Many features of the use of scientific models will be put in question. The role theories must first be re-evaluated to accommodate the possibility of autonomous models. Consequently, the confirmation of theories becomes secondary, while the confirmation of models becomes fundamental to their accurate application, justifying an instrumental conception of models. As such, the division of labour between theoreticians, experimenters and engineers can be clarified. It reveals distinct types of models and a hierarchy among them. It also confirms a conflicting dichotomy between, in the words of Layton, "knowing" and "doing". My aim is to better represent this fundamental dichotomy which I take to be the keystone in distinguishing science from other types of knowledge. I argue, contra Keller, that this dichotomy can be included in our conception of scientific models without the need to resort to a new type of model. For this purpose, I put forward, as a first approximation, a revision of the scientific modeling process presented by Giere. This revision, I propose, could represent equally the work of theoreticians and experimenters of all fields.

Nicholas Ray (nray2@uwo.ca)
The Aufbau and Conceptual Freedom
Session D3
In "Epistemology in the Aufbau", Michael Friedman argues that Carnap's early reconstruction of physics using the concepts of autopsychology is often misrepresented as a "phenomenological reduction" of the sort offered by Russell. While Carnap chose to reduce physical concepts to the experiential concepts of a single cognizer, this choice was arbitrary. As Carnap makes explicit in the Aufbau, one could take the alternative approach of reducing the concepts of autopsychology to those of physics. The typical Quinean charge, therefore, that Carnap's program was an instance of phenomenological reduction, is a clear misunderstanding. The project may be reductive, but in no way is the reduction akin to Russell's. Firstly, it is not necessarily phenomenalistic. Secondly, because it is not necessarily phenomenalistic, it does not view the concepts of experience as essentially fixed and semantically given. As I will show, this mistaken understanding of Carnap did not end with Quine. The mistake has been made recently by Anil Gupta, for example, in his Empiricism and Experience. I want to argue that Carnap's project in the Aufbau should thus be read differently. Far from being a continuation of the British empiricist tradition of making sense of the external world through a clever use of psychological, logical and mathematical concepts, it is the first example of an epistemology of science that provides the individual cognizer with a comprehensive conceptual freedom to choose foundational concepts at will before engaging in reconstruction.

Céline Riverin (celine.riverin@umontreal.ca)
La nature et le statut des hypothèses astronomiques selon Kepler
Session F1
L'apport de l'astronome et mathématicien Johannes Kepler (1571-1630) est immense : non seulement fut-il le premier défenseur sérieux de l'héliocentrisme de Copernic, mais il a permis rien de moins que la naissance de l'astrophysique, en concevant l'étude du mouvement sidéral comme une physique céleste, c'est-à-dire une discipline mixte alliant une description géométrique à des fondements physiques et cosmologiques. Néanmoins, l'absence d'ouvrages proprement philosophiques ou épistémologiques de la part de ce savant est parfois surprenante tant il est évident que l'astronome réfléchissait au sujet de la science qu'il

était en train de révolutionner. Il est toutefois possible de reconstruire sa pensée au sujet de l'astronomie elle-même en étudiant différents passages qui, une fois regroupés, s'organisent de façon cohérente et éloquente tout en trouvant des échos dans la façon dont il a pratiqué sa science. Notre communication a pour objectif de mettre en lumière les conceptions de Kepler au sujet de l'astronomie elle-même – plus particulièrement, ses propos concernant la nature, le statut et l'évaluation des hypothèses astronomiques – en s'intéressant notamment à l'importance de la reconstruction des mouvements célestes (démonstration géométrique) par rapport à celle de la méthode fondée sur la compréhension de la nature des objets célestes (démonstration physique). Nous verrons également comment Kepler reprend la méthode syllogistique en y greffant des éléments pythagoriciens pour obtenir une science astronomique dont la valeur va au-delà de celle d'une entreprise purement descriptive et prédictive.

Ryan Samaroo (ryan.samaroo@philosophy.ox.ac.uk)
What is background-independence?
Session A2
A physical theory is said to be background-independent if it is free of geometrical structures that remain invariant under dynamical evolution. Proponents of the canonical quantisation programme including Smolin and Rovelli think that a quantised theory of gravity will be background-independent, and they take background-independence to be a good research strategy. In general, they identify background-independence with some form of general covariance. My task is one of conceptual clarification. I show that the identification of background-independence with general covariance fails to account for forms of background-structure that are not encoded in the geometrical objects on a spacetime manifold. I argue that there is no sufficiently general definition of background-independence and that it is unclear whether complete background-independence is even attainable. I begin by reviewing Einstein's understanding of general covariance, with which the current debates about background-independence began, and I confront it with Kretschmann's challenge, which shows general covariance to be merely a condition on the well-formedness of a theory rather than one that restricts its physical content. Then, I present an abstract framework as a basis for talking about classical field theories and their symmetries. Within this framework, I consider a landmark contribution to the understanding of background-independence; namely, the Anderson-Friedman account. The main idea there is that a theory is background-independent just in case it has no non-dynamical objects amongst its fundamental variables up to a symmetry transformation. I close by presenting a more general account that takes into consideration forms of background-structure that escape Anderson's and Friedman's definitions.

Corey Sawkins (ces120@mail.usask.ca)
Determining Underdetermination
Session B3
Arguments from underdetermination take two forms, global underdetermination and local underdetermination. The arguments from global underdetermination bring into question all knowledge, they develop skeptical scenarios where we cannot trust any knowledge that we obtain within the world. The arguments from local underdetermination bring into question the nature of our knowledge and are designed to question scientific realism. This essay is an evaluation of the arguments that claim to do just that, it shows that these arguments are not arguments from local underdetermination but are from global underdetermination. Based on this evaluation a new argument from local underdetermination is developed that shows that local underdetermination is indeed a problem for scientific realism.

Jacob Stegenga (jstegeng@ucsd.edu)
Varieties of Evidential Experience
Session F2
Contemporary philosophical accounts of evidence explicate the notion in terms of probabilities. Instead, I describe the features of evidence which scientists appeal to in practice, which include general features of methods, such as quality and relevance; and general features of evidence, such as patterns in data, concordance with other evidence, and believability of the evidence. Examples from biomedical research will help illustrate these features. I argue that there is no privileged ordering of the relative importance of these features of evidence, and so given some particular evidence there can be competing rational determinations of the likelihood of that evidence.

Mike Stuart (mike.stuart@utoronto.ca)
The Role of Henri Poincaré and Pierre Duhem in the Establishment of Conventionalism in Modern Philosophy of Science
Session A3
Recent work has tried to free Henri Poincaré from the misinterpretations of the logical positivists (Giedymin 1982, McMullin 1990, Friedman 1999, Galison 2001, Zahar 2001, Brenner 2003, Ben-Menahem 2006). Continuing this trend, I submit a new interpretation of the origin of philosophical conventionalism in the philosophy of science, which shows that Pierre Duhem was the true father of the movement.

While Duhem himself criticized Poincaré's conventionalism, it is only upon Duhem's philosophy that the scaffold can be set. I argue that the most important difference between books like Science et l'Hypothèsis and La Thèorie Physique consist in the relationship between mathematics and experience. The authors also held extremely different views on the general nature of theory development: Poincaré sees experience and theory as continuous, where Duhem thinks that theory can often afford to ignore experience. Both writers strive to create a middle ground: between the "realist in the laboratory" and the "anti-realist in the archives," between common-sense intuition and scientific realism. I too, try to find a middle ground; between the conventionalism of the "received view" (garnered from the positivists and contemporaries of Poincaré and Duhem), and what now seems evident given modern scholarship. I finish by showing how the lively debate between the two flowered into the modern debate of structural realist accounts (Worral 1989, Psillos 1995, Chakravartty 1998, Ladyman and Ross 2007) versus empirical adequacy accounts like van Fraassen's (1980, 2002, 2008), and finally considering what other consequences may be drawn from this study of the confused origin of what is now a ubiquitous feature of philosophy departments and textbooks worldwide.

Eran Tal (eran.tal@utoronto.ca)
Measurement and Robustness
Session H2
The current paper argues that a certain sort of robustness condition is constitutive of physical measurement. More specifically, I suggest that a procedure is adequate for measuring a physical quantity if and only if the outcomes of that procedure are shown to converge to those of other, sufficiently diverse procedures when these procedures are appropriately described in terms of that quantity. The necessity and sufficiency of this robustness condition for measurement are argued for by considering three constraints on any admissible method of constructing measuring procedures. These constraints are (i) the requirement that quantity concepts be coordinated to measuring procedures in a non question-begging manner, (ii) the requirement that the unity of quantity concepts across different measuring procedures be demonstrable, and (iii) the requirement that the evaluation and reduction of uncertainty be possible. These requirements are jointly satisfiable by all and only those methods that result in robust families of empirical procedures, in the abovementioned sense of 'robustness'. Finally, I show that the proposed robustness condition accounts for the methods actually used to standardize measurement procedures. This is illustrated with a case study of the standardization of time conducted at the laboratories of the US National Institute of Standards and Technology (NIST).

Morgan Tait (mtait7@uwo.ca)
Why I am not a Quantum Bayesian
Session D2
I argue that the quantum Bayesian ('QBist') interpretation of quantum probabilities defended by Caves, Fuchs and Schack is inadequate. The quantum Bayesian holds that all probabilities, including those occurring in quantum mechanics, are purely subjective. The main positive argument in favor of QBism, the so-called 'steering argument', is examined and rejected. The quantum Bayesian fails to consider the distinction between separability and locality, and is therefore led to conclude erroneously that objective quantum probabilities are incompatible with special relativity.I also consider an objection to quantum Bayesianism recently raised by Chris Timpson, according to which the QBist account is unable to explain the value of gathering data from experiments. I defend the QBists against Timpson's objection, on the grounds that Timpson fails to consider the confirmation-theoretic implications of the Value of Learning Theorem and the de Finetti Representation Theorem. I then argue that the QBist is nevertheless unable to explain the intersubjective agreement within the physics community about the correctness of certain quantum state assignments. To explain this agreement, I invoke the concept of objective single-case probability. The notion of objective chance is neither metaphysically loaded, as the QBists claim, nor explanatorily eliminable. On the contrary, objective chance is an empirically motivated concept that fits with both quantum mechanics and special relativity, and is able to explain a wide variety of phenomena that are mysterious by the lights of subjectivism.

Stéphanie Tésio (irejean2002@yahoo.fr)
Jean-François Gaultier (1708-1756) et la genèse des sciences canadiennes
Session A4
En Europe, dans les colonies françaises, l'Académie royale des Sciences de Paris met en place un réseau de correspondance via ses académiciens qui entretiennent des relations épistolaires avec des médecins disséminés dans le monde. Ce qui est le cas de Jean-François Gaultier, médecin du roi à Québec entre 1742 et 1756. Le XVIIIème siècle est marqué par cette volonté de tout connaître, de tout répertorier sur le globe. Lors de son mandat, Gaultier occupe plusieurs fonctions professionnelles et répond aux demandes de l'Académie. Pour son compte et celui des académiciens, il écrit plusieurs mémoires qui nous font part de ce que sont les sciences au

Canada au milieu du XVIIIème siècle :traités de botanique (description de la végétation dans la vallée du Saint-Laurent), traitémédical (soins à donner), journal d'observations botanicométéorologiques (météo del'époque), traité sur la minéralogie (minéraux présents et ceux à exploiter), traité surl'acériculture (comment produire le sirop d'érable ?). L'ensemble de cet oeuvremanuscrite montre à quel point JeanFrançois Gautier joue un rôle primordial dans lamise en place des connaissances scientifiques canadiennes et dans leur diffusion enFrance à cette époque. En cela Jean-François Gaultier n'est pas uniquement à voir comme le successeur et le continuateur de Michel Sarrazin dont le rôle dans les sciences canadiennes est également fondateur. Ici la communication annonce le prochain projet derecherches qui consiste à examiner dans toute l'oeuvre de Jean-François Gaultier lesinterrelations qui se dessinent : par exemple, médecine-botanique, médecinemétéorologie,botanique-minéralogie

Michael Thicke (mike.thicke@utoronto.ca)
Efficient Science: Achieving Objectivity with Limited Cognitive Ability
Session H2
Philosophers such as Philip Kitcher and Alvin Goldman have offered models of science that are based on a conception of scientists as entrepreneurs, concerned not just with the pure pursuit of truth, but also with more personal goals such as career advancement and recognition. These models are meant to counteract an implication from the Sociology of Scientific Knowledge that the trustworthiness of science depends on the trustworthiness of scientists. While it is generally acknowledged that Kitcher and Goldman's models owe a debt to mainstream economics, there has not been much work detailing just how that connection works, or what it entails.My talk will make the connection between Kitcher's models of scientific activity and a theory of financial economics called the Efficient Market Hypothesis (EMH). According to the EMH, the share price of companies on the stock market is an objective measure of those companies' value, or future earnings. New information about a company is instantaneously reflected in the price of its shares, as buyers and sellers are collectively able to assess the impact of such information. Similarly, the distribution of research effort among scientists may be able to inform us about the aggregated knowledge of scientists in a way that no individual scientist is able to. Conversely, critiques of the EMH can shed light on potential problems with the economic approach to explaining science.

Leslie Tomory (ltomory@gmail.com)
The environmental history of the early British gas industry
Session E1
The gas industry developed in Britain after 1810, and soon became one the worst of industrial polluters, discharging tar, lime, and sulphuric acid into nearby sewers and watercourses. Unlike many other industrial polluters, gasworks were located in cities, and gas companies soon faced a barrage of complaints and nuisance lawsuits from neighbours and fishermen. In this paper, I will explore this pollution, how the gas industry reacted to these lawsuits, and the subsequent political reaction and regulation of the industry. This story is of particular importance in the history of environmental regulation because the Alkali Acts of the 1850s are usually considered to be the first environmental laws regulating industry in Britain. I will show that despite the dominance of laissez-faire attitudes of the period, the government regulated the gas industry on environmental grounds through the use of clauses in municipal and company charters beginning the 1820s. Although the regulations were not very effective, they do show that political action for environmental reasons was possible in early 19th century Britain.

Jonathan Y. Tsou (jtsou@iastate.edu)
Why Depression Is Not a Disease
Session B1
This paper concerns disease explanations of abnormal behavior, focusing on disease explanations of depression. I argue that disease explanations of depression are unwarranted, suggesting that—in most cases—depression is better explained as a normal psychological reaction to distressing events. In supporting this argument, I contrast depression to a number of mental disorders (e.g., schizophrenia, chromosomal disorders), wherein disease explanations are warranted. My analysis assumes that disease explanations of mental disorders are justified when three conditions are met: (1) there is evidence of a dysfunctional biological mechanism, (2) this dysfunctional biological mechanism falls outside the normal range of biological functioning, (3) the dysfunctional biological mechanism causes significant undesirable and harmful effects for the individual. Appealing to research on prevalence rates of depression and cross-cultural research on the variability of depression across cultures, I argue that depression fails to meet condition (2). On the basis of this argument, I suggest that mental disorders can usefully be distinguished into mental illnesses (e.g., schizophrenia, Down syndrome, bipolar disorder), and psychological reactions (e.g., depression, generalized anxiety disorder, posttraumatic stress disorder). I present my analysis in the context of debates in the philosophy of medicine and philosophy of psychiatry (e.g., Thomas Szasz, Christopher Boorse, Peter Sedgwick, Charles Culver, Bernard Gert).

Louis Vervoort (louisvervoort@hotmail.com)
A frequentist interpretation of probability.
Session B4
In the following we propose an interpretation of probability that aims at rendering explicit the fundamental notions of the frequency interpretation of Venn, von Mises, and others. We will argue that (objective) probability can only be defined for events that can be repeated in similar conditions, and that exhibit 'frequency stabilization'. We will partition probabilistic systems into object, environment, and probing subsystem, and show that such partitioning allows to solve paradoxes. By the same token, we will be able to derive a definition of what 'similar events' are – a problematic concept in traditional interpretations -, and point out an analogy between quantum systems and classical ones.

Martin A. Vezér (martinvezer@hotmail.com)
Historical Methodology and Climatology
Session E1
Climatology involves many methods of data collection and analysis that merit explicit philosophical examination. Methods of climate modelling, for instance, aim to explain the past and to make projections about plausible future states of climatic conditions. In such endeavours, modellers depend on data gathered by different methods of scientific inquiry, some of which can be classified as experimental and others of which can be classified as historical. Historical methodology plays a salient role in climate modelling as it is an inevitable component of paleoclimatology, which produces information that climate modellers rely on as 'benchmarks' for calibrating and evaluating their models. However, some philosophers (e.g., Turner 2007) maintain that historical methodology is epistemologically limited in ways that experimental methodology is not and, therefore, the latter is epistemologically superior the former. In this paper, I analyze such claims in a climatological context and investigate techniques deployed in the historical sciences that may be used to work around asymmetries between historical and experimental methodologies, which will be detailed below. I maintain that while historical methodology has its limitations it is, both in principle and in practice, no less objective, scientific or reliable than experimental methodology.

Andrew Wayne (awayne@uoguelph.ca)
Mind the GAP: Explanation in Galileo's New Science of Mechanics
Session F1
Galileo held that a fundamental challenge of his new science of mechanics was to account for experimental results in terms of predictions based on "ideal constructs" satisfying simple and universal hypotheses, or as he put it, "to balance the books." Call this the Galilean Accounting Problem (GAP): the problem of explaining observed phenomena based on claims about highly idealized models. Pierre Duhem famously called Galileo an "impenitent realist" and asserted that this was his biggest mistake. Duhem argued that Galileo's hypotheses save the appearances, but are not true or even likely. For this reason, the GAP is unsolvable. This paper contends that while Duhem may have been correct in his instrumentalism, he was wrong about the GAP; it can be overcome, and moreover Galileo need not have been a realist to do so. The paper argues that the problem of explanation via idealized models is independent of the extent to which those models successfully represent. Duhem also, although far less famously, claimed that Galileo's signal achievement was to save all the appearances with a single set of interconnected hypotheses. The paper makes the case that it is this degree of unity of Galileo's science of mechanics that underwrites the explanatory power of its models.

Erich Weidenhammer (eweidenh@gmail.com)
Medicine and Chemical Practice in Eighteenth-Century Britain
Session C3
It has become commonplace, within the history of science, to acknowledge the diffuse disciplinary boundaries that characterized the study of nature before the modern era. Until the divergence between chemistry and pharmacy, a process that began towards the end of the eighteenth-century, much chemical practice took place within the medical sphere. Well after the seventeenth-century heyday of Paracelsian chemical medicine, chemistry remained a popular idiom through which to interpret the body. Thus chemistry permeated the medical marketplace, from physicians inclined to chemical explanations for certain diseases and their cure (increasingly presented as the result of empirical reasoning and experimental testing), to the apothecaries whose labour did much to shape chemical practice, even (arguably) touching aspects of household kitchen physic. The noted "amateur" natural philosophers of the eighteenth century, experimenters closely associated with the founding of the Chemical Revolution, frequently carried out their chemical work in search of solutions to noted medical issues. By the end of the century, however, chemical practitioners, particularly in France, were turning the field towards a more "philosophical" focus on identifying elements, their properties and combinations. Focusing, by way of example, on the discovery and medical exploration of "fixed air" (our carbon dioxide), this paper will define an approach that makes possible a coherent account of chemistry's place in British medicine over much of the

eighteenth century—a key relationship linking medicine to the Enlightenment project of improvement through natural knowledge.

Aaron Sidney Wright (aaron.wright@utoronto.ca)
Einstein, unified (1914-1921)
Session D1
Einstein has often been portrayed as a man of two minds, one scientific and the other encompassing both his socio-political views and his personal life. This paper argues for a unified treatment of Einstein, and develops Gerald Holton's conception of `resonance' between the physical and the social in Einstein's thought. It combines both technical and social treatments. Rather than seeking causal relations between the social and the scientific, this paper uncovers their commonalities and parallel development. Using Einstein's published work and extensive personal and scientific correspondence from the time he arrived in Berlin in 1914 until 1921, it is shown that on multiple levels Einstein's social and scientific thought resonated. His empiricist stance on the importance of direct sensory experience is shared between his thoughts on relativity and on how to further the cause of scientific internationalism. This scientific internationalism---the belief that scientists must collaborate across national borders, and that they had the power to effect international political change---expressed an elitism that carried into his understanding of how scientific disputes should be resolved. Most tellingly, it is shown that a `style of thought' Holton identified in Einstein's development of special relativity applies both to the genesis of general relativity and to Einstein's views on how to foster scientific internationalism in interwar Europe. In both areas, Einstein applied a combination of empiricist insistence on the primacy of sensory experience and an appeal to transcendental principles. The paper concludes with Einstein's own admixture of relativity and politics, in humour.

Amy Wuest (awuest@uwo.ca)
Emilie du Châtelet, Newton's Method, and the Vis Viva Controversy
Session F3
Emilie du Châtelet is known for her French translation of Newton's Philosophiae Naturalis Principia Mathematica. However, she also has an original work on physics, the Institutions Physique. Du Châtelet's project there is to synthesize Newton's mechanics with Leibniz's metaphysics. Her project of synthesis comes to a head when she gives an argument for the contentious subject of vis viva. Vis viva, or Leibniz's way of accounting for kinetic energy as the mass times the square of the velocity of a moving body (mv_), was dismissed by the followers of Newton as an ad hoc introduction of metaphysics into natural philosophy. Du Châtelet seeks to bring these two camps together in the case of vis viva by using Newton's method of argument in the Principia. I argue that she, like Newton, relies on systematic dependencies between her mathematical analysis, empirical data, and the definition of theoretical terms to justify her theory. To make clear Newton's method of argument I examine how Newton uses his rules of reasoning and his specific argument for the two chief-worlds problem in Book III of the Principia. Du Châtelet recreates Newton's style of argument in her account of vis viva by using Leibniz's principle's of reasoning together with previously confirmed experimental results to create a theory with a rich, interconnected web of justification.

-SELECTED SPECIAL EVENTS AT CONGRESS-

Robert Darnton, Technology and the Book
Fri May 28, 2010 from 12:15 to 13:20
John Molson School of Business, room 1-210
Books have proven to be extremely efficient tools for the delivery of written and visual content. Since Gutenberg, the mass-produced, easily-transported and easily-distributed book has been one of the most important intellectual technologies in the human repertoire. How will "the book" develop and change given the spread of Open Access, the progress of Google Book Search and the plethora of new reading devices? What do these current developments say about our relationship with books and the printed word?
As an intellectual pioneer in the history of the book and the Director of the Harvard University Library, Robert Darnton is a unique authority on the life and role of the book in society. Join one of North America's leading voices in this critical debate about the digital future of books and knowledge.

Céline Galipeau, A Journalist's Reflections on Human Rights
Fri May 28, 2010 from 19:00 to 20:00
Location TBA
The anchor of Radio-Canada's weekday Téléjournal and one of Canada's leading journalists, Céline Galipeau has witnessed the consequences of war on human rights in such places as Somalia, Chechnya, Iraq, Kosovo and Afghanistan. Winner of numerous awards and distinctions, Céline Galipeau will share her experience on the front lines of conflict as part of a special human rights day at Congress.

Graham Smart, Elucidating Climate Change Argumentation: A discourse-analytic approach

Canadian Association for the Study of Discourse and Writing
Sat May 29, 2010 from 09:00 to 10:00
Faubourg Building, room B060
Current arguments over climate change typically combine multiple discourses and meanings — including those of science, economics, social justice, and governance — making these arguments difficult to understand and assess. Graham Smart employs constructivist discourse analysis in attempting to render climate-change argumentation more readily comprehensible.

John Coveney, Bodies of Knowledge, and Knowledge of Bodies: The pleasure and anxiety of eating
Canadian Association of Food Studies, Dieticians of Canada
Sat May 29, 2010 from 17:30 to 19:00
Location TBA
By connecting historical, cultural, scientific, and political approaches to food, John Coveney (Flinders University) bridges the interests of scholars from multiple disciplines. Drawing from his recent book, Food, Morals and Meaning, Coveney explores the historical tensions concerning food, eating and appetite that are now embedded in Western cultures

Montréal Onscreen
J.W. McConnell Building, room de Sève Theatre, LB125
Spanning seven decades of filmmaking (from 1934 to 2008), this intimate festival of films about Montreal promises something for everyone. Twelve films screened over five two-hour screenings will reflect the richness of our urban environment, its history, and the passions of its inhabitants. Curated by Concordia Research Chair in the Mel Hoppenheim School of Cinema professor Thomas Waugh and MA Film Studies student Marcin Wisniewski, organizers are looking forward to presenting a diversity of voice, cultures and film genres on the screen of Concordia's de Sève Cinema. In addition, many directors will be present to introduce their work and answer questions.

Saturday May 29, 2010 19:30, de Sève Cinema
•La mémoire des anges (Luc Bourdon, 2008, 80 min.)
•Little Burgundy (Bonnie Sherr Klein & Maurice Bulbulian, 1969, 30 min.)
•The Street (Caroline Leaf, 1976, 10 min.)

Sunday May 30, 2010 19:30, de Sève Cinema
•Discordia (Ben Addelman & Samir Mallal, 2004, 69 min.)
•Journal inachevé (Marilú Mallet, 1982, 50 min.)

Monday May 31, 2010 19:30, de Séve Cinema
•Montréal vu par (Denys Arcand, André Brassard, Michel Brault, Léa Pool, Atom Egoyan, Jacques Leduc, Patricia Rozema, 1991, 125 min.)

Tuesday June 1, 2010 19:30, de Sève Cinema
•Il était un fois dans l'Est (André Brassard, 1974, 101 min.)
•Fannie dans le temps (Miriam Ginestier, 2005, 10 min.)
•Comédie (Nelson Henricks, 1994, 7 min.)
Thursday June 3, 2010 19:30, de Sève Cinema
•Le confort et l'indifférence (Denys Arcand, 1991, 89 min.)
•Rhapsody in two languages (Gordon Sparling, 1934, 10 min.)
•The Devil's Toy (Claude Jutra, 1966, 15 min.)

Hardy Grant, Kenneth O. May Lecture — Mathematics and the Liberal Arts: The beginning
Canadian Society for the History and Philosophy of Mathematics
Sun May 30, 2010 from 14:00 to 15:00
Faubourg Building, room B040
Noted historian of mathematics Hardy Grant traces the considerable presence of mathematics in the ancient and medieval liberal-arts tradition, and considers what aspects of that legacy remain vital in our time

Sociological Engagements with Science
Canadian Sociological Association
CSA041 Tuesday/Mardi June/Juin 01 09:00 – Room TBA
Chairs: Martin French and Christopher Canning
Writing during the Second World War, the eminent sociologist Robert K. Merton provided an enduring sketch of the "ethos of science". Modern science, he suggested, is characterized by four sets of normative, institutional imperatives: universalism; communism (of method); disinterestedness; and organized skepticism. By the end of the 20th century, however, many argued that science had fundamentally changed, and that it was no longer possible (if it ever had been) to think of science in terms of the norms identified by Merton.
Contemporary science, in its multiple and variegated layers of complexity, confronts sociologists with both a wealth of knowledge, and a host of analytic challenges. We invite 250 word abstracts for papers, to be presented on a panel at the 2010 Canadian Sociological Association Annual Meeting, May 31-June 4 in Montreal, on sociological engagements with science. Which contemporary scientific questions have the greatest sociological significance, and why? How can sociologists best study, and/or engage with, contemporary science? How are sociologists engaging and/or collaborating

with scientists themselves? What new ontological, epistemological, and methodological challenges is this collaboration presenting sociologists of science?

Sociology of Climate Science
Canadian Sociological Association
CSA042 Tuesday/Mardi – June/Juin 01 10:45 Room TBA
Organiser: Myra J. Hird, hirdm@queensu.ca
Chair: Mark Vardy, mark.vardy@queensu.ca
The phenomenon of climate change can be approached through many different sociological lenses, including theories of governmentality, risk, indigenous knowledges and mass media. The purpose of this panel is to engage with the science of climate change, which appears necessary if for no other reason than to apprehend the materiality of those aspects of the earth-system that, to use Latour's phrase, might "strike back." But how should science be engaged? How should scientific knowledge be incorporated into political practice without eliding either scientific insights or the need to maintain inclusive ways of framing the meaning of climate change? What are the social and political implications of the science of climate change, and on what ontological and epistemological bases can such claims be made? This panel welcomes papers that engage theoretically or empirically with these or any other issues related to the sociology of climate science.

- ANNOUNCEMENTS -

Canadian Conferences and Workshops

Conceptions of Empirical Success: Historical and Philosophical Perspectives
London, 7-8 May 2010

Bill Harper's work on Newton, and his work on scientific methodology, illustrate the ways in which history and philosophy of science can be mutually illuminating. This conference brings together some leading historians and philosophers of science to continue the dialogue, and to honour Bill on the occasion of his retirement.

Speakers: John Earman, Allan Gibbard, Alan Hájek, James Joyce, Rhonda Martens, Brian Skyrms, George Smith, and Curtis Wilson.

The conference will be followed on May 9 by a graduate student conference on Logic, Mathematics, and Physics. There is no attendance fee. However, since we'd like to have an idea of numbers, please e-mail Wayne Myrvold (wmyrvold@uwo.ca) if you plan to attend.
For more information visit:
http://tiny.cc/ZqeOc for programme

Models and Simulations 4
Toronto, 7-9 May 2010
The Institute for the History and Philosophy of Science and Technology at the University of Toronto is delighted to be hosting Models and Simulations 4. This is the fourth in a series international conferences examining the nature and use of scientific models and simulations across the natural and social sciences, following excellent meetings in Paris (2006, MS1), Tilburg (2007, MS2), and Charlottesville (2009, MS3).
Scientific models and computer simulations play numerous roles in the sciences, but as a class of tools for use in the articulation of theory, experiment, technological design and application, and prognostication for purposes of public policy, they have only relatively recently come under systematic scrutiny by the community of scholars in history and philosophy of science. The conference aims to raise and investigate important questions about the methodology of practices of modelling and computer simulation, providing a forum for ongoing debates and new angles of approach, on such topics as: how models and simulations are constructed; how they are confirmed; how they may be understood to represent and explain worldly phenomena; how they function in cutting-edge research; and how they influence decision making in the arena of public policy.

For more information see our website: http://www.hps.utoronto.ca/ms4/

Experiments of the Mind: A Summer Institute on Scientific Thought Experiments
Halifax, 16-17 June, 2010

Whether it be Newton and his bucket experiment, Darwin and his "just-so" stories, or Heisenberg and his microscope, thought experiments—i.e., the experiments played out in the imagination—have played a major role in the discovery and justification of some of our most revolutionary theories. This stands as fascinating challenge to the belief in the necessity of empirical data to support scientific theories and it forces us to reconsider the role of experiment in the empirical sciences.
How are thought experiments substantially different from empirical experiments? What is their role in science? Can they tell us anything about the world? Are they giving us a special intuition of the laws of nature? Are they models? Are they akin to literary works? Graduate students and junior scholars are invited to explore and discuss such questions with

five researchers who have shaped the literature on thought experiments and aesthetics in science: Roy Sorensen, Nancy Nersessian, James McAllister, David Davies, and James R. Brown.

While this summer institute is a comprehensive two-day event, all participants will be welcome to attend, as observers, the June 18-19 workshop Science without Data: The Role of Thought Experiments in Empirical Investigations. This two-day workshop brings together close to 20 philosophers, historians, anthropologists, and scientists for a discussion of their most recent work on the topic.

Dates: Summer Institute: June 16 and 17, 2010 (Summer Institute participants are welcome to attend, as observers, the workshop Science without Data: The Role of Thought Experiments in Empirical Investigations, that will be held on June 18 and 19 at Dalhousie University (there is no workshop fee, but we are unfortunately unable at this time to pay for observers meals or travel expenses).)

Location: University of King's College/Dalhousie University, Halifax, NS, Canada
Organizers: Dr. Mélanie Frappier, University of King's College
Dr. Letitia Meynell, Dalhousie University

Application deadline: May 1, 2010

Application procedure
The programme will consist in a series of seminars exploring the literature on thought experiments. The Institute is designed for graduate students, post-doctoral researchers, and faculty members interested, but new to the research on thought experiments.

To apply, participants need to send a short CV or letter of interest to the melanie.frappier@ukings.ns.ca. Students interested in presenting and discussing their own work on thought experiments during the Summer Institute are encouraged to send, along with their application, a copy of their work (or a long abstract) for review.
A notice of admission, together with a detailed syllabus, will be sent to successful applicants by May 10, 2010.

The programme fee is CDN $300 and includes tuition, coffee breaks and lunches on the two days of the Summer Institute. Affordable accommodations will be available at the residences of Dalhousie University. Payments should be made by cheque or money order upon admission to the programme.

For further information please contact:
Mélanie Frappier (melanie.frappier@ukings.ns.ca)

Objectivity in Science Conference, University of British Columbia
Vancouver, 17-20 June 2010
Over the past two decades questions have arisen regarding the objectivity of specific projects in or fields of science: for example, can we trust medical research when it is funded by pharmaceutical companies? Or, whose research in climate science meets the standards of scientific objectivity? Such questions have become important in framing public debate about science and science policy. At the same time, the objectivity of science has become an increasingly important topic among historians and philosophers of science as well as researchers in other fields in science and technology studies (STS) such as sociology of science, rhetoric of science, and cultural studies of science. This conference seeks to advance scholarly perspectives on the objectivity of science by bringing them into conversation with one another. The conference also asks whether and how such scholarly perspectives on objectivity might or should inform public debate. The conference will investigate, moreover, how the specific concerns of scientists, science policy experts, science journalists, and other groups might be made more salient in the research of the STS community.

The goal of this conference, thus, is to provide a forum for STS researchers of diverse disciplinary backgrounds, practicing scientists, and other researchers to discuss and debate issues concerning the nature of objectivity in science. A particular concern will be to discuss how, when, and why questions of objectivity arise within science, in science policy debates, and in public engagement with science. In addition to conference sessions held during the day, this conference will feature two evening panel discussions, open to the public and focused on particular areas of research wherein the issue of scientific objectivity is particularly salient. The public panel discussions will focus on questions of objectivity in collaborative aboriginal research and in research on harm reduction.

For more information, see our website: http://conferences.arts.ubc.ca/objectivity/

Feminism, Science, and Values
London, 25-28 June, 2010
In June 2010, the International Association of Women Philosophers (http://www.iaph-philo.org/) will be meeting at The University of Western Ontario. This will be the organization's first meeting in Canada and only its second

meeting in North America. Co-hosted by the Rotman Institute for Science and Values and the Department of Philosophy, the conference is scheduled to take place just before the international conference on science and values organized by the Rotman Institute.

More information will be available about the conference on our website, http:www.uwo.ca/iaph2010

Reading Artifacts: Summer Institute in Material Culture Research
Presented by: Canada Science and Technology Museum Corporation Collection and Research Branch and Conservation Services
Ottawa, 16-20 August, 2010

Our 2009 Summer Institute broke new ground on how to approach history through intense study of artefacts by bringing together international experts and participants from diverse backgrounds. This years SI will use the same formula to expose you to a unique experience and change the way you appreciate objects and material culture.

Participants will:
• investigate artifacts, trade literature and photographic collections as resources for research, teaching, and the public presentation of history
• work with leading collection scholars in a national museum setting to explore material culture methodologies and approaches
• use artifacts as the centre of discussion and hands-on activities
• immerse themselves in a material culture perspective of the technological past
• learn the basics of conservation, cataloguing and developing collections in local environments – a growing and essential resource for history studies.
Location: Canada Science and Technology Museum, Ottawa, Ontario, Canada
Dates: August 16-20, 2010 Registration due: June 16, 2010.
Registration: https://nt8.magma.ca/technomuses.ca/summerinstitute/summerinstitute_e.asp
Tuition: Students $250. Post-Docs $350; Faculty and Professionals $450 (includes breaks, lunches, and a field trip)
Some financial support for students may be provided.

Visit the website at http://www.sciencetech.technomuses.ca/english/whatson/2010-reading-artifacts.cfm

Circulating Knowledge, East and West
Inspired by Dalhousie University's online launch of their Dinwiddie Archives, this conference aims to further international dialogue and scholarly exchange between those working on the history of science in Asia, Europe and North America by examining the global circulation of scientific knowledge from the Early Modern Period to today.

Circulating Knowledge, East and West will culminate in a half-day facilitation workshop to plan for further Science Studies dialogue and exchange, "East" and "West", with future conferences in Bangalore and Singapore.

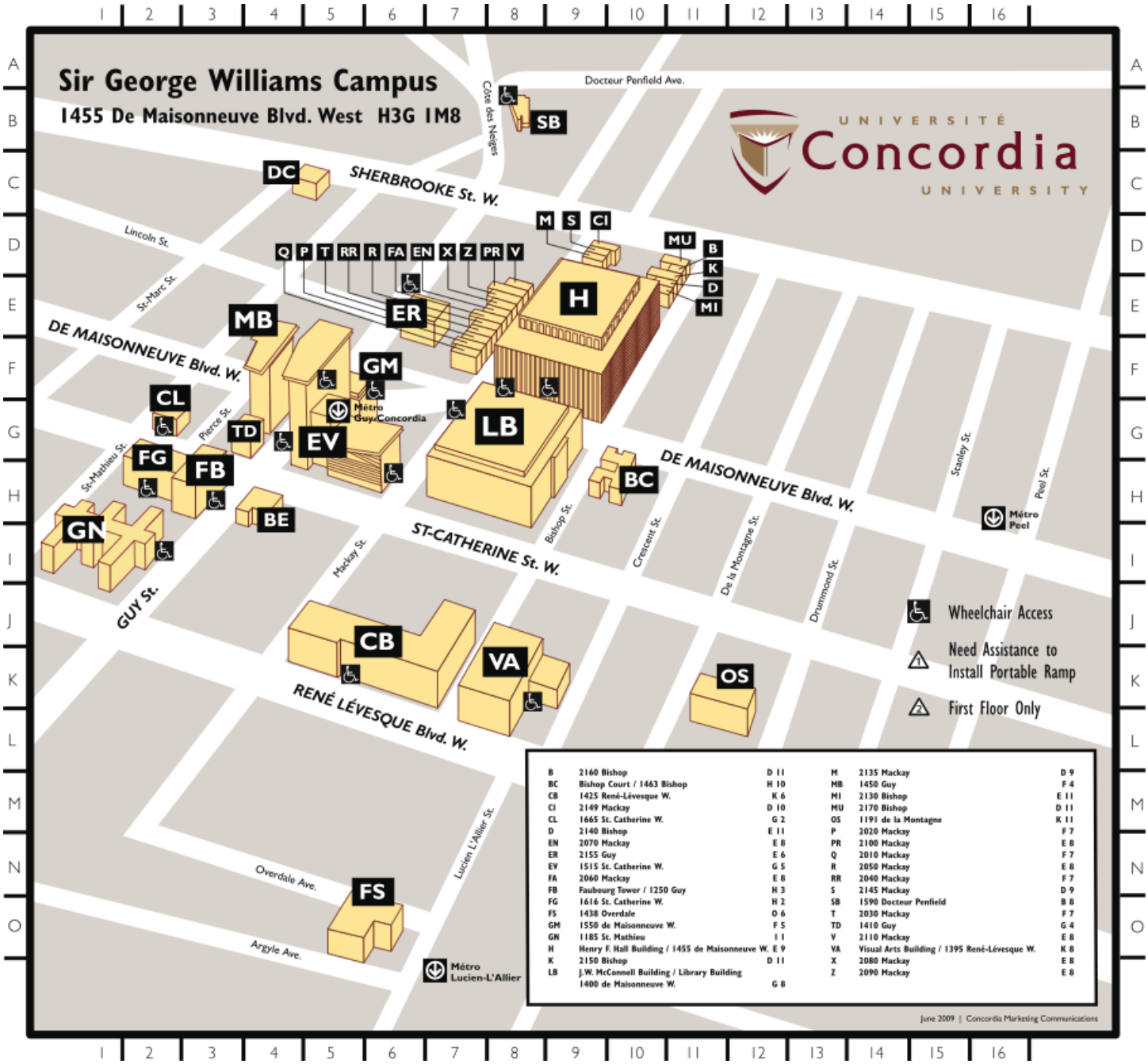
SPEAKERS INCLUDE:

Fa-ti Fan, State University of New York at Binghamton
Yves Gingras, l'Université du Québec à Montréal (UQAM)
Jan Golinski, University of New Hampshire
Jahnavi Phalkey, Imperial College London
Dhruv Raina, Jawaharlal Nehru University
Kapil Raj, École des Hautes Études en Sciences Sociales
Sundar Sarukkai, Manipal University
Jon Topham, University of Leeds

CIRCULATING KNOWLEDGE, EAST AND WEST will be held at the University of King's College, Halifax, Nova Scotia, Canada from July 21-23. For further details - including information on registration and accommodations - please visit: WWW.SITUSCI.CA.

Sponsored by the Situating Science Knowledge Cluster, the Social Sciences and Humanities Research Council, the Shastri Indo-Canadian Institute and the Centre for Interdisciplinary Research (University of King's College)

The James Dinwiddie (1746-1815) papers were donated to the Dalhousie University Archives in 1999 and are now being prepared for online access. Dr. Dinwiddie (1746-1815) was the scientific attaché of the first British embassy to the 18th Century Chinese imperial court, and the first Professor of Mathematics, Natural Philosophy and Chemistry at the College of Fort William in Calcutta, India. One of the most important of the new itinerant Newtonian natural philosophers and lecturers of the Early Modern Period, the bulk of Dinwiddie's papers consist of his scientific observations, experiments, lecture notes, and journals with dates ranging from 1767 to 1815.



CHSPS/SCHPS Location: MB (John Molson School of Business) 1450 Rue Guy