Day I. Monday, May 28 / Jour I. Lundi, 28 Mai

9:00–10:30 Session/Séance I.1

I.1A Enlightenment Science
Physic 107
Chair/Président: Ian Hesketh

9:00-9:30
Victor D. Boantza (Toronto)
Pneumatic Transmutations, the Constitution of Air(s), and Phlogiston's Last Hurrah

Contrary to common belief, Lavoisier's greatest phlogistic rival was not Joseph Priestley but Richard Kirwan, a fact which was explicitly recognized by both the Lavoisians (Berthollet, Fourcroy, Morveau, Monge) and by Priestley himself. Throughout the 1780s, which saw the unprecedented rise of pneumatic chemistry, Kirwan's phlogiston-as-inflammable-air theory was the most dominant alternative to Lavoisier's oxygen theory of combustion and acidity. The debate culminated with Kirwan's 1787 publication of his *Essay on Phlogiston and the Constitution of Acids*, which was commented upon by the Lavoisians and republished with Kirwan's final remarks in 1789. This paper offers a genealogical contextualization of Kirwan's phlogistic stance, uncovering how traditional Stahlian phlogistic explanations have been variously appropriated by pneumatic practitioners in the 1770s, resulting in fierce metaphysical conflicts over the constitution of airs and the role of phlogiston in pneumatic processes. In this context, particular attention is paid to the contributions of Priestley, Scheele, Bergman and Fontana, all of whom shared the perception that different airs corresponded to various degrees of phlogistication of a generic pneumatic entity, implicitly endorsing 'aerial transmutation'. Kirwan's phlogistic version emerged against the background of these disputes as a unique synthesis between Priestley's aerial transmutational sequence and Adair Crawford's 1779 innovative formulation of the relation between air, heat and phlogiston. On this account, phlogiston was not overthrown by Lavoisier's quantitative interpretation of combustion but was displaced by arguments concerning the constitution of airs. This is further substantiated by illustrating how an intriguing pneumatic compositional paradox precipitated the demise of Kirwan's phlogiston.

9:30-10:00
Erich Weidenhammer (Halifax)
Late Eighteenth-Century English Approaches to Health and the Environment

My Master's thesis is focused on the medical qualities attributed to natural features of the British landscape from the early 1770s until the end of the century. This project will produce a metaphorical map of areas then considered medically significant. These will include swamps, forests, urban areas, mineral springs, and the seaside. I will use the career of the English aerial philosopher Joseph Priestley (1733-1804) as a departure point and a guide. Two technologies, first introduced in his earliest publications on pneumatic

chemistry, demonstrate that Priestley was prominent among late-Enlightenment natural philosophers concerned with explaining long-standing medical ideas relating health to the environment. Priestley's "nitrous air test" and his device for impregnating water with fixed air (CO_2) both drew on contemporary medical ideas, notably those of his prominent medical contemporary John Pringle (1707-1782), which ascribed medical significance to natural processes taking place in nature. Both technologies were highly influential upon physicians and natural philosophers with similar concerns.

While late eighteenth-century medical reformers concerned with managing the environment were not as influential as their successors, their efforts nevertheless tend to share a number of interesting characteristics. Themes like the importation of scientific instruments into medicine, the manifestation of late-Enlightenment views on the human relationship to the natural world, and early efforts to systematically sample environmental conditions relative to location, give these efforts a collective consistency and coherence. My conference presentation will be an abridgement of my Master's thesis research paper.

10:00-10:30
Ernst Hamm (York)
Outsiders, Insiders and Science in the Dutch Enlightenment

The contributions of religious dissenters to the growth of science and technology in the seventeenth, eighteenth and nineteenth centuries has been much documented and debated for well over half a century in the history and social relations of science. The large literature in this field of study has attended almost exclusively to the Anglo-Atlantic world, especially England. Yet Protestant dissenters were also to be found in other places, most notably the Netherlands—the most tolerant, democratic and commercial nation in Europe. And, as a growing body of literature is making clear, the Netherlands was also of central importance for the growth of seventeenth-century science (e.g. Harold Cook,, *Matters of Exchange*, 2007) and was the centre of the European Enlightenment (Jonathan Israel, *Radical Enlightenment*, 2001, and *Enlightenment Contested*, 2006). My paper will consider Mennonites, a group of Dutch dissenters, who were unusually active in the promotion of natural philosophy, natural history and experimental philosophy. Their participation in scientific societies and their promotion of scientific publishing were far from inconsequential, and, as my paper will argue, a closer consideration of the place of Mennonites in eighteenth-century Dutch science and society will shed light on the more general character of the Dutch Enlightenment.

I.1 B Holism, Confirmation and Scientific Theories
Physic 130
Chair/Président: Micheal McEwan

9:00–9:30
Joe Morrison (Sheffield)
Three Holistic Theses

Evidential holism is generally regarded with suspicion, if not overt scepticism, in the philosophy of science, and yet it is regularly invoked in prominent debates in epistemology and metaphysics. Attempts to resolve this disparity of popularity face the additional problem that there is no univocal statement of holism's central claims or its provenance. I propose that we distinguish three holistic theses: holism about prediction, holism about falsification, and holism about confirmation. Recent debates in the philosophy of science show that holism about confirmation is highly controversial. Nevertheless, it's this highly controversial thesis that has been recently defended on the grounds that scientific observation is theory-laden. I maintain that the theory-ladenness of observation only motivates the claims about prediction and falsification. I argue that holism about confirmation will only follow if we rely upon an outmoded and untenable principle of hypothetico-deductivism, which equates confirming instances with deductive consequences. As such, this recent argument fails to imply confirmational holism, but does serve to motivate a narrower, less controversial version of evidential holism than that which is usually discussed by philosophers of science.

9:30–10:00
Michael J. DeMoor (Amsterdam)
Holism and the Constitutive A Priori: Hegel, Friedman and Quine

In a paper given at last year's Congress, I argued that, contrary to Michael Friedman's arguments, there is no direct conflict between Quine's semantic holism and Friedman's proposal that a relativised version of the constitutive a priori is necessary for understanding scientific theories. However, showing that the two are *not in*compatible on the semantic level does not address what appears to be a remaining incompatibility: viz. that Quine's holism is of a piece with a kind of naturalism (presented particularly in "Epistemology Naturalized") that Friedman (rightly, in my view) rejects. The current paper attempts to demonstrate that a version of the kind of "differentiated holism" that Quine advocates can not only be divorced from his philosophical naturalism, but also include and account for Friedman's own views on the (communicative) rationality of scientific revolutions, which lead him to reject Quinean naturalism. It is argued that Hegel's dialectic of Concept and Object (as outlined and developed in the *Phenomenology of Spirit*) is simultaneously a version of differentiated holism and a non-reductive account of the rationality of the development of scientific theories. Viewed *synchronically*, then, Hegel's dialectic resembles Quine's holism (at least in certain crucial respects); viewed *diachronically*, it resembles Friedman's account of the communicative rationality of scientific revolutions. Insofar as Hegel's account of science is coherent, its actuality demonstrates the possibility of a simultaneously holistic and "neo-Kantian" philosophy of science.

10:00-10:30
Jan Sprenger (LSE)
A confirmation-theoretic guide to explanation

Accounts of scientific explanation can roughly be divided into two groups: the unificationist and the causal-mechanistic accounts. Both have their merits, but they are

also incomplete: they do not provide a quantitative measure of explanatory power. Thus they fail to give a decision rule for competing explanations, a gap that I would like to close. Therefore I develop a confirmation-theoretic model of explanation where entire theories are taken as explanantia. Proper explanantia have to (i) *make the explanandum more likely*, to (ii) *receive considerable independent confirmation* and to (iii) *change the syntactical description of the explanandum*. The first condition ensures the relevance of the explanans for the explanandum. The second condition rules out spurious, purely adaptive explanantia with poor predictive power. Finally, the third condition ensures that a new light is shed on the explanandum by a change of description (usually by abstraction, generalization or connection to other theoretical levels). This, I argue, generates scientific understanding. Then I propose a measure for the degree of explanatory power that builds on the literature on confirmation measures. The suggested measure balances structural virtues of the explanans against explanandum-relative relevance and the degree of total explanandum-independent confirmation. I argue further that the confirmation-theoretic account of explanation is not only more precise, but also more comprehensive than its predecessors: By means of the above trade-off we are able to reconstruct unificationist *and* causal-mechanistic explanations.

10:45–12:45 Session/Séance I.2

I.2A Science, Art and Modernity on Both Sides of the Rhine
Physic 107

Chair/Président: Joan Steigerwald

10:45–11:15
Sofie Lachapelle (Guelph)
The Great Magician as Ambassador of Science: Robert-Houdin on the Stage in France and Algeria (1845-1871)

Jean-Eugène Robert-Houdin (1805-1871) is remembered today as a pioneer of modern magic. In the famous Paris theatre bearing his name, he presented his audience with performances in which theatrics, physical and chemical experiments, trickery, and illusions were combined and accompanied by an entertaining commentary. Many nineteenth-century magicians performed at the Théâtre Robert-Houdin. They called themselves professors of amusing physics and blended simple scientific experiments and illusions in their shows. While the theatre thrived until the Great War, Robert-Houdin retired in 1852. Away from Paris, in his family home in Blois, he concentrated on his clocks, electrical inventions and ophthalmic instruments. Four years later, he was called out of retirement to perform once more, this time in Algeria. Summoned by the French government with the purpose of convincing the Algerian population that French science was superior to the magic of their holy men, Robert-Houdin took the stage one last time. Robert-Houdin toured Algeria, presenting his show as an ambassador of science in the name of France. This paper will explore Robert-Houdin's vision of experimental magic

as presented in his writings and the ways in which science and magic were combined in his stage performances.

11:15–11:45
Robert M. Brain (British Columbia)
Physiognomies of the Accused: Graphology, Psycho-Physiology and Battle of Images in the Dreyfus Affair

The rivers of ink that have flowed concerning the Dreyfus Affair contain scant reference to the central issue of its judicial component: the debate over the infamous bordereau, a small handwritten document containing military secrets, alleged to have been written by Captain Alfred Dreyfus. The judicial arguments over the bordereau quickly spilled over into larger areas of public debate raised by the handwriting itself. Questions of rules and procedures of evidence quickly expanded into a range of larger issues concerning the nature of expertise in handwriting, the science and art of graphology, of physiological inscription, the reliability of photography, and finally, the nature of expertise itself.

This contribution will examine the battle of the experts over the meaning and interpretability of the bordereau: the initial testimony of the criminologist Alphonse Bertillon, which attempted opportunistically to adapt handwriting to his system of criminal identification, the testimony of international and supposedly impartial graphologists, the attempts of experimental psychologists such as Alfred Binet to treat gestures of the hand as a psycho-physiological inscription revealing identity and character. At the centre of these discussions swirled some of the most vexing questions of fin de siècle psycho-physiology and forensic psychiatry, involving psychological automatism and the nature of the will. These were also key questions in contemporary art and aesthetics, leading to public discussions involving leading artists and scientists on the question of graphology.

It will be argued that beyond the broader political stakes of the trial, concepts of method, disciplinary prestige, and the authority of scientific expertise stood at the center of these debates. These disputes, moreover, participated in the more general "battle of images" and competing forms of visuality which swirled through the acrimonious political crisis of the larger Affair.

11:45–12:15
Cornelius Borck (McGill)
Montage: Stitching Science, Art, and Work together in Weimar Germany

Montage, the strategy to cut and reassemble more efficient units, unfolds as a fascinatingly rich field when analyzing Weimar Germany, where Taylorization was introduced at large scale, for example, and complemented by psychological testing and vocational training, in an effort to increase industrial efficiency scientifically by optimizing the use of human and non-human resources. The emblematic representation of this new and often ruthless way of integrating human beings into industrial modernity was the prothesentraeger, the man with a prosthesis which "allowed" him to work.

Ubiquitous because of WWI, the prothesentraeger signaled a hybridization of human beings with machines, if not their mutual transformation. During the same period, montage became recognized as the formative principle in film and also emerged, in photomontage, as a new visual medium in its own right. Here, montage's ambiguity is particularly prominent, and photomontage was embraced as a critical weapon while being used simultaneously to celebrate technological modernity. And finally, montage was already heavily theorized during this period, when critical observers, from Kracauer and Brecht to Benjamin, noticed the reshaping of human beings to the needs of modernity—and speculated about a transformative power of machine man.

12 :15-12 :45
Oliver A. I. Botar (Manitoba)
The Biocentric Bauhaus

I have argued elsewhere that Biocentric (nature-centric) world views played an important role in the development of 20th century art. In light of this, the history of Modernism, and therefore of the Bauhaus, is in need of revision. The standard view of the Bauhaus as having been a hyper- rationalist institution obsessed with technology and somehow placed in opposition to "nature" is in dire need of revision. In this paper I will undertake a re-examination of the Bauhaus under its first director Walter Gropius (1919-1928), and examine the ways in which Biocentric discourses themselves felt at the school during that period.

I.2B Laws of Nature
Physic 130
Chair/Président: Anjan Chakravartty

10:45–11:25
J. J. MacIntosh (Calgary)
Robert Boyle on Laws of Nature

It is hard not to be anachronistic when we read Boyle on laws of nature, but a close attention to the texts suggest that Boyle did not understand by "law of nature" precisely what contemporary philosophers do (not that *that* is always clear, either). Boyle held that the "law" in "law of nature" is misleadingly metaphorical, while "nature," equally, is misleadingly ambiguous, and often qualified his use of the term "law." For Boyle, "what ever be the *cause* of motion, the *Laws* of the Communication of motion, cannot reasonably be supposed to proceed from the particular nature or any compact of the body's of Corpuscles themselves; but, to be certaine and regular, must have been instituted by an Intelligent and powerfull Being." In this paper I investigate the question of what Boyle understood a law of nature to be and how, exactly, he believed God to have inaugurated, and to be sustaining, his system of laws. As a coda I discuss Boyle's account of the discovery of "Boyle's Law," in which he outlines an experimental result without any suggestion that such a result constitutes a *law*.

11:25–12:05
Travis Dumsday (Calgary)
Aquinas on the Laws of Nature

In her influential 1986 paper, "The Origins of Scientific 'Law,'" Ruby argues that according to Thomas Aquinas one can speak of a 'law' of nature in only a metaphorical sense. Nature does not really participate in laws. Ruby's interpretation of Aquinas continues to impact discussions in the philosophy of science. In his 2004 book *Laws in Nature*, Mumford argues that the whole notion of a 'law' of nature is radically flawed and ought to be dispensed with. He mentions Aquinas approvingly as an historical figure who held much the same view, taking 'law' to be purely metaphorical, and cites Ruby as the source of his interpretation. But the interpretation is false, for Aquinas believed that laws really do apply to nature. I demonstrate this through an analysis of the relevant texts, and explain how the misinterpretation arose. I conclude by briefly discussing where Aquinas' theory fits in with the current literature on laws of nature.

12:05–12:45
Kirk McDermid (Montclair State)
Physicalism and Miracles

Miracles and natural laws are conventionally supposed to be inherently in metaphysical conflict, notwithstanding epistemological 'reconciliations' of the Humean variety. Assessing the metaphysical compatibility between miracles and natural laws is often prejudged by either adopting or rejecting a strong physicalist thesis (independent of this issue, or otherwise.) The operative component of physicalism involved is a causal closure principle: that every caused event is a physically caused event. If physicalism and this strong causal closure principle are accepted, then supernatural interventions are ruled out *tout court*, while rejecting physicalism gives miracles metaphysical *carte blanche*. I argue that this is a false dichotomy; a more moderate version of physicalism exists that respects important physicalist intuitions about causal closure, while allowing for miracles' logical possibility. I then (briefly) propose two separate mechanisms, suitable for deterministic and indeterministic worlds, respectively, that do conform to this more moderate physicalism.

12:45–14:00 Lunch/Dîner

14:00–15:30 Session/Séance I.3

I.3A Joint Session with ACCUTE
"Adaptation: The Plenary, Not the Movie" — Three Perspectives on Adaptationism: Literary, Biological and Philosophical
THORV 271

Chair/Président: Steven Bruhm

Recently, the notion of "adaptation" has begun to leak beyond the specialised and heated debates over functionalism, adaptationism and pluralism in biology, and into wider literary and cultural domains. From Spike Jonze's award winning 2002 *Adaptation* to noted Canadian literary theorist Linda Hutcheon's exploration of the nature of literary adaptation in her *A Theory of Adaptation* (2006), adaptation threatens to resurge as a theoretically useful device across the disciplines. This plenary session will explore the nature and various dangers in cross-border adaptations of adaptation.

Linda Hutcheon (Toronto)
Recent Literary Interest in Adaptation

Gary Bortolotti (Saskatchewan)
Parallels and Homologies with Evolutionary Biology

Gordon McOuat (King's)
Adapting the Adaptationish Project — Very Dangerously, Mind You

I.3B Philosophy of Mathematics
Physic 130
Chair/Président: Lawrence Deck

14:00-14:30
María Ponte Azcárate (Brown)
Naturalism, mathematics and its applications to science: The role of the indispensability argument

One of the most sticking features of mathematics is its applications to empirical science and precisely from this Quine and Putnam derived what it is considered the best (if not the only) argument for the existence of mathematical entities: the indispensability argument. This argument has been largely discussed in the literature. In this work, we aim to analyse one particular form of criticism, mainly developed by Penelope Maddy, and some possible responses to it, mainly championed by Mark Colyvan.

The main point will be, on the one hand, to analyse whether the argument of indispensability of mathematical entities is coherent with the daily practice of both scientists and mathematicians. And especially, whether the claim that the unapplied parts of mathematics can be coherently considered as "recreational" (as Quine suggested we do). On the other hand, we will outline the implications of these reflections for a defence of naturalism in mathematics.

We will conclude that the indispensability argument doesn't stand these criticisms but that, contrary to what it is usually believed, this negative fact does not represent an important objection for naturalist proposals in mathematics nor for realist's options.

14:30-15:00
Makmiller Pedroso (Calgary)
Sets, Categories, and the Foundation of Mathematics

Because sets are so important for the explaining of mathematical concepts, some mathematicians and philosophers contend that Set Theory is the foundation of mathematics [Maddy 1997, Mayberry 1994]. Like Set Theory, Category Theory is a mathematical theory that can be used to define many notions employed in mathematics without relying on set-theoretic concepts. Because of this, some mathematicians and philosophers propose that Category Theory can be seen as an alternative foundation of mathematics [Awodey 1996, Landry 2006]. Nevertheless, Feferman [1977] argues that: (1) a theory cannot be a foundation of mathematics without providing the notions of operation and collection; (2) Category Theory cannot provide an account for the notions of operation and collection. As opposed to Feferman, I will propose that the distinction between operation and collection is not clear-cut, and that the thesis (2) is false. In order to defend my view, I also intend to discuss what constitutes a foundation for mathematics.

15:00-15:30
Jean-Philippe Villeneuve (Montréal)
From Cauchy's integral to Lebesgue's integral axiomatization: When a new interpretation becomes a reinterpretation

The purpose of this talk is to review some of the most important 19th century research on the mathematical notion of the integral to shine the spotlight on two important processes: the new interpretation and the reinterpretation of a mathematical notion. What differenciates these processes is that, in the first process, the same way of defining the initial notion is kept; in the second, it has completely changed. Note that these processes can be linked to the generalization and abstraction processes, but this is fodder for another talk.

To illustrate that, we will discuss Cauchy's integral (1823) on continuous functions defined as the limit of the Cauchy sums. We will then look at Riemann (posthume 1867) and Darboux (1875) who reinterpret the Cauchy integral by introducing a new way of defining the integral: the equality between the upper and lower integrals. We will also present Jordan's new interpretation (1892) of both integrals within the context of $\mathbf{R}^n$ and the new notion he had to develop: the measure.

Finally we will see how Lebesgue (1902) reinterprets the integral by axiomatizing it and, using step functions, introduces a new way of defining the integral on bounded functions. With his axiomatization, the Riemann integral is no longer an integral, but the Cauchy integral on continuous functions keeps this title.

15:45–17:15 Session/Séance I.4

I.4 A Methodologies in Experimental Psychology and Neuro-Physiology Physic 107

Chair/Président: Tara Abraham

15:45–16:15
Kenton Kroker (York)
Insomnia in the laboratory and the clinic

Insomnia has long been recognized as a pervasive phenomenon, but it was not until the early twentieth century that investigators began to translate this experience into what eventually emerged as the core of an experimental and clinical analysis of sleep. The most prominent of these attempts took shape during the late 1960s, as American investigators began to frame the problem of insomnia by applying techniques and technologies borrowed from the laboratory-based study of dreaming. Although this attempt ultimately failed to clearly define insomnia as a specific disease or family of diseases, it did help create sleep disorders as one of the most prominent public health issues in western society. It effectively created the medical specialty of sleep disorders medicine in the processThe history of insomnia, then, provides an example of the differential success a 'biomedical platform' (as described in the book of the same name by Peter Keating and Alberto Cambrosio) can have when confronted by a well-entrenched 'looping' process of self-classification that, according to Ian Hacking, is characteristic of human kinds.

16:15–16:45
Tracy Finn (Waterloo)
Hypothesis Testing in Psychology: An Investigation and Defense

Empirical psychology uses null hypothesis significance testing to test hypotheses and gain information from sample populations. Experimenters follow a falsificationist method to reject the null hypothesis and confirm a hypothesis that predicts a causal relationship between independent and dependent variables. I examine the model of experimenter rationality that psychologists follow during experimental investigations, and discuss the validity of the experimental inference.

I defend the experimental psychologists' experimental inference against two famous criticisms: the existence of subject and experimenter biases, and Paul Meehl's "crud factor." Solutions to the problems these criticisms raise can be found by closely examining the experimental design, and thus I argue that the experimental inference is valid. I suggest that psychologists adopt an error statistical model of empirical investigation developed by Deborah G. Mayo. This will overcome the problems inherent in a falisficationist framework in empirical investigations without necessitating a radical change in experimental designs and practices.

16:45-17:15

Gord Binsted (Saskatchewan)
The Difference Between Observation, Data and Evidence: A Case Study from Physiology

One of the main facets of the general problem of underdetermination (the Duhem-Quine Thesis) is that experimental data are ambiguous with respect to theory choice. (Can data be better evidence for one theory than another, when it is consistent with both?) A related problem is the interpretation of data – how is data extracted from observation, and does this influence the evidential value of that data?

In this talk, we will describe and tentatively assess a current debate in the area of motor control and perception, about the acceptability of the "Perception-Action Model" (PAM) of human visuomotor functioning. The PAM hypothesizes that there are two relatively independent visual systems: one (the dorsal neural pathway) executes movement planning and function, and the other (the ventral pathway) is engaged in perception. The PAM has gained some support from studies of several 'selectively brain-damaged' individuals (those with the blindsight-like visual deficits of agnosis and ataxia), but recently the debate has centered around data gathered from studies involving subjects with neurologically intact visual systems. PAM proponents interpret these experiments as providing substantial support for the model, while critics dispute not only the quality of the evidence but the existence of the data in the first place. There seem to be only two options: justify (somehow) the validity of the data, and its evidential value to the PAM, or admit that the interpretation of the data is dependent upon prior acceptance of PAM, and therefore its evidential value is severely undermined. We will explore what sorts of arguments PAM proponents use against their skeptics and, perhaps, what arguments they should use instead.

I.4B Scientific Representation, Models and Thought Experiments
Physic 130

Chair/Président: Frédéric Bouchard

15:45–16:15
Anjan Chakravartty (Toronto)
Informational versus Inferential Views of Scientific Representation

Scientific theories, models, simulations, and diagrams are generally held to have representational content – they represent target objects and processes in domains of scientific investigation. Such representation is usually characterized in terms of two basic features: intentionality; and information. A representation is something that is *about* a target system, and that *shares* some substantive properties or relations with that system, where sharing is commonly explicated in terms of relations defined precisely in mathematics and logic, such as isomorphism, homomorphism, "partial" isomorphism, etc. (for recent discussions, see Bueno 1997, da Costa & French 2003, and French 2003). Recently, however, several authors have argued that these twin conditions of intentionality and information yield an inadequate analysis of scientific representation.

Missing, they suggest, is a recognition of the role representations play in facilitating scientific purposes (see Hughes 1997, Frigg 2002, Suárez 2003, 2004, Giere 2004). More specifically, they suggest that any acceptable analysis must take into account the role of representations in facilitating inferences or "surrogate reasoning" regarding target systems.

Though no one would dispute the connection between scientific representations and purposes, nor the intrinsically interesting nature of such purposes, one might nevertheless worry that this objection to informational accounts of representation is overblown. For given that scientific inferences are in fact made *on the basis of* representational information, it is arguable that the inferential view adds nothing of substance to informational descriptions of the basic, necessary conditions of representation. In this paper, I use examples to probe the extent to which, as it turns out, such inferences take as premises background knowledge (i.e. information) that is not incorporated into the relevant representations. As a consequence, I suggest, the capacity to facilitate inferences does constitute an interestingly distinct feature of scientific representation after all – one whose genuine novelty has not yet been thoroughly articulated by its proponents.

16:15–16:45
Micheal McEwan (Waterloo)
The Semantic View: Language Independence, Löwenheim-Skolem and Quantum Mechanics

The semantic view of scientific theories, which has been dominant for more that thirty years, is characterized by its focus on models. This focus is claimed by its proponents--especially Bas van Fraassen--to facilitate some measure of 'language independence'. This claim, however, has been challenged by John Worrall (1984) who argues that the semantic approach is equivalent to, or at least complementary to, a syntactic approach. Van Fraassen (1985; 2000) offers an interesting and influential (cf., French and Ladyman (1999)) response to Worrall appealing to the limitative results of mathematical logic. In this paper I evaluate his response and conclude that it rests on two dubious assumptions: (1) that scientific theories are typically presented by 'directly presenting' a class of models; and (2) that the 'direct presentation' of a class of models provides a characterization of a theory which is largely language independent. I then use early quantum mechanics--particularly von Neumann's proof of the equivalence between matrix and wave mechanics--as an example with which to demonstrate that the above assumptions are historically inaccurate and that any semantic approach which is overly focused on models is unable to capture crucial aspects of scientific practice.

16:45-17:15
Mélanie Frappier (Minnesota State)
A Good Play with a Badly Written Third Act: Reflections on the Theoretical Dependence of Thought Experiments

Hacking (1983) has claimed that thought experiments, unlike real experiments, are fixed and immutable, and that their different embodiments are more like retelling old jokes

than repeating or recycling a given experiment if one accepts Radder's taxonomy of experimental reproducibility. Against this view, Bokulich (2001) has argued that the fact that thought experiments can be reanalyzed from different theoretical perspectives suggests that thought experiments do evolve, adapt, are being recycled and retooled. However, as it is based on the reinterpretation of thought experiments, Bokulich's argument is still subject to Hacking's idea that different versions of a thought experiment are akin to the different versions of the same character that arise as various versions of a play are produced. For Hacking, what makes an ordinary experiment truly alive is that it can yield results without being analyzed from any specific theoretical framework.

In this talk, I want to show that some thought experiments actually meet Hacking's criterion, which in turn enables them to be repeated in an innovative manner, and be "alive" in the same way ordinary experiments are. However, I argue that Hacking's claim that real physical experiments, but not their fictive counterparts, can have a "life of their own" is ambiguous and leads to an oversimplified characterization of thought experiments. On this account, Hacking's static view of thought experiments yields an oversimplified reading of thought experiments that disregards important procedural differences between various thought experiments, while artificially building into them interpretative stands that are not necessary to their success.

17:00-19:00 **Department of Philosophy Reception**
PCS Atrium, Commerce Building
All members of CSHPS welcome

Day II. Tuesday, May 29/Jour II. Mardi, 29 Mai

9:00–10:30 Session/Séance II.1

II.1A Ampliative Inference, Causal Prediction and Naturalized Epistemology
Physic 107
Chair/Président: Ben Almassi

9:00–9:30
Ryan Samaroo (Western Ontario)
A Reichenbachian Reply to Earman's Critique of the Gaifman-Snir Theorem

In *Bayes or Bust*, Earman surveys and critiques several probabilistic rules for ampliative inference that appeal to convergence in the limit. In particular, he critiques the Gaifman-Snir theorem, which is subject to the two main problems faced by Reichenbach's limiting frequency rule for induction. The first problem is that of long-run convergence: the

values assigned to our hypotheses may converge in a sequence of data, but the place of convergence may be beyond human observation. The second problem is that of short-run irrelevance: even if a given sequence is convergent, an initially observed subsequence may prove to be irrelevant when the sequence is extended. Earman critiques the Gaifman-Snir theorem because of these two problems.

My purpose is to argue that, in emphasizing the technical limitations of the theorem, Earman misses a fundamental point about induction that Reichenbach captures. I argue that, though the Gaifman-Snir theorem cannot establish the certainty of a hypothesis, it remains a guide to reliable inference, if we adopt Reichenbach's 'practical limit' interpretation, *i.e.*, if we seek convergence in sequence accessible to human observation rather than in a potentially infinite one. I then argue that the irrelevance-of-the-short-run problem can be evaded by a practical or Reichenbachian interpretation of a sequence of data; the problem of an anomalous or non-representative initial subsequence only poses a problem in the blindly mechanical application of some inductive rule. In his pursuit of objectivity (with regard to a scientific inference), I think Earman overlooks the guide to reliable inference that the Gaifman-Snir theorem provides.

9:30–10:00
Erdal Atukeren (KOF Zurich)
On Granger-Causality

This paper takes a closer look at the conceptual grounds of the notion of causality in Granger's (1969, Econometrica) sense. According to Granger, a stochastic variable X is said to cause another distinct stochastic variable Y if, in addition to all other available relevant information, the inclusion of the history of X significantly reduces the forecast error variance of Y. In essence, Granger defines causality in terms of predictability and precedence. This notion of causality is an operational definition. We argue further that the concept of Granger-causality calls for a multivariate framework of analysis. This is because taking all available relevant information into account is indeed required in Granger's definition of causality. This is in line with rational behaviour and learning under imperfect and incomplete information, especially in an open system.

The notion of Granger-causality in a multivariate framework is redefined by Hsiao (1982, Journal of Economic Dynamics & Control). Hsiao introduced the concepts of "direct", "indirect", and "spurious" Granger-causality. According to Hsiao, for a variable X to be a direct cause of another variable Y, X must Granger-cause Y not only in a bivariate setting but also in all multivariate contexts. In essence, the Granger-Hsiao definition of direct-causality should be in line with the "invariance under intervention" argument (Hoover, 2001) or the "faithfulness" concept (Pearl, 2000; Spirtes et al., 2000) for establishing a cause-effect relationship between the variables of interest. Nevertheless, Hsiao's definition of direct-causality has largely been overlooked in the philosophy of science literature. Drawing on Hsiao, we also examine the criteria for establishing the cases of indirect causality and spurious causality. For example, the Granger-Hsiao definition of indirect causality requires "transitivity" between the events. Here, we address the controversial topic of "transitivity" in a causal chain by referring to

the "predictability" requirement in the definition of Granger-causality. We also examine the semantics of the definition of causality in Granger's sense. In particular, we elaborate on the qualifiers "all", "available", and "relevant" information and reconsider their meaning with reference to testing for Granger-Hsiao causal inferences.

Finally, we suggest a framework of analysis which evaluates the results from Granger-Hsiao causality tests within a discrete choice model (such as a probit regression). This framework provides insights into the conditions or characteristics under which, say, X Granger-Hsiao causes Y. We argue that this approach is compatible with some of the arguments made by Nancy Cartwright and Keith Campbell. An empirical illustration of this procedure is also provided [See also, Atukeren (2005, Kyklos).]

10:00-10:30
Tudor Baetu (Montreal)
Co-referential models and the cumulativity of scientific knowledge

If a model *M* derived from theory T_1 is true and a model *M'* from a theory T_2 is true, where truth is reference to empirical reality, then *M* and *M'* are or at very least can be both true irrespective of our ability or inability to conceive or imagine how reality can be at the same time as described by *M* and *M'*. I grant the antirealist that, in principle, it is possible that no model can ever be shown to be true. I reject however the arbitrary claim that absolute unity and conceptual coherence are required for the realist thesis to hold. Although desirable, neither universality, nor unity is necessary in order to have the kind of knowledge science has to offer. First, truth needs not to apply to entire theories. Suffice some models are true. In layman's terms, this means that a theory can be right about some things without being right about everything. Second, assuming that two models are shown to be true, it doesn't follow necessarily that there is something wrong with the method, proof, justification or criteria by the means of which the models in question are shown to be true.

Si un modèle *M* dérivé d'une théorie T_1 est vrai et un modèle *M'* dérivé d'une théorie T_2 est vrai, ou la vérité est définie par rapport à la réalité empirique, alors *M* et *M'* sont ou tout au moins peuvent être tous deux vrais indépendamment de notre habilité ou inhabilité à concevoir comment la réalité peut être telle que décrite par *M* et *M'*. On doit admettre que, en principe, il se peut que aucun modèle ne soit vrai. Il ne s'ensuit pas par contre qu'une absolue unité et cohérence est requise pour que la thèse du réalisme tienne. Bien que désirable, ni l'universalité, ni l'unité ne sont nécessaires au savoir scientifique. Premièrement, la vérité ne concerne pas des théories entières, mais seulement modèles particuliers de ces théories. En langage courant, on dira qu'une théorie peut être vraie à propos de certains phénomènes sans qu'elle soit vraie de toute chose. Deuxièmement, si deux modèles sont trouvés vrai, il ne s'ensuit pas que la méthode de justification grâce a laquelle la vérité des modèles a été établie est problématique ou inadéquate.

II.1B Issues in Nineteenth Century Science

Physic 130

Chair/Président: Ernst Hamm

9:00–9:30

Elizabeth Neswald (Brock)

'The Benefits of a Mechanics' Institute and the Blessings of Temperance'. Science and Temperance in mid-nineteenth century Ireland

In Ireland, the 1840s saw both increasing popular enthusiasm for science and the emergence of Temperance as a mass popular movement. Mechanics' Institutes often required the Temperance pledge, while Temperance societies established their own programmes of scientific education, instruction and entertainment. Although informal affiliations between Mechanics' Institutes and temperance organizations were not uncommon in neighbouring Britain, this close co-operation between the two movements was specific to Ireland. The proposed paper will investigate connections between popular science education, temperance and the dissemination of scientific knowledge in mid-nineteenth-century Ireland. It will show that the class fears and aspirations that motivated discourses of science education and temperance in other countries played only a minor role in Ireland. Instead, science and temperance were seen as two fundamental aspects of a common project: creating an inclusive Irish nation. Sobriety and technical education, moral improvement and industrial improvement were to create the necessary conditions for self-government and economic self sufficiency, an Ireland politically and economically independent from British control.

9:30–10:00

Ian Hesketh (British Columbia)

"It is history itself that speaks": Aperspectival Objectivity and the Science of History in Victorian Britain

Making history a science was on the forefront of the professionalizing agenda among a close-knit group of highly self-conscious historical practitioners in mid to late Victorian Britain. Historians such as E. A. Freeman, J. R. Seeley, William Stubbs, and Lord Acton were not willing to go as far as positivist Henry Thomas Buckle, who wanted history to conform to the law making techniques of the natural sciences, but they did want historians to appropriate a scientific method and identity, one that would separate a professional history from its unwelcome bedfellow: literature. By incorporating the German historian's inductive research method with the natural philosopher's "aperspectival objectivity," the professionalizing historian in Britain developed a methodology that sought to let the "past speak for itself."

This paper will consider the methodological pronouncements of Freeman, Seeley, Stubbs, and Acton as a communal discourse that supposedly broke with so-called romantic and literary historians of a previous generation, men of letters whose immense skills of narration skewed—or simply ignored—facts in order to please a growing reading public. The new professional historian's scientific method, by contrast, promoted a strict

presentation of facts at the expense of drama or readability, culminating in a style of narration that was meant to conceal the subjective persona of the historian.

10:00-10:30
Kathleen Okruhlik (Western Ontario)
The Traffic between Epistemic and Ethical Issues in the Work of John Stuart Mill

In her 2006 book, *Reforming Philosophy,* Laura Snyder argues that in certain important ways, John Stuart Mill's political concerns motivated his attacks on Whewell's philosophy of science. Mill's (mis)perception of Whewell as an arch-conservative in politics influenced his criticism of Whewell's account of induction. Mill tended to regard Whewell as an "intuitionist" in the sense of Coleridge and was scathingly critical of intuitions as nothing more than prejudices standing in the way of truth, morality, and human progress. So, for example, the fact that Mill mistakenly believed Whewell opposed the abolition of slavery may have contributed to the controversy in philosophy of science.

I shall argue the relationship between epistemic and ethical issues in Mill merits closer scrutiny. An obvious place to start is with a comparison of the methodologies of the natural and moral sciences in the *System of Logic*. This leads naturally to another look at the status of political economy in the earlier and later Mill.

Going one step further, I shall contrast the anti-hypothetical enumerative and eliminative induction advocated in the *System of Logic* with *On Liberty*'s claim that epistemic progress is promoted by a proliferation of competing theories from diverse sources.. Finally, I shall argue that although Mill's most important contribution to early 20th-century philosophy of science may have been as a bridge between Hume and Russell within the tradition of radical British empiricism, his influence on philosophy of science in the later 20th century was due almost entirely to his liberalism.

10:45–12:15 Session/Séance II.2

II.2A Episodes in Twentieth Century Physics: Attempts at Unified Theories and Cosmological Models
Physic 107
Chair/Président: Yvon Gauthier

10:45–11:15
Carrie Klatt (Victoria)
Conditions for the Unification of Gravity and Electromagnetism

After Einstein published his theory of General Relativity (GR) attempts were made to extend the idea that gravity is a curvature of spacetime in ways that would unify electromagnetism with gravity under one theory. Kaluza-Klein theory considered a five-

dimensional spacetime that would include electromagnetism as part of the same mathematical framework. Wheeler's geometrodynamics considered how electromagnetic phenomena could be understood as resulting from exotic spacetime structures like wormholes. I will compare these attempts with the aim of establishing what conditions must be met in order for one to make claim to have a unified theory of the fundamental forces. I will show that first, a unification needs to include a principle that outlines how the new conceptual basis of the theory can account for both forces, much like what is accomplished by the equivalence principle in GR; second, the understanding of gravity that we get from GR shows why a unification that includes gravity will be much more difficult to achieve than a unification of the other three fundamental forces. Finally, I will use these results to put into context the aims of Einstein's own work, later in his life, to unify electromagnetism and GR.

11:15–11:45

Brian Allan Woodcock (Western Ontario)

Quantum State Collapse Along a Light Cone: History and Objections

The physicist I. Bloch in his 1967 paper "Some Relativistic Oddities in the Quantum Theory of Observation" came to the conclusion that the instantaneous collapse of the quantum state, as it has traditionally been conceived, presented a problem for compatibility with relativity. Rather offhandedly, Bloch suggested that the problem might be avoided by embracing the idea that the state transition due to a measurement occurs along a Lorentz invariant surface in Minkowski spacetime (i.e., the spacetime of special relativity). In particular, Bloch focused attention on the past light cone of the measurement event. The idea of positing the past (or future) light cone of a measurement event as the collapse transition surface in spacetime has a natural attractiveness to it because of the privileged status of the light cone structure in relativity theory. In this paper, I review the historically significant interactions with this proposal in the physics literature and critically examine objections to the proposal. Ultimately, the proposal fails to perform the explanatory work we seek from the process of state collapse in EPR situations. I attempt to show that reflection upon this failure provides insights which motivate embracing a hypersurface-dependent account of quantum state collapse of the sort first advocated by Aharonov and Albert in 1984.

11:45–12:15

Dylan Gault (Western Ontario)

The Quasar Challenge

One of the last scientific exchanges of note involving the Steady State theory took place in a few pages of the journal *Nature* from 1990 to 1992. Opponents of the standard cosmological model put forward, among other problems, a challenge to the model based on the distribution and nature of quasars. This quasar challenge is usually associated with the work of H. Arp, who participated in the exchange along with G. Burbidge, F. Hoyle, J. Narlikar, and V.C. Wickramansinghe. The defense of the standard cosmological model was taken up by P.J.E. Peebles, D.N. Schramm, E.L. Turner, and R.G. Kron. This paper examines the exchange and provides an analysis of the evidence presented in the quasar

challenge and in the defense of the standard cosmological model. Though it might be tempting to interpret the exchange as a collision of two competing and incommensurable paradigms, the defenders of the standard cosmological model do better than their opponents in providing quantitative evidence for their position. An important lesson from this exchange is in the use of tests that provide constraints and measurements on multiple parameters in agreement with those produced by other observations. These tests provide the standard cosmological model with an advantage over the quasar challenge in the absence of evidence about both the physics governing the ultimate natures of quasars and unresolved cosmological questions.

II.2B Ancient Cosmologies
Physic 130

Chair/Président: Walter Kreyszig

10:45–11:15
Jacqueline Feke (Toronto)
Aristotle's Theological Cosmology

The mutual influence of theology and such sciences as physics, cosmology, astronomy, and mathematics is evident in the natural philosophical writings of Aristotle. In *Metaphysics* E1 and K7, Aristotle distinguishes between physics, mathematics, and theology by differentiating between the objects which each of these fields studies. Most notably, physical and mathematical objects are inseparable from matter, while theology deals with bodies that are separable. In other words, theology, for Aristotle, is the study of prime movers. These bodies are immaterial but perfect in form; they are eternal and, therefore, divine. Despite his categorical division of the sciences, however, Aristotle's cosmological texts bear witness to the reciprocal relationship of physics, mathematics, and theology. For instance, in the *De caelo*, Aristotle puts forward his argument for the fifth element, the aether. As the material of the heavens, the aether is the most perfect element. Moving in uniform circular motion, it is eternal and divine. While not separable from matter, the aether shares these characteristics of being eternal and divine with theological entities. Moreover, in *Metaphysics* Λ, Aristotle places 55 (or 49) prime movers within the heavens as teleological and efficient causes of the planetary spheres. While separable from matter, the prime movers coexist with the aethereal spheres and are causes of their motion, which drive the movements of the planets. Thus, examination of Aristotle's natural philosophical texts illustrates how Greek thought allowed for theology not only coexisting with cosmology but also sharing with it a relationship of mutual influence and cooperative development.

11:15–11:45
Elizabeth Burns (Toronto)
Ancient Astronomical Mechanisms: Ptolemy's Planetary Hypotheses

In the *Planetary Hypotheses*, a work following the *Almagest*, Claudius Ptolemy gives a physical description of the cosmos, in which he discusses the motions, dimensions and

the driving forces of the heavenly spheres. Ptolemy clearly states that his work is written for both the astronomer and the instrument maker; however, the *Planetary Hypotheses* is generally treated as work for the former and not the latter. This is primarily due to Ptolemy's focus on astronomy and his evident lack of instruction addressing how to build a mechanical replica of the heavens. Nevertheless, this omission does not mean that Ptolemy did not intend for this work to be utilized by the instrument maker. Taking into consideration recent research on the Antikythera Mechanism, an astronomical device consisting of some thirty gears dating from the first century B.C., I will take a closer look at the aim of the *Planetary Hypotheses*, examining what kind of model Ptolemy intended the instrument maker to construct and if it would have been possible to build such a device.

11:45–12:15
Salvatore di Piazza (Palermo)
Technai stochasticai et la théorie du hos epi to polu: la connaissance conjecturelle dans la Grèce Antique

Notre texte se propose de démontrer que la catégorie de *tècnhe stochastikè* est très similaire à l'idée moderne de science. Plus précisément, il s'agit d'un concept que nous retrouvons déjà chez Aristote mais dont la plus claire définition se retrouve dans les textes de médicine de Galien ou dans la rhétorique de Philodème. A partir de la définition d'Alexandre d'Aphrodisias (V. Ierodiakonou 1996), le *tècnhai stochastikai* ont quatre caractéristiques:

> Elles procèdent d'une manière systématique mais conjecturelle, donc d'une manière partiellement déterminée.
>
> Elles se proposent de faire tout le possible afin d'arriver à leur but.
>
> (donc)
>
> Leur succès ne devrait pas être jugé en fonction de leurs résultats mais pour la plupart (*hos epi to polu*).
>
> Leur échec est dû aux caractéristiques intrinsèques de l'art, car les objets sont à leur tour influencés par des facteurs externes.

Comment se justifie-t-elle l'analyse de ce concept?

Une explication réside dans le statut artistique de ces disciplines comme c'est le cas de la rhétorique et de la médicine, des disciplines qui envisagent inévitablement la possibilité d'un échec car elles se retrouvent devant des questions qui ont exclusivement une valeur pour la plupart. Une autre question s'ajoute à notre raisonnement. Si un art est faillible, conserve-t-il encore le statut d'art? Quelle est la différence avec d'autres arts où l'erreur est interprétée comme due aux technìtes et non pas à la techné elle-même? Dans son œuvre, Aristote essaie de donner une dignité scientifique à des disciplines qui sont

caractérisées par la faillite et il le fait en reconsidérant la notion de pour la plupart, ce qui apparait par la suite comme fondamental pour notre raisonnement.

L'analyse du concept de stochastichè implique d'autres nombreuses questions auxquelles nous allons essayer de répondre: qu'est-ce que c'est la science? Qu'est-ce qu'elle devrait être? Où se situe-t-elle la frontière entre ce qui est science et ce qui ne l'est pas?

Le rhétoricien qui faillit à convaincre son auditorium, le médecin qui échoue dans la guérison de son patient ou le timonier qui échoue dans le sauvetage de son navire, ce sont des exemples d'artistes qui ne poursuivent pas une méthode infaillible et algorithmique mais une méthode basée sur *stochàsmos*, la conjecture, et, par conséquent, faillible.

A partir de cet encadrement général, notre texte se propose de répondre à cette question: Ne serait-il pas possible de retrouver dans la définition de tèchne stocastichè une première définition de la science moderne, similaire au cadre conceptuel de Popper ou de celui bien antérieur de Peirce, à savoir une science caractérisée par une approche conjecturelle et faillible?

II.2C Sociology of Scientific Knowledge
Physic 128

Chair/Président: Cornelius Borck

10:45–11:15

Lynda Lange (Toronto)

Scholarly Prosperity and Institutional Epistemology in Early Modernity

This paper will briefly set out familiar early modern conceptions of empirical science or "natural philosophy" (Francis Bacon and others), in order to juxtapose them with the kinds of "observations" and knowledge claims Europeans made concerning the indigenous peoples in what is now South and Central America during roughly the same period. Considering that divisions between what we now call natural science and social science had not yet been formed and a diversity of knowledge claims were supposedly on about the same footing, the discrepancy of method is something that inspires amazement. It seems to call for a fuller analysis than presumed colonial motivations of exploitation and/or duplicity to explain the scholarly prosperity of knowledge claims about indigenous non-European peoples. I use the logic of Edward Said's critical concept of "orientalism" as a self-referential field of scholarship that became completely respectable in both government departments and universities, in spite of its inaccuracies from the point of view of Eastern peoples, and its divergence from what might be considered "the facts", to make a first attempt to develop a critical notion of what I call "institutional epistemology", which I maintain cannot simply be dismissed as bad method, and still has relevance in the scholarly institutions of the present.

11:15–11:45
Jeff Kochan (Alberta)
Scepticism and the Sociology of Scientific Knowledge

There has been almost no systematic study of the role of scepticism in the Sociology of Scientific Knowledge (SSK). This is a serious oversight as central figures in SSK have clearly emphasised the importance of scepticism for their work. In this paper, I shall take some steps towards filling this lacuna. I will gloss scepticism as both a method and a problem in SSK, and discuss it with detailed reference to the works of Barry Barnes, David Bloor, and Harry Collins. I will argue that Barnes and Bloor have failed to adequately address the problems posed to their projects by scepticism, and that those projects are seriously, perhaps fatally, flawed as a result. I will furthermore argue, in contrast, that Harry Collins has been more successful in answering the problems posed to his work by scepticism. I suggest that this success is due, at least in significant part, to Collins' self-conscious application of the techniques of phenomenology. I will conclude the paper with a general plea for a broader collaboration between SSK and phenomenological philosophy of science.

11:45–12:15
Karyn Freedman (Guelph)
Diversity and the Fate of Objectivity

Helen Longino (2002) argues that the way to ensure that scientific knowledge is objective is to have a diversity of inquirers in our scientific communities. This is the best example of recent feminist arguments that hold that the real value of diversity is epistemic, and not political, but even it only partly succeeds. In this paper I argue that by socializing cognition Longino gives us a good reason for wanting diversity in our communities of scientific investigators, and by socializing knowledge she gives us a kind of objectivity. But in the final analysis this objectivity is nothing more than intersubjective agreement about contextually based standards and goals, and the good reason for wanting diversity in our scientific communities turns out to be strictly political.

12:15–14:00 Lunch/Dîner

12:30–14:00 Council Meeting/ Réunion de Conseil d'Administration Physic 175

14:00–15:30 Session/Séance II.3

II.3A Structural Explanations in Relativity and Quantum Theory

Physic 107

Chair/Président: Brian Allan Woodcock

14:00–14:40

Iulian Toader (Notre Dame)

Structural Realism and the Problem of Inequivalent Representations in Algebraic Quantum Field Theory

My aim in this paper is to show that the algebraic turn taken by some proponents of structural realism leads to a backbreaking difficulty with the interpretation of quantum field theory (QFT).

In the first section, I present this algebraic turn, advocated today by Steven French and James Ladyman, whose view I will refer to as algebraic eliminative structural realism (AESR). The main idea of AESR is that there are no objects in the world, except insofar as they are defined via an abstract algebraic structure, which is all there is out there open to our cognition. AESR is mainly motivated by non-classical indistinguishability phenomena in quantum mechanics. Further support is allegedly given by philosophical implications of QFT.

In the second section, I raise a challenge to AESR. I contend that the claim according to which the physical content of a theory is fully captured by its mathematical structure is bound to fail. To support my contention, I provide a short analysis of the problem of inequivalent Hilbert space representations of an abstract C^*-algebra in QFT. Then I show that no notion of equivalence, from the few available in the mathematical formalism, can be taken as expressing the physical equivalence of Hilbert space representations, without loss for the AESR position.

I close with an example from quantum statistical mechanics regarding the explanation of thermodynamic phase transitions, which makes quite intuitive the kind of conundrum faced by AESR.

14 :40-15 :20

Yvon Gauthier (Montreal)

Minkowski's space-time diagrams: from the geometry of numbers to physical geometry

To the historian or philosopher of science, Hermann Minkowski is known for the formulation of Special Relativity in terms of four-dimensional space-time. The original text is the famous 1908 « *Raum und Zeit* » (see [2]), but it is rarely mentioned that Minkowski is the author of a geometry of numbers « *Geometrie der Zahlen* », a most important work in number theory. In his arithmetic geometry, Minkowski introduces the notion of numerical grids or lattices « *Zahlengitter* » that are meant as a geometrical representation of arithmetical relations, that is isolated points and intersection points used to define the approximation of a real number by rational numbers.

I want to show that the concept of a numerical grid is the origin of Minkowski's diagrams in the physical geometry of Special Relativity. Minkowskian space-time is isomorphic to a universal numerical grid with no ontological import. This might also be relevant for the new physics of spacetime (with strings or loops) which puts into question the modern concepts of space and time in their relativistic or quantum-mechanical settings (see [1]).

II.3B Histoire des sciences et des technologies biomédicales
Physic 130

Chair/Président: Salvatore di Piazza

14:00–14:30

Yohan Philippe (Sherbrooke)

Louis Pasteur et la génération spontanée

Avec ses travaux sur la génération spontanée, Louis Pasteur s'attaque à un problème dont il a clairement conscience que les retombées scientifiques, mais aussi philosophiques, sont incalculables puisque s'interroger sur les conditions d'apparition de la vie, c'est s'interroger sur la définition même du vivant. Dès lors, on comprend pourquoi le règlement de la question des générations spontanées a fait, avant même les travaux sur la vaccination, la gloire de Pasteur. Plus que scientifique, la question était devenue, à la fin du 19ème siècle, une question d'actualité.

Après avoir replacé dans son contexte le débat sur les générations spontanées, l'enjeu consistera à saisir la spécificité de l'intervention de Pasteur au niveau de la méthode employée et des conclusions auxquelles elle permet de parvenir.

Pour tenter de saisir les points importants de la méthode pasteurienne, nous nous proposons de faire une analyse générale de son « *Mémoire sur les générations spontanées* » afin de montrer, d'abord, que le premier chapitre, qui à lui seul nous semble contenir comme « en germe » les principaux procédés méthodologiques et épistémiques de Pasteur, dépasse le simple « historique de la question ». Ensuite, que ce texte permet de mieux comprendre le balancement pasteurien entre théorie et pratique expérimentale afin de définir cette rigueur théorique et cette précision expérimentale qui lui étaient propres.

14:30–15:00

Stéphanie Tésio (Laval)

La pharmacie civile et urbaine au XVIIIème siècle. Etude comparative (France-Canada)

En colonisant l'Amérique, la France implante, dans ses colonies, des institutions telles que la seigneurie, la paroisse, le gouverneur, les institutions religieuses hospitalières et le corps médical, etc. Cette communication a pour objectif de faire état des résultats de la thèse récemment terminée portant sur l'introduction de l'une de ces institutions françaises : la pharmacie, à travers les exemples de deux régions liées historiquement par le mouvement migratoire, la Basse-Normandie et le Canada. Dans ce contexte de rapport

métropole-colonie, il s'agit de comprendre le passage du modèle pharmaceutique entre un pays vieux et un pays jeune qui vit dans un contexte nord-américain. Pour ce faire, trois axes directeurs sont retenus : l'organisation professionnelle (avec la définition des vocables, les effectifs et les densités, les règlements en vigueur, la présence en Basse-Normandie et l'absence au Canada des communautés de métiers, et les procès) ; puis, la pratique thérapeutique (les livres et les bibliothèques, les théories médicales, le contenu des officines, les exemples de traitements médicaux et les relations patients-praticiens) ; enfin le niveau social et économique (la démographie, le réseau relationnel, la transmission du métier, le niveau de fortune). Ces trois points essentiels mettent en exergue que le Canada, par rapport à la France, garde des éléments typiques de la pharmacie et crée sa propre originalité sur d'autres aspects.

This Ph. D. has one main : to study the European model of pharmacy in 18th century in two areas, the first in Low-Normandy in the French metropolis (mother country), the second in Canada in the North America colony. It wishes to understand the becoming of one typical civil European institution in the Mother Country and in his settlement, to see if some variations appear between both. This study is based on the inventory after death of the praticians of pharmacay (apothcaries), the judicial archives, and the state of being provate citizen. Three following themas are : the first, the professional organisatin of the practicians of pharmacy (definition, numbers, density, laws, existence and inexistance of corporations, lawsuits) ; the second, the therapeutic practice (books and libraries, medical 18th century theories, the inside of apothecaries — simple and composed drugs, plants, seeds, the influence of American seeds on the French pharmacy, and the typical tools — examples of medical treatments, and the relationships patients-praticians) ; and the third, the social and economic study (demography, social web, transmission of the job, fortune and revenues). The conclusion of this Ph. D. showes that pharmacy stays in its standard in France, and knows some variations in the settlement due to the North America context, and to the attitude of the French government and the English government.

15:00-15 :30
Fabrice Gzil (Sorbonne)
L'individualisation de la maladie d'Alzheimer (1906-1911): une découverte fondatrice ou une contre-pensée?

'individualisation de la maladie d'Alzheimer (1906-1911) : une découverte fondatrice ou une contre-pensée ?

La maladie d'Alzheimer (MA) a été individualisée il y a tout juste cent ans. Kraepelin forgea cette expression en 1910, pour nommer une série de cas dont le premier avait été décrit par Alzheimer en 1906.

Pour la plupart des commentateurs, l'individualisation de la MA fut une découverte capitale : Alzheimer a parfaitement décrit les particularités cliniques et anatomopathologiques de cette affection, il a remis en cause le monopole de l'étiologie vasculaire de la démence, et il a inauguré un champ de recherche radicalement nouveau.

Certains historiens sont beaucoup plus critiques. Selon eux, la MA n'a pas été découverte mais construite : Kraepelin et Alzheimer ont artificiellement opposé la MA et la démence sénile ; leur interprétation a plus entravé la recherche qu'elle ne l'a fait progresser.

Une lecture attentive des textes montre que s'ils étaient intéressés par les questions de nosographie, les chercheurs du début du 20e se sont surtout interrogés sur la nature et la genèse des lésions histologiques et des troubles cliniques ; que c'est sur ces questions que les contributions d'Alzheimer étaient les plus originales ; et qu'Alzheimer n'a jamais opposé la MA et la démence sénile car pour lui la MA était une forme atypique de démence sénile.

Alzheimer ne pensait pas avoir découvert une nouvelle maladie. Il ne mettait pas sous le nom de MA la même chose que nous. Ses conceptions étaient pourtant très proches des nôtres. Comment expliquer l'oubli dans lequel sont tombées, pendant plusieurs décennies, les nuances de sa pensée ?

II.3C Forging Disciplines
Physic 128
Chair/Président: Kenton Kroker

14:00–14:30
Katharine Anderson (York)
Oceans and Scientific Observation

This paper investigates how oceanographic research in the 1920s contributed to discussions about 'modern' forms of natural history. It focuses on the Arcturus expedition of the New York naturalist William Beebe in 1925. This expedition is firmly entrenched historically as popular natural history: backed by millionaires Henry Whiton and Harrison Williams, led by a charismatic 'explorer' and writer, it was in many ways a media extravaganza, orchestrated by Putnam & Co. But because Beebe sat on the borderland between scientist and showman, he was particularly interested in defining the intellectual credentials of his work. The expedition is revealing, therefore, as a representation of modern scientific methodologies in the relatively new discipline of oceanography. Beebe's approach to oceans as a form of field study, closely related to his earlier work in tropical field ecology, gives us a picture of the range and energy with which scientists in the 1920s sought ways to study the oceans.

14:30–15:00
Vivien Hamilton (Toronto)
What do Doctors Need to Know about Physics? The Creation of Medical Physics Following the Discovery of X-Rays.

Within weeks of Röntgen's announcement of a new type of radiation in 1896, a number of physicians eagerly began to explore the clinical utility of the new rays. Their excitement was not tempered by the fact that the physics community was unable to give a

confident characterization of the physical nature of the radiation. As expressed by an early piece in the *Lancet*, it was not a full physical understanding of the properties of the rays that physicians hoped would yield better results and clearer pictures, but a perfection of "the conditions of the experiments." In fact many medical practitioners and engineers forged on with clinical research, developing improved techniques and instruments without waiting for answers to deeper ontological questions. But if the medical community was content simply to develop an operational understanding of the technology, leaving an exploration of the physical nature of the rays to the physicists, then how do we understand the emergence of medical physics just prior to WWI? Why did hospitals and medical schools begin to employ physicists? What role did these new medical physicists play and how was this role constructed? This paper will examine the transmission of specific concepts and techniques from physics to medicine in the years leading up to and following the appearance of medical physics in order to investigate both the reasons for the existence of this new discipline as well as the effect that its emergence had on communication and collaboration between the two communities.

15:00-15 :30
Martha Harris (Toronto)
Emergence of a Chemical Physicist: Pauling and a Community on the Verge

In 1939, American chemist Linus Pauling published the now classic text in chemical physics, The Nature of the Chemical Bond, the culmination of more than a decade's research into structural chemistry and valence bond theory. The formative research for this work developed from 1927-1931, when Pauling returned from a postdoctoral research year in quantum mechanics in Munich to begin his career as Professor of Theoretical Chemistry at Caltech. During this period, as one of the few American chemists skilled in the new chemical applications of wave mechanics, Pauling developed a program combining these applications with experimental understanding of chemical structures and valence bonding. Within two years of his Caltech appointment, Pauling received a series of unsolicited offers for faculty positions and guest appointments from major American universities due to physical and chemical interest in his research. A new study of Pauling's correspondence in this period with contemporaries at Harvard, M.I.T., Berkeley and Caltech, reveals a fragmented physico-chemical community on the verge of disciplinary change to a new chemical physics. These offers, and the contacts Pauling formed within the burgeoning chemical physics community, show that his chemical bond research presented both a challenge to the older, early twentieth-century disciplinary framework of physical chemistry, and an opportunity to build new disciplinary ties with physicists and chemists who now recognized the value of combining theoretical physics and theoretical chemistry.

15:45-16 :45 Session/Séance II.4

II.4A Music as *Scientia*
Physic 107

Chair/Président: María Ponte Azcárate

15:45-16:15
Ohran Noh (Saskatchewan)
Art or Science OR Art and Science --- Wolfgang Amadeus Mozart from the Perspective of Franz Joseph Haydn's Compositionswissenschaft; Testing the Analytical Method of Heinrich Schenker on the Opening Movement of Mozart's Keyboard Sonata in F-Major, KV 533/494

In a letter dated 16 February 1785 from Leopold Mozart to his daughter Nannerl Mozart, Franz Joseph Haydn pronounces Wolfgang Amadeus Mozart the greatest in the science of composition—what at first sight might appear as provocative statement. In his original prophetic comment, Haydn captures Mozart's manifold contributions in the single noun *Compositionswissenschaft*, literally the "science of composition," but misinterpreted in various English translations as "knowledge of composition". What Haydn meant by the term *Compositionswissenschaft* toward Mozart's genius can be tested by music theorist Heinrich Schenker's analysis of Mozart's Keyboard Sonata in A Major, KV 331, in which Schenker denies vehemently that music is a science. However, his analysis contradicts his denial, demonstrating a structural and systematic approach to music which is more closely related to science. Considering its origin, the *disciplina musicae* had been viewed not as an *ars* but as a *scientia* since Antiquity and through the Middle Ages and the Renaissance, evidenced by its inclusion with arithmetic, geometry, and astronomy as part of the *quadrivium* (the four core quantitative sciences), and Haydn holds fast to this earlier notion of music embedded in scientific discourse. The introduction of printing in the late Renaissance provided rapid dissemination of music and thus a new appreciation of the discipline focusing on the endeavor's pleasurable side without completely neglecting its scientific aspect, and music gradually became a "Fine Art," a status it retains today. Schenker's contradictory approach to music—artistically and scientifically—highlights the question whether music, historically and philosophically, is considered as an Art or Science OR Art and Science.

16:15-16:45
Walter Kreyszig (Saskatchewan)
The Greek systema teleion and the Guidonian systema hexachordum as scientia musicae in the Context of the Humanist historiographia and philosophia: Bridging National, Linguistic and Disciplinary Boundaries in the Trilogy of Franchino Gaffurio (1451-1522)

In his *Introductorium musice practicae* (Münster, 1513), the German music theorist Dietrich Tzwyvel (born before 1490; died after 1516) included a poignant remark extolling the extraordinary achievements of Franchino Gaffurio (1451-1522) in transmitting the thoughts of Greek antiquity, the Middle Ages and the early Renaissance. "I collected those things for you [the reader], that you asked for, especially extracting practically all the theory from the music treatise of the excellent Franchino Gaffurio of Lodi. This [theory] seems to me very fruitful and authentic. For he applied his mind to this discipline from his youth on and he became an outstanding professor of the two kinds

of music [i.e. theory and practice]. He taught theoretical music and opened up its more secret aspects." Notwithstanding the reference to a specific volume within Gaffurio's trilogy, that is, the *Theorica musice* (Milan, 1492), or the *Practica musicae* (Milan, 1496), or even the then anticipated *De harmonia musicorum instrumentorum opus* (Milan, 1518), the extent of Tzwyvel's comments leaves little doubt as to a decisively broad reference embracing all three volumes ? alluding to the overall exceptionally vast perspective, with the three volumes carefully conceived as a unified corpus of writing as is readily gleaned from the numerous references to the individual volumes offering a cogent linking of thoughts. In his detailed examination of the *systema teleion* and the Guidonian *systema hexachordum* as the widely acknowledged principal pillars of the Medieval and early Renaissance *scientia musicae*, and that in the broader context of the *artes liberales* and the *artes mechanicae*, Gaffurio wholeheartedly subscribes to the principles of making public knowledge and making knowledge public and as such to the theme of the 2007 Congress of the Humanities, in a most forceful manner bridging the many communities elegantly exemplified in the magic triangle between author, opus and audience.

II.4B Canadian Industry : Organizations and Practices
Physic 130

Chair/Président: Elizabeth Neswald

15:45-16:15

Janet Martin-Nielsen (Toronto)

In Principle but not in Practice: Professional Engineering Organizations in 20th century Canada

The two principal bodies which served Canadian engineers in the 20th century were the Engineering Institute of Canada (EIC) and the group of provincial licensing associations. Dating from 1887, the EIC is a learned society focused on the dissemination and advancement of technical knowledge. The provincial licensing associations – legally-constituted bodies charged with the regulation and legislative protection of the engineering profession – came onto the scene after the First World War. These organizations twice attempted to merge and form a single national body for the management of the engineering profession, once in the 1930s and again in the 1960s. While these movements consumed vast quantities of resources and energy, both attempts ultimately failed and the two bodies remain separate today.

This paper explores the transformation of professional engineering organizations in Canada from the 1920s to the 1970s and, specifically, the two attempts to merge the EIC and the provincial associations. It is argued that the learned and regulatory engineering bodies developed distinct definitions of a "professional engineer", and that the impossibility of reconciling these definitions resulted in the failure of the merger attempts. More broadly, it is argued that the historical mandates of the EIC and the provincial associations fostered divergent views of the professionalization of engineering, creating a fundamental divide in Canada's engineering community.

16:15-16:45
Renald Fortier (Canada Aviation Museum)
Weed'em and Reap - A Look at Agricultural Aviation in Canada in the Early Postwar Period

For thousands of years, people all over the world have depended on the crops they grow to sustain them. Throughout that time, they had to battle a host of natural enemies in order to protect these vital food supplies. Even today, in a wealthy country like Canada, pests of all types still pose a formidable problem. The impact can be devastating given agriculture's contribution to the economy - more than 27 billion dollars in 2005.

Even though some dusting of crops with fine powders was done in Canada during the 1920s and 1930s, agricultural aviation actually emerged in this country after the Second World War. The proposed paper looks at the history of a few pioneering companies located in various regions of Canada. Some were existing firms that diversified their activities while others were created in the early postwar years by air force veterans. Others still pioneered the use of helicopters in Canada. Some of these companies have survived to this day, among them one of the most successful civilian helicopter operators in the world.

Sadly enough, the history of the Canadian aerial application industry remains poorly known. While it is true that it never compared in size to that of the United States, the story of the people and companies involved - not to mention their impact on the environment - is well worth telling.

II.4C Material Practices in the Bio-Medical Sciences
Physic 128
Chair/Président: Tracy Finn

15:45-16:15
Tricia Koenig (Louis Pasteur, Strasbourg)
One's garbage is another's treasure: "Informed materials" in the pathology lab

From the cutting out a tumour to cataract removal to amputation, alleviating the body of a diseased or superfluous part has long meant alleviating the body of illness. As such, the removal of tumours and diseased organs was the recourse in treating cancers, amongst other illnesses, at the turn of the twentieth century. In this paper, I will not discuss the history of surgery or of extirpating body parts, but I will look at the postoperative existence of the parts. I suggest Bensaude-Vincent and Stengers' "informed material" as a useful framework for understanding the development of medical diagnosis in the pathology laboratory.

It can be expected that surgical remains were tossed out with the trash; after all it was unwanted matter. However, the early twentieth century pathologist, responsible for post-

mortem examinations, used this rubbish to study disease in much the same way that the dead body was used to reveal lesions. In redirecting the surgical remains from the garbage bin to the research lab, the pathologist attributed the remains with a new identity; it became a treasure box waiting to be opened. The morphological classification of tumours further transformed the remains into an object with a name. The histo-pathological classification of tumours temporally corresponded to the development of radiotherapy treatment of tumours and cancers. The information locked in the surgical remains then became an entrance ticket to cancer treatment centres.

16:15-16:45
Howard Hsueh-Hao Chiang (Princeton)
Laboratory Practice, Biological Knowledge: The Intersecting Paths of Separation Science and Biomolecular Science, 1945-1965

Preparative and analytical methods developed by separation scientists have played an important role in the history of molecular biology. One such early method is gel electrophoresis, a technique that uses various types of gel as its supporting medium to separate charged molecules based on size and other properties. Historians of science, however, have only recently begun to pay closer attention to this material epistemological dimension of biomolecular science. My work substantiates the historiographical thread that explores the relationship between modern laboratory practice and the production of scientific knowledge. This paper traces the historical development of gel electrophoresis from the mid-1940s to the mid-1960s, with careful attention to the interplay between technical developments and disciplinary shifts, especially the rise of molecular biology in this time-frame. Claiming that the early 1950s marked a decisive shift in the evolution of electrophoretic methods from moving boundary to zone electrophoresis, I reconstruct a trajectory in which scientists such as Oliver Smithies sought out the most desirable solid supporting medium for electrophoretic instrumentation. Biomolecular knowledge, I argue, emerged in part from this process of seeking the most appropriate supporting medium that allowed for discrete molecular separation and visualization. The early 1950s, therefore, marked not only an important turning point in the history of separation science, but also a transformative moment in the history of the life sciences as the growth of molecular biology depended in part on the epistemological access to the molecular realm available through these evolving technologies.

17:00–19:00 President's Reception

19:30–22:00 CSHPS/SCHPS Banquet

Day III. Wednesday, May 30/Jour III. Mercredi, 30 Mai

9:00–10:30 Session/Séance III.1

III.1A Conceptions of Space and Infinity
Physic 107
Chair/Président: María Ponte Azcárate

9:00–9:30
Brett Coppenger (Western Michigan)
The Notion of the Void in the History of Science

The purpose of my paper is to argue that the development of the notion of the void supports the Continuity Thesis. In order to achieve this goal, first, I will argue that Galileo's understanding of the void was the result of a slow progression of thought. Second, I will argue that this slow progression of thought offers support for what has been coined the Continuity Thesis.

In an effort to argue for my thesis I will show that Galileo's concept of the void, while distinct, was the result of a gradual progression of arguments stemming from the rejection of Aristotle's arguments against the void. Specifically, I will focus on the texts of Philoponus and Avempace to accurately track the progression of the notion of the void. My conclusion will be that while Galileo's contribution was extensive, his idea of the void was not wholly original. As a result, this episode in the history of science offers support for the Continuity Thesis.

In order to support the idea that this episode in the history of science illustrates continuity, I will argue that the Continuity accounts given by Moody and Grant are more accurate than those of Discontinuity given by Drake and Strong. An initial reason for this method is the support of the Continuity Thesis. While it is true that the Discontinuity Theorist may maintain that Galileo's understanding of the void is of little importance to the history of science at large, it is my contention that any episode in the history of science that shows continuity does some positive work for the Continuity Thesis.

9:30–10:00
Eran Tal (Toronto)
Kant and the Problem of Continuous Space-Perception

A primary goal of Kant's philosophy of mathematics is to establish the *a priori* applicability of geometry to outer experience. Working towards this goal, the *Critique of Pure Reason* attempts to establish a common ground for the possibility of two types of syntheses. These are (i) the synthesis of pure space, i.e. geometrical construction; and (ii) the synthesis of empirical space in apprehension. In this paper I reveal a substantial gap in Kant's argument: he only proves that the synthesis of apprehension is successive and continuous in time, without providing a corresponding proof for the case of space. This exposes his argument to the worry that objective spatial determinations may not be possible in empirical space as they supposedly are in pure space.

Rather than concluding that Kant simply ignored the problem, I suggest that the solution appears implicitly in an unexpected section of the *Critique,* namely in the Third Analogy of Experience. While the Analogies are traditionally viewed as assuming that the empirical validity of mathematics is already secured, I reinterpret the Third Analogy as supplying justification for the spatially continuous synthesis of apprehension and hence for the possibility of applied geometry. My revised reading of the Analogy exposes a hidden assumption that Kant makes concerning an isomorphism between the workings of perception and the dynamics of Euclidian construction.

10:00-10 :30
Lawrence Deck (Montréal)
Greek Antecedents of Hegel's Doctrine of the Mathematical Infinite

The paper examines the conceptual heritage of Hegel's treatment of "Quantitative Infinity" in the *Science of Logic*, with special reference to ideas of infinity and continuity elaborated in classical antiquity by Aristotle, Eudoxus, Euclid and Archimedes. In his analysis of "the infinite in general" and the infinite as it appears in the mathematics of his day, Hegel combines ancient and modern notions in a unique and complex way. He considers the application in mathematics of the "bad infinite" of indefinite progression as well as the true infinite of ratio. It has been suggested that the latter is a case of the "actual infinite", a notion with both classical and modern connotations. The question can be put: to what extent does Hegel's doctrine agree with the Greek doctrine or doctrines of the infinite? In particular, can Hegel's quantitative infinity be called an actual infinite in an Aristotelian or any other sense?

III.1B Challenging the Hierarchy of Evidence in Evidence-Based Medicine
Physic 130
Chair/Président: Gord Binsted

9:00–9:30
Robyn Bluhm (Western Ontario)
The Hierarchy of Evidence and Biomedical Research

Although in principle EBM is supposed to operate with a broad definition of evidence, on which "any empirical observation about the apparent relation between events constitutes potential evidence,"[3] in practice the evidence considered by EBM comes from its "hierarchy of evidence." This hierarchy places clinical trials, which borrow from the methods of epidemiology, above studies examining physiological mechanisms, and favours randomized over non-randomized clinical trials. The hierarchy also "implies a clear course of action for physicians addressing patient problems: they should look for the highest available evidence from the hierarchy."[4] In this paper, I argue that, as it stands, the hierarchy of evidence provides a view of medical research that will ultimately limit the progress that can be made in biomedical research (and thus in medical practice). I further suggest an alternative characterization of the relationship between different

types of biomedical research that (1) replaces the focus on the randomized/nonrandomized dichotomy in clinical research with a more nuanced understanding of methodological choices and (2) draws more closely on epidemiology in integrating population-based research with research into causal mechanisms underlying disease.

9:30–10:00
Kirstin Borgerson (Toronto)
The Value(s) of Objectivity in Evidence-based Medicine

This paper investigates a central assumption underlying the hierarchy of evidence offered by EBM, namely that evidence derived from research methodologies ranked higher on the hierarchy is more *objective* than evidence below. Objectivity is regularly used to signify "everything from empirical reliability to procedural correctness to emotional detachment."[5] Heather Douglas has recently identified eight distinct senses of objectivity in common use. [6] I draw upon her careful catalogue in order to characterize the sense(s) of objectivity implicitly and explicitly assumed by proponents of EBM in the design of the evidence hierarchy. I raise some concerns about the potential dangers of relying exclusively on these mechanisms for producing objectivity, particularly in terms of their inability to address the influence of unidentified social values on science. I argue that an over-reliance on procedural objectivity has led proponents of EBM to the false belief that methodology alone (narrowly construed) can secure objectivity, and also to the related and even more problematic belief that guidelines produced on the basis of the evidence hierarchy provide an objective basis for medical decisions. Finally, I draw upon philosopher of science Helen Longino's comprehensive account of contextual objectivity and some of the early writings from EBM proponents in order to suggest ways in which EBM might be improved.

10:00-10 :30
Maya Goldenberg (Toronto)
Iconoclast or Creed? Objectivism, Pragmatism and Evidence-Based Medicine's Hierarchy of Evidence

Because "evidence" is at issue in EBM, the movement has been largely critiqued on postpositivist grounds, where the critics have drawn from the work of Quine,[7] Kuhn,[8] or Popper[9] to demonstrate the untenability of the objectivist account of evidence underscoring the evidence- based approach. While these post-positivist critiques seem largely correct to me, I propose that the critics miss important and desirable pragmatic features of the evidence-based decision making technology. I redirect critical attention toward EBM's rigid hierarchy of evidence as the culprit of its objectionable epistemic practices. Reframing the EBM discourse in light of a distinction between objectivist and pragmatic epistemology will allow for a more nuanced analysis of EBM than previously offered: one that is not "either/or" in its evaluation of the decisionmaking technology as either iconoclastic or creedal.

10:45-12:15 Session/Séance III.2

III.2A Problems in the Philosophy of Biology
Physic 107

Chair/Président: Howard Hsueh-Hao Chiang

10:45–11:15
Frédéric Bouchard (Montréal)
Termites, symbiosis and the necessity for causal explanations in evolutionary biology

Walsh, Ariew and Lewens (2002) and Matthen and Ariew (2002) have argued that natural selection's role in evolutionary theory should not to be described in causal terms but in statistical terms. In their account, natural selection is not a cause of evolutionary change but the result of change caused by other physical and biological constraints. I will argue that one of the shortcomings of this view is that it cannot account for many cases of evolution in the biological world such as the evolution of many symbiotic communities (e.g. *Macrotermes-Termitomyces* colonies). I will describe how the complex ecological and evolutionary interactions found in many symbiotic relationships cannot be described exclusively in terms of differential reproductive success. Using this case-study I will argue that a purely statistical approach to understanding natural selection misrepresents the true nature of evolutionary change for symbiotic communities in particular, but also for other cases of evolution by natural selection as well.

11:15–11:45
Rachel Bryant (Toronto)
"Invasive" species and the diversity-stability hypothesis

The diversity-stability hypothesis has been a keystone of community ecology since the young discipline began to thrive in the mid-twentieth century. Roughly, the hypothesis states that the stability of ecological communities correlates positively with their diversity. Current debates about the hypothesis focus on the definitions of diversity and stability. Yet all sides have this in common: an assumption that the putative correlation is driven by diversity.

Contemporary incarnations of the diversity-stability hypothesis attend solely to the effects of diversity on stability, neglecting the effects of stability on diversity. However, the earliest versions of the hypothesis took them seriously. For instance, in his watershed *Ecology of Invasions by Animals and Pants, (1958)* Charles Elton relied on both the effects of diversity on stability and the effects of stability on diversity to explain ecological communities' differential susceptibility to so-called invasive species. In this paper, I revisit Elton's view and suggest a novel way in which invasions demonstrate the bi-directionality of diversity-stability relationships.

11:45-12:15
Eric Desjardins (British Columbia)
Historicity and Irreversibility in Biology

Many claim that the historical nature of biological processes follows from their irreversibility ö(Gould 1970; Szathmary 2006; Levine and Sober 1985; Prigogine 1986, just to name a few). This paper argues that this received view is too vague and sometimes false. I highlight three distinct ways in which historicity can connect with irreversibility and show that some cases of reversibility rely on the historical nature of biological systems.

According to one prevalent position, irreversibility is the practical impossibility for a system to return to some previously visited states. This impossibility derives from the tendency of complex organisms to retain past modifications. Under this tradition, irreversibility thus results *from* historicity.

By contrast, Szathmary (2006) refers to contingent irreversibility as the phenomenon of uniqueness of evolutionary transition, and equate historicity with path dependence. According to his analysis, irreversibility is a condition for path dependence, and hence becomes a consequence of irreversibility.

A third approach represents irreversibility in biology, by analogy with entropy in thermodynamics, as a built-in temporal asymmetry. Levine and Sober (1985), for instance, apply this reasoning to evolutionary biology, claiming that the average fitness of ideal populations always increases with time. Under this understanding, historicity becomes identical to irreversibility.

Contrary to the received view, I suggest that not all cases of reversibility should count as a-historical. Examples of throwbacks and secondary adaptations, both apparent manifestations of reversibility, rely on the capacity to retain information from the past.

III.2B Bodies in Space: Measurement and Prediction
Physic 130

Chair/Président: Ryan Samaroo

10:45–11:15

Josipa G Petrunic (Edinburgh)

Continuity and the ether: William Kingdon Clifford (1845-1879) and the measurement of space in the 1860s

The 19th-century is often referred to as a period of important growth and development in the fields of calculus, algebra, and geometry. Many of those developments were linked to changing conceptions of space. They were also linked to changing conceptions of "continuity", an idea that underlies the technique of measuring movement in space – namely, differential calculus. By looking at the way in which William Kingdon Clifford (1845-1879) used the concept of "spatial" and "temporal" continuity in his own mathematical and philosophical works, I will argue that work on non-Euclidean geometrical models in Great Britain in the 1860s was heavily shaped by materialist beliefs in the *continuous* ether medium pervading the universe. I plan to use the

Cliffordian case study in this regards to highlight the sociological nature of mathematical thinking as it relates to historical debates surrounding the distinction between “discrete number” and “continuous quantity” (a distinction that originated for Clifford in Bernhard Riemann’s now-famous 1854 paper, “On the Hypotheses that Lie at the Base of Geometry”).

11:15–11:45
Allan Olley (Toronto)
"In the most accurate way and by the shortest path." E. W. Brown and calculating machines

Ernest William Brown (1866-1938) is best remembered for his monumental work on lunar theory that allowed precise predictions of the moon's position. This remained the most accurate predictor of lunar position for much of the 20th century. In the history of the development of the modern computer Brown is also remembered as the teacher of W. J. Eckert (1902-1971). Eckert along with the engineers of IBM made important developments in automatic punched card calculating machines in the 1930s and 40s, often in order to further Brown's work. Brown is occasionally mentioned in this regard as providing Eckert with knowledge of the work of L. J. Comrie, who applied punched card machines to calculating Brown's lunar equations. What is not recognized is that Brown himself showed a significant interest in using calculating machines himself and did work himself on applying mechanical devices to problems in celestial mechanics. These developments are in ways reminiscent of later developments. I will examine this thread of Brown's career and its relations with Eckert's work with reference to published accounts and archival correspondence.

11:45-12:15
Ben Almassi (Washington)
Trust in Expert Testimony: Arthur Eddington’s 1919 Eclipse Expedition and the British Response to General Relativity

The 1919 British astronomical expedition led by Arthur Eddington to test a particularly alluring implication of Einstein’s general theory of relativity provides a fascinating case study on the role of trust in expert testimony in the generation and propagation of scientific knowledge. Popper lauded the eclipse experiment as clearly vindicating the general theory over classical mechanics and as an exemplar of conjecture-and-refutation style science. More contrarian accounts such as that offered by Collins and Pinch suggest a messier epistemological arrangement, in which a decidedly not disinterested lead researcher makes non-trivial choices in the interpretation of the experimental results in presenting those results to a British scientific community unfamiliar with the general theory or its observational implications. In this paper I revisit the historical record of the eclipse episode as captured in Nature, The Observatory, proceedings from the Royal Society, and personal correspondences between Eddington, Einstein, and others. In this way we might better appreciate the relevant relative expertise of the participants, the relationship between Eddington and the scientific community to which he testified, his methods and manner of presentation, and the reception afforded his testimony by the

British scientific community. In the absence of much corroborating or conflicting testimony from others, what sorts of considerations guided his contemporaries in their acceptance of his work and its sweeping implications? Just how blind was the trust accorded Eddington's expert testimony in this case?

12:30-14:30 **Lunch & Annual General Meeting/**
Dîner & Assemblée Générale Annuelle
Physics 107

15:00-17:00 **Stillman Drake Lecture/**
Conférence Stillman Drake
Physics 107
Chair/Président : Gordon McOuat (King's)

David Kaiser
(Program in Science, Technology, & Society and Department of Physics, Massachusetts Institute of Technology)

Toil, Trouble, and the Cold War Bubble: Physics and the Academy since World War II

17:00-19:00 **Department of Philosophy Reception**
PCS Atrium, Commerce Building
All members of CSHPS welcome!