Canadian Society for the History & Philosophy of Science
Société Canadienne D'Histoire et Philosophie des Sciences
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**Abstracts**

**TUESDAY MAY 26TH**

**Session I.1.a**
**9:00 – 10:30 am**
**B249 Loeb**
JOINT SESSION BETWEEN THE CANADIAN SOCIOLOGICAL ASSOCATION AND CSHPS-SCHPS
*Examining the Public-Political-Academic Nexus in North American Sociology, 1930s-1990s*

9:00–9:20 am
Neil McLaughlin, "A Comparative Analysis of American Public Intellectuals from the 1950s and 1960s"

The lively public intellectual debate has not yet combined historical analysis and social science methodology in the ways this paper seeks to do. Drawing from a systematic sampling procedure and detailed citation analysis in a range of social science and intellectual journals, this paper will offer an account of the rise and fall of the major public intellectual sociologists, psychologists and anthropologists in the United States from 1956 to 1990. The empirical part of the paper involves an analysis of the citation pattern of a cohort of 13 highly cited but also famous book writing scholars, including David Riesman, C. Wright Mills, Margaret Mead, Erich Fromm and Seymour Martin Lipset. Citation data over a period of 40 years from a range of academic journals will allow us to say something about the reputational patterns and scholarly reach of these public intellectuals, in comparative context. We will put this empirical analysis in context of the broader literature in the sociology of intellectuals, and draw out the implications for more use of both traditional historical and well as contemporary social science methods in the historical study of social science.

9:20–9:40 am
Mark Solovey, "Forging the Uneasy Partnership between Academic Social Science and the Federal Patron in Mid-20th-Century America: Sociologist Harry Alpert and the National Science Foundation"

During the 1950s sociologist Harry Alpert was the key figure in establishing the new U.S. National Science Foundation's basic policy framework for funding the social sciences. This paper places Alpert's policy work at the NSF in the context of his intellectual and professional career. First, we will consider Alpert's extensive writings on the great French sociologist Emile Durkheim. We will then examine Alpert's experiences with and concerns about government social science programs during and after WWII. With this background, we will see that at the NSF Alpert found himself dealing with old problems in a new context. In the final section, I propose that Alpert's success in crafting a viable policy framework for NSF's support of the social sciences came at a price, for Alpert's own views about the social sciences were at odds with his policy work and major trends in the social sciences that NSF's policies were associated with. This analysis, in turn, illuminates important developments and tensions within the US social science enterprise during the middle decades of the twentieth century.

9:40–10:10 am

Rick Helmes-Hayes, “John Porter and the New Liberalism in Canadian Sociology, 1950–79”

Beginning in the mid-1950s, but especially after releasing *The Vertical Mosaic* in 1965, John Porter became – arguably – Canada’s most high profile sociologist. Indeed, from 1965 to his early death in 1979, he was one of the most influential social scientists in Canada. There exists an extensive critical literature regarding his scholarly oeuvre. This paper focuses in particular on the origins of Porter’s sociological/political worldview and sense of political commitment in the tradition of British New Liberalism developed *inter alia* by Graham Wallas, Leonard Hobhouse, and Morris Ginsberg. The New Liberalism had some influence on political economy at Queen’s and Toronto early in the 20th century, but had its greatest impact via the work of economists in the federal civil service until after World War II. The paper documents Porter’s use of the New Liberalism (at once political and sociological) as an orienting framework for his entire Lebenswerk and speculates about its more general influence in Canadian English-language sociology, 1950–79.

10:10-10:30 am
Donald Fisher, “Theoretical and Methodological Shifts within the Discipline of Sociology in English-Speaking Canadian Universities, 1950–90”

This paper has four objects of concern. The first section will synthesize the literature on the history and sociology of the discipline of sociology in Canada. The second section will describe the method and the design of the broader research study from which this paper draws. This is followed by an account of the methodological and theoretical shifts that have occurred in the discipline of sociology during the latter half of the twentieth century from the perspective of full-time faculty working as sociologists in English-speaking Canadian universities. The paper ends with a conclusion that attempts to link the changes in the “internal” life of the discipline with the “external” politico-economic changes in Canadian society.

**Session I.2.a**
**11:00 am – 1:00 pm**
**516 Southam**
***Realism and Anti-Realism***

11:00–11:40 am
Kathleen Okruhlik, “Empiricist Structuralism and the Problem of Coordination”

Bas van Fraassen’s 2008 book, *Scientific Representation*, develops a position he calls “structuralist empiricism.” This position, he says, “is a view not of what nature is like but of what science is.” The structuralism in “empiricist structuralism” refers solely to the thesis that all scientific representation is at heart mathematical. In this context, the slogan “all we know is structure” means simply: science represents the empirical phenomena as embeddable in certain abstract structures that are describable only up to structural isomorphism.

The problem that faces such an austere view is the “problem of coordination.” How can a mathematical structure be said to “represent” empirical phenomena? To address this challenge, van Fraassen employs a distinction between science (the product) and the historical process that creates science. The measurement problem is addressed by focusing on process: by taking the view “from within” to establish the claim that theory and measurement evolve in a “thoroughly entangled” way. This entangled evolution is what establishes coordination. To the “loss of reality” objection, that this may allow coordination of data models with theoretical models but still does not address the question of how the mathematical models represent phenomena, van Fraassen replies with an indexical argument. This argument turns on a claim he describes as a “pragmatic tautology.”

I shall maintain that the argument fails, as does van Fraassen's larger strategy of relying heavily on pragmatic and perspectival arguments to describe how science is created, only to treat the product itself as a form of aperspectival representation.

11:40 am–12:20 pm
Daniel McArthur. "Metaphysics Constrained by Science: Ross and Ladyman's Metaphysical Structural Realism"

In their recent book, *Everything Must Go*, Ross and Ladyman defend a structuralist metaphysics that denies the existence individual entities. This view comes along with an account of metaphysics that restricts metaphysical theses to those that are consistent with or follow from well-confirmed science-the so-called principle of naturalistic closure. The principle is further constrained by the "primacy of physics constraint" that any given special science that might ground a metaphysical thesis it itself constrained by fundamental physics – *i.e.* any metaphysical thesis must be physically possible as well as consistent with the finding of a special science. Consistent with these principles, Ross and Ladyman claim that well-confirmed findings in physics, particularly in quantum theory, mandate their version of a structuralist metaphysics. By making this case they hope to rebut the critics of the sort of metaphysical structural realism they defend. In this paper I will analyse Ross and Ladyman's arguments from the perspective of their principles of naturalistic closure and the primacy of physics constraint with a view to assessing the viability of their "metaphysical structural realism" and structuralist metaphysics in general. I will draw on recent work in the philosophy of quantum field theory by MacKinnon, Cao and others in order to make the case that Ross and Ladyman's structuralism actually violates their own constraints by being inconstant with the "standard-model" in particle physics. I will conclude by outlining the basic features of a metaphysical position, from a structural realist perspective, that is in fact constant with fundamental physics.

12:20–1:00 pm
Corey Mulvihill, "Models and Modals: Scientific Models and Semantic Anti-Realism"

The realism/anti-realism debate about scientific theories originally focused over what has been called the "Miracles" argument, that is the argument that the truth or approximate truth of our theories is the only explanation for their success. As Stanford (2000, p. 266) notes this argument was "classically articulated by Popper (1963), Smart (1968), Putnam (1975, 1978), and Boyd (1984)" and more recently by Musgrave (1988) and Leplin (1997). However, with reference to scientific models, anti-realists have pointed out that the models do not actually aim at truth (Cartwright 1983 & 1989) and that scientists actually use several models which are at face incompatible to make predictions (Morrison, 2000). Realists have responded to these criticisms by arguing that models either reveal various aspects of phenomena (Giere, 2004), or that models are representations and all representations are idealizations and hence false (Teller 2004, *cf.* Frigg and Hartmann 2006). The scientific realist responses then do not argue that models are approximately true, rather they argue that models do not represent a full picture of the world. This paper will argue that such arguments have much in common with semantic anti-realism which holds that different logical principles hold in different domains. According to semantic anti-realists like Michael Dummett, debates about realism and anti-realism are really about the correct set of logical laws to apply to a domain (Dummett, 1978a). Thus Dummett asserts that realism is correct only when the principle of bivalence holds for a domain (Dummett, 1991). To resolve metaphysical debates then, we choose a logic that does not appeal to principles which are not justifiable in that domain.

**Session I.3.a**
**2:00 pm – 4:00 pm**

**516 Southam**
***Realism and Anti-Realism II***

2:00–2:40 pm
Michael McEwan, "A New Taxonomy of Idealizations, Abstractions and Approximations"

The ubiquity of idealizations, approximations and abstractions (IAAs) in the natural sciences have led many to develop broadly anti-realist attitudes towards some aspects of theories and laws. One fact about IAAs is under emphasized: many (probably most) are philosophically benign. They are well understood and their presence in scientific representations cannot play a special role in the warrant of the kinds of anti-realist conclusions drawn. Other IAAs are more interesting. The aim of this paper is to identify some of the features that distinguish the interesting ones from the benign. To this end I will outline a partial taxonomy that better captures many of the most salient features.

I first consider some of the distinctions and classifications made in the literature. Despite some useful suggestions, I argue that none cut along quite the right lines. Instead we are best served by focusing on the source of our belief that particular representations are unrealistic. Three sources are common: (1) we have independent knowledge of the target system(s) which is in conflict with the representation; (2) we have employed inferential techniques know to introduce inaccuracies; (3) the representation is in conflict, in some sense, with some background theory. These constitute three of the principle dimensions of my taxonomy and, I claim, most of the interesting IAAs fall into the third. IAAs associated with (1) and (2) are more likely to be the subject of careful investigation by scientists themselves, but those associated with (3) are, for systematic reasons, more difficult to investigate.

2:40–3:20 pm
Dana Tulodziecki, "Underdetermination, methodological practices, and the case of John Snow"

One realist solution to underdetermination is an appeal to "theoretical virtues," criteria besides the empirical evidence that are supposed to have epistemic import and break ties in underdetermination scenarios. Despite widespread appeal to these virtues, however, there has been little discussion of how to generate a robust set of such criteria. In this talk, I want to make some headway towards this goal.

I will examine a case in the history of medicine – that of the physician John Snow and his reasoning about cholera – and argue that Snow used a variety of inferential and methodological practices that led him to accept various hypotheses about cholera that were unobservable at the time and that none of his contemporaries accepted. I will argue that this case-study suggests (i) an expanded conception of the theoretical virtues, so as to include our inferential and methodological practices, (ii) that many of these practices are, in fact, epistemically significant, and (iii) that we can test for the success of these practices empirically by examining case-studies in the history of science. Analysing this case, I will explain how it (and other cases like it) can help us resolve specific cases of underdetermination. I will show that this approach issues a new challenge to anti-realists, and argue that, even if anti-realists can successfully diffuse the new objections I pose, they will at most be able to do so in a piecemeal fashion.

3:20–4:00 pm
Curtis Forbes, "Is Constructive Empiricism Less Risky than Scientific Realism?"

The constructive empiricist is willing to believe whatever the scientific realist believes regarding observables, but distinctively chooses to remain agnostic about any claims made regarding unobservables. Because it entails only a

commitment to claims regarding observables, constructive empiricism is supposed to "deliver us from metaphysics." This might be taken to imply that constructive empiricism is a less risky strategy for belief-formation than scientific realism's more risky "metaphysical" strategy.

I argue that constructive empiricism is in fact a more risky belief-forming strategy in an epistemologically important sense. While constructive empiricism is less risky in that it generally leads one to hold less false beliefs, it is also more risky in that it can lead one to hold a lower ratio of true-to-false beliefs. This is because scientific revolutions generally involve more comprehensive revisions to scientific claims regarding observables than claims regarding unobservables. This paper supports this view through case studies in the history of astronomy and chemistry. It concludes with a discussion of some epistemological and practical reasons to prefer a belief-forming strategy that aims for a higher ratio of true-to-false beliefs (*i.e.* scientific realism) over and above one that settles for less false beliefs *per se* (*i.e.* constructive empiricism).

**Session I.4.a**
**4:10 - 5:30 pm**
**516 Southam**
***Daston, Galison, and* Objectivity *Reconsidered***

4:10–4:50 pm
Danielle Hallet, "On the Subject of Goethe: Contributions to a History of Objectivity"

Lorraine Daston and Peter Galison's *Objectivity* opposes the image of the scientist as a rational, objective, and dispassionate investigator of nature with that of the intuitively guided and emotionally volatile artistic genius. The authors argue that the emergence of objectivity as an epistemic virtue in nineteenth-century scientific practices was intimately tied to a newly perceived threat to knowledge: that of the subjective self. In their discussion, Daston and Galison cite the artist's creative imposition of ideas on the world as quintessentially subjective and opposed to science.

This talk will examine Hermann von Helmholtz's conception of the virtuous scientist, focusing primarily on two papers written on the subject of Goethe's scientific work and its relation to science proper. In working out a coherent picture of Helmholtz's scientific and epistemological commitments, it will become apparent that his guiding image of the virtuous scientist did not map onto the scientific personas described by Daston and Galison. The particular position occupied by Helmholtz brings into question their claims that structural objectivity arose in response to the failings of mechanical objectivity, and that the opposition between the objective and subjective can be characterised as one between passive and active research methods.

4:50–5:30 pm
Alan Richardson, "The Virtue Epistemology of Logical Empiricist Structural Objectivity"

In their recent book, *Objectivity*, Lorraine Daston and Peter Galison argue for two important claims: first, that specific configurations or regimes of objectivity arise from specific anxieties about subjectivity; second, that regimes of objectivity are, thus, bound up with (changing) understandings of the epistemic virtues. In tracing developments in nineteenth- and early twentieth century concerns with objectivity particularly in a region of science concerned with visual imagery, Daston and Galison are left with little to say about structural objectivity, which seems to deny the relevance of images to science entirely. I will not have anything to say about structural objectivity most broadly construed, but I will argue that a form of structural objectivity that is associated with early logical empiricism – given voice in, for example, Hans Reichenbach's *Relativitätstheorie und Erkenntnis apriori* (1921) and Rudolf Carnap's *Der logische Aufbau der Welt* (1928) – can fruitfully be

considered in the light of the two broad claims scouted above. I will argue, however, that unlike the largely moral concerns with unruly subjectivity that Daston and Galison find in mechanical objectivity, logical empiricist structural objectivity is concerned with the formation of a community of virtuous knowledge workers (in science and philosophy) and, thus, with a form of objectivity motivated more by social-political than by moral concerns. The larger point of the talk is to argue that historical virtue epistemology needs to be as careful in its history of epistemic virtue and vice as in its history of large epistemological categories such as objectivity.

**Session I.1.b**
**9:00 - 11:00 am**
**517 Southam**
***History, Science, and Philosophy in the 18th and 19th Centuries***

9:00–9:40 am
Eric Palmer, "The best of all Panglosses"

Scholars have recently associated the character of Pangloss in Voltaire's *Candide* with figures such as Gottfried Leibniz, Alexander Pope, and Christian Wolff. Pangloss is a pastiche, but Voltaire had a particular target who is almost always missed, and whose writing and biography as a tutor fit Pangloss particularly well – in fact, Pangloss paraphrases him extensively in the first chapter of *Candide.* In many of his aspects, Pangloss is the specific parodic image of Noâl Antoine Pluche (1688–1761), a popularizer of science who is the author of one of the most printed and most translated works of the mid-18th century, *Le Spectacle de la Nature*. This presentation makes the case for casting Pluche as a significant opponent in Voltaire's eyes. Pluche, as well as being the most popular among contemporary physico-theologians, promoted a unified ideal of intellectual enlightenment in an epistemology wedded to pious humility that differed markedly from the approach of the philosophes. This made him a significant opponent of Voltaire, who frequently chose the strategy of lowering Pluche by disdaining to name him as an adversary. Pluche's popularity is likely to have made his position as a target apparent to Voltaire's audience, however. Voltaire's invective against Pluche has been neglected in recent scholarship, as a consequence of Voltaire's use of elliptical reference, and of Pluche's subsequent eclipse by Buffon. A look at Pluche can serve to help us build our understanding of the importance of this figure and of the significance of popular scientific writing in French culture.

9:40–10:20 am
Omar Nasim, "Data, Phenomena and Non-Propositional Observational Records"

The primary concern in this paper will be to re-examine the notion of scientific observation in light of nineteenth century practices in sidereal observational astronomy. In particular, I will give an historical account of the third Earl of Rosse's (William Parsons) practices in observing nebulae using the largest reflecting telescope at the time – built by him in 1845. I propose to follow records of a few celestial objects as they traveled through the programme's record books and publications, which ranged over a forty year period. It will be made manifest that within the "procedures" of this practice of observation what stands out more than the calculations and descriptions made of the object are the hundreds of drawings made. A taxonomy of the various uses of the images within the project of nebular research will then be catalogued and I will attempt to assess the images, as they were produced within the particular procedures of observation, in light of the propositional stance that has been unquestionably taken for their analysis. It is from the explication of such a practice that I then attempt to formulate certain general features meant to philosophically sharpen our analysis of observation. I will be aided in this by Jim Bogen and Jim Woodward's work on observation, phenomena and data, which will be used as a framework for my own analysis. In turn, it will be found that their work will require some modification,

such as a weaker emphasis on the epistemic notion of reliability, and the idea that phenomena may have also been illustrated at some point in these data-production procedures. But above all, I hope to give at least one feature of observation that many philosophers have simply ignored; namely, the role played by previous observations, recorded in the form of images or as reports, to query, guide, and direct future observations, even within one and the same observational programme. This significant aspect is usually ignored, it seems, because either a series of such observations were thought to negatively influence future work, or that observation used in the classification of objects is rarely examined.

10:20–11:00 am
Joseph Petrunic, "The Tait-Clifford debate over mathematical foundations (1870–80)"

In the mid-19th century, the motivation to produce new mathematical techniques in Great Britain came, in part, from the engineering needs of industrialization (especially in northern Britain). This motivation fuelled much of the scientific research pushed forward by P.G. Tait (1831–1901) and William Thomson [later Lord Kelvin] (1824–1907), whose experiments in, and conceptualizations of, "energy," "work," and "thermodynamics" helped to construct the "science of energy." These developments were fed and shaped by prominent Presbyterian religious views regarding the creation and dissipation of the universe as a whole. By contrast, at University College, London in urban Britain the overriding ethos was one of secularism and practical education. Demonstrated in a particularly vibrant form through the efforts of W.K. Clifford (1845–79), this overarching philosophy in mathematics could be defined as boldly empiricist, conventionalist, and anti-metaphysical.

A significant divergence in belief as to the origins and nature of mathematical knowledge existed between these two geographical and academic communities. In brief, P.G. Tait viewed mathematical statements as a handmaiden to science, the ultimate goal of which was to depict a divinely designed universal/natural order. W.K. Clifford, on the other hand, conceptualized mathematical statements using Darwinist language that reflected his belief in the evolving nature of mathematical and scientific knowledge. Ultimately, mathematics was uncertain, empirical and inductive. The explanation for this divergence can be located by juxtaposing the religious context within which Presbyterian northern scientists such as Tait operated to the proudly secularist (and even atheistic) atmosphere within which London's practitioners, including Clifford, operated. In comparing Tait and Clifford's works from the mid-1860s to the late-1870s, I will demonstrate that the empiricist and secular philosophy of mathematics that emerged in London in the 1870's was largely a reaction to, and rejection of, northern British, religiously-minded interpretations of the foundations of knowledge in general.

**Session I.2.b**
**11:10 am – 1:00 pm**
**517 Southam**
***Politics, Pedagogy and Popularization in Victorian Science***

11:15–11:50 am
Gordon McOuat, "Diffusion of Really Useful Knowledge: A Victorian Challenge to Interest Free Science"

The Victorian period provided our very model of a modernist major general value-free science and closely associated "views from nowhere." Yet that categorical view did not remain unchallenged. Against the educational and scientific reform initiated by the highly influential utilitarian/reformist "Society for the Diffusion of Useful Knowledge" (SDUK) and its *Penny Magazine* stood a radical artisanal network of autodictatic knowledge makers and local Owenite societies wholly unhappy with the SDUK outright ban on political and social discussion. At the core of the dispute lay the problem of the politics of science and the diffusion of knowledge. This paper will track the rise and fall of the radical alternative "Society

for the Diffusion of Really Useful Knowledge" (SDRUK) and the lost Victorian attempt to merge the scientific and the political.

11:50am–12:25 pm
Bernie Lightman, "Popularizing Evolution in Children's Books"

With the exception of scholarly studies of Charles Kingsley and Arabella Buckley, little has been written on books on evolution that targeted a young reading audience in the second half of the nineteenth century. Seth Lerer's chapter "On Beyond Darwin" in his recently published *Children's Literature* provides an overview of the impact of Darwin on children's literature. He argues that Darwin's way of discussing natural development contained an imaginative dimension that helped to shape the children's literature of the late nineteenth and early twentieth centuries, using Water Babies as his prime example. He also discusses how various writers explored the Darwinian themes of adaptation (*e.g.* Rudyard Kipling) and the relationship between humans and animals (*e.g.* Kipling and H. G. Wells). However Lerer does not examine what versions of evolutionary theory were popularized, and he focuses exclusively on works of fiction. There were several attempts in this period to use the genre of non-fictionalized children's literature to convey the meaning of evolutionary theory. These works did not begin to appear until the late 1870's, which suggests that Water Babies notwithstanding evolution was a difficult topic to tackle when writing books for the young. In this paper I will deal with biographies of Darwin and accounts of his Beagle voyage written for children by British and American authors in the last few decades of the 19th century. I will discuss how the authors of these books attempt to sanitize evolutionary theory in order to make it fit for their young audience.

12:25–1:00 pm
James Elwick, "'An inconvenient test': Victorian examinations, metrology, and accountability"

The Victorian era was one of examinations. Such devices promised to reform administration by selecting competent employees, and to establish common standards in a largely private and fragmented educational system. Because of such hopes one can understand this mania for exams by deploying HPS-STS tools used to study accountability and metrology. As common standards, and as forms of accountability, exams can be analyzed in two ways. Seen through a Foucauldian lens, exams facilitated the surveillance of student populations while also constraining possible modes of education. Conversely, however, Theodore Porter's work can be used to show how exams allowed marginalized groups to exert moral claims on stronger groups and institutions.

This "Porterian" perspective is the focus of today's paper, which claims that English reformers of female education used exams-as-standards to appeal for justice in their successful campaign for greater female participation in formal education. The paper also looks at how exams were used to demonstrate women's intellectual equality with men, even in apparently "male" subjects such as mathematics.

**Session I.3.b**
**2:00 - 4:00 pm**
**517 Southam**
***Communicating Knowledge in Early Modern Science***

2:00–2:40 pm
Kathryn Morris, "Fiction and Philosophy in Margaret Cavendish's *The Blazing World*"

In 1666 Margaret Cavendish published *The Description of a New World, Called the Blazing World*, the fantastic tale of a beautiful lady who finds herself shipwrecked on another world joined "pole to pole" to our own. She is made Empress of the strange new kingdom and quickly imposes political and religious reforms. She also spends a great deal of time discussing the principles and methods of natural philosophy with the world's inhabitants (which include Bear-men, Ape-men, and Lice-men). Cavendish published *The Blazing World* as a companion-piece to her *Observations Upon Experimental Philosophy*, a more-or-less straightforward treatise in which Cavendish sets out her own materialist, vitalist natural philosophy while criticizing the experimental approach of the Royal Society. Cavendish suggests that *The Blazing World* was intended in part as a pleasing diversion from the serious philosophical discourse of the *Observations*. However, she also describes the story as one part philosophical (in addition to being one part "romantical" and another part pure fancy). In this paper I will argue that Cavendish's science fiction, though bizarre, serves her philosophical project in two ways: first, *The Blazing World* allows her to assert that the first principles of her natural philosophy would hold across all possible worlds. Secondly, the text brings out important connections between Cavendish's natural philosophy and her politics. As I will argue, *The Blazing World* illustrates the ways in which both political order and disorder are, for Cavendish, rooted in the relationship between the natural and social worlds.

2 :40–3 :20 pm
Louis Sagnieres, "Science, Confiance et Internet"

Cet article propose d'étudier la relation Internet–science à partir de la notion de confiance. Celle-ci est en effet essentielle pour comprendre le fonctionnement de la science moderne qui n'est plus le fait de chercheurs indépendants et solitaires, mais de communautés complexes et interdépendantes. Notre analyse de ce concept lui ôtera toute connotation morale et proposera d'y voir l'expression d'une fiabilité. Partant de cette idée, il sera possible d'analyser le développement de l'activité scientifique et, par exemple, les efforts de la Royal Society et de R. Boyle, comme un processus d'amélioration et de garantie de la confiance que les scientifiques ont pour les résultats de leurs expériences. Cependant, aujourd'hui on constate une crise de confiance envers les « institutions » traditionnelles qui garantissaient la fiabilité de l'activité scientifique. Impossibilité de détecter certaines fraudes, remise en cause du système des comités de lectures, *etc.* La deuxième partie de cet article sera donc consacrée à l'idée que le développement des technologies de l'information et de la communication permet de mettre en place un certain nombre de mesures qui peuvent garantir la confiance nécessaire au bon développement de la science. Les concepts de "folksonomie" et de "social bookmarking" y seront centraux.

3:20–4:00 pm
Ian Stewart, "Thinking inside the box: the 'new logic' of Francis Bacon"

Early-modern natural philosophical texts are very difficult for contemporary readers to read because of the care with which they were composed. This care is accutely present in those "canonical" texts that sought to establish some form or other of "new" philosophy, due to the clear but decidely complex problem associated with persuading readers who were deeply inculcated in established ways of conceiving nature to see, think and speak differently.

Francis Bacon offers a particularly strong example. The work of Graham Rees (most forcefully in recent volumes of the Oxford *Francis Bacon* series) has for years drawn attention to the fact that all of Bacon's major works on natural philosophy are deeply informed by the fact that he had a very carefully worked-out system of natural philosophy, but one that he kept largely hidden from public view. Ironically, this system is most influential (though hidden) in the pages of Bacon's foundational work of induction, the *Novum Organum* (1620), a work that encapsulates for many accounts of the Scientific Revolution the "classic" picture of Baconian science: the triumphant denunciation of dogmatic adherence to natural philosophies; the open-ended exploration of nature for the sake of new knowledge; the painstakingly patient empiricism of a particularly English bent that the period itself (and subsequent histories) so loved to set in opposition to

Cartesian rationalism. The "classic" Baconian picture, still alive and well in the discipline of HPS, has yet to fully reflect on this irony.

My paper will highlight three features of the *Novum Organum* that, together, require us to rethink this classic text, and the "classic" Bacon. The similarities of the Bacon I will describe to the Descartes of the *Discourse*, *The World*, or even the *Meditations* should provide grounds for rethinking some of categorizations and oppositions in our accounts of early-modern science and philosophy.

**Session I.4.b**
**4:10-5:30 pm**
**517 Southam**
***Systems and Networks in the History of Technology***

4:10–4:50 pm
Janis Langins, "Vauban's theories of fortification and Vauban's disciples"

Sébastien le Prestre de Vauban (1633–1707) occupied an almost iconic status not only in his native France but among military engineers in Europe and even America as well. The Vauban "systems" were studied in military academies well into the nineteenth century and a significant proportion of debate on fortification took place within the framework of his ideas. This paper will discuss some ways in which his public image and his ideas were perceived and often distorted by followers and critics alike. I will argue that this story illuminates more fundamental issues like the respective roles of theory and practice in engineering and the status of fortification as an autonomous discipline. Vauban also emerges from the story as someone who would have vigorously rejected the place assigned him in the historical Pantheon by those invoking his name. Instead he can be situated in the position of an encylopaedist *avant la lettre.*

4:50–5:30 pm
Leslie Tomory, "Building a stable network: gas in London 1812–20"

Gaslight was invented and deployed in the early years of the nineteenth century. The first commercial installations were at mills and factories in northern England in the period 1805–11, but it was not this model that came to be the dominant form for the new technology. Rather, it was as a large urban utility. The period from 1812–20 witnessed the successful construction of a large scale gas network was in the city of London.

The transformation of the technology from discrete on-site installations into a large stable network posed various technological, business, political, and social challenges, many of which were not anticipated by the promoters of the new utility. The technological challenges included finding ways to ensure stable gas supply in a constantly expanding dynamic system by the invention of devices such as valves and syphons, as well as the adoption of techniques to mitigate leaks. The stability of the new system depended equally on social and political factors, and the new company found it had to educate its users in how their habits affected the overall system, as well as having to devise means of controlling users who were unable or unwilling to moderate their consumption.

**Session I.1.c**
**9:00 – 10:55 am**

**A602 Loeb**
***Philosophy of Evolutionary Biology I: Thinking about Selection***

9:00–9:40 am
Jill Oliver & Shannon Dea, "Darwin and Sex Revisited"

In her seminal article "Have Only Men Evolved?" (1979) Ruth Hubbard scathingly indicts Charles Darwin's theory of sexual selection for its reliance on "androcentric...false facts." Undoubtedly, Darwin's sexist assumptions and related blindspots frequently betray themselves in his *The Descent of Man*, and *Selection in Relation to Sex* (1871). Despite this, a number of passages in *Descent* (passages that Hubbard does not discuss in her article) overtly treat the differences between the sexes as differences in degree rather than kind. These passages suggest the possibility that Darwin was on his way to rejecting essentialism and binarism about sex. If this is right, then he was an important precursor of such contemporary scholars as Anne Fausto-Sterling and Suzanne Kessler, both of whom reject the male-female binary in biology. Darwin's account of the continuity between the sexes is particularly striking in that it runs counter to Victorian medical trends which relied on sex essentialism, as described in Alice Dreger's *Hermaphrodites and the Medical Invention of Sex* (1998). This paper considers Darwin's discussions of the human sexes in Descent in light of (*inter alia*) Fausto-Sterling and Dreger, in the process excavating much in Darwin's account of sex that escapes (or should escape) Hubbard's criticism.

9:40–10:20 am
Peter Gildenhuys, "Explaining the Persistence of Biological Altruism without Invoking Group Selection"

Group selection is often invoked to account for the how altruistic behaviors, ones beneficial to the reproduction of others but costly to the altruist, can persist in biological populations (*e.g.* Sober and Wilson 1998). I show how to explain the persistence of altruistic behaviors in biological populations without invoking group selection. I show what features biological populations must have in order for standard "group selection" models to be applicable to them; these features can be tied to the mathematical formalism used to calculate population dynamics in a one-to-one fashion. Though populations must form temporary subgroups in order for standard group selection models to be applicable to them, they need not be described as undergoing "group selection" or "multi-level selection" at all. I conclude that the features I pick out as what license the deployment of "group selection" models over natural populations are the features that explain their dynamics, while "group selection" explains nothing about them.

10:20–11:00 am
Eugene Earnshaw-Whyte, "Selection and Drift: An elimination of process"

Natural selection and drift are often conceived as evolutionary forces; processes that causally influence the evolution of populations. Authors such as Millstein and Sober have also argued that natural selection and drift can be identified as evolutionary outcomes, thereby drawing a distinction between drift and selection as process, and as product. This paper analyses their suggestion, arguing in favour of a three-fold distinction between force, causal variable, and product. The term process as employed in the literature is ambiguous between a distinct causal mechanism (such as the force of gravity) and a causal variable (such as mass). It is denied that drift is a force and argued that natural selection can occur as a product even when no force of natural selection is operating. It follows that, on the usual understanding employed in evolutionary biology, the existence of the force of natural selection is explanatorily irrelevant to determining whether a population evolves by natural selection. The paper concludes by suggesting that evolutionary forces be characterised in terms of drift and selection with regard to their expected outcomes in a given environment.

**Session I.2.c**
**11:15 am – 1:00 pm**
**A602 Loeb**
***Philosophy of Evolutionary Biology II: Systems and Phylogenies***

11:15–11:50 am
Lisa Gannett, "Trees, Trellises, and the Garden of Eden"

The importance of lateral gene transfer in prokaryotic evolution has led biologists and philosophers to question the tree of life. It is argued that there is no coherent prokaryote species concept, that life has evolved as a web not a tree, that there is no last universal common ancestor, and that there is no guarantee that evolutionary history can be reconstructed. Because of reticulation due to gene flow and lineage melding, efforts to achieve a phylogenetic reconstruction of human evolutionary history face similar scientific and philosophical challenges. There are methodological challenges: Is it possible to overcome the charge that methods used to construct phylogenetic trees assume treeness but do not prove it? How are group boundaries drawn, and is there only one way to do this? There are also epistemological challenges: Is it possible to establish an original progenitor population for all humans? What data would decide between Templeton's trellis and Cavalli-Sforza's tree? Under what conditions should we expect trees constructed using different genes to coincide, and can evolutionary history be reconstructed best at particular levels (gene, chromosome, population)? Is reconstructing the whole of human evolutionary history feasible? And there are metaphysical challenges: Can we justifiably assume that there is a real underlying tree? Is realism about intraspecific groups (subspecies, races, populations) justified? Reflecting on reconstructing evolutionary history for a single species, where population genetics models traditionally rule, helps us to explore what it might mean for a population genetics model to replace cladistics in the investigation of microbial evolution.

11:50 am–12:25 pm
Kirsten MacDonald, "Evolutionizing Culture: Can It Be Done?"

In their 1999 paper, "Does Culture Evolve?" Joseph Fracchia and Richard Lewontin argue that culture cannot usefully be explained via the principles of Darwinian natural selection. They argue that evolutionary accounts of culture fail for three reasons. First, none of these accounts have identified a unit of culture. Second, Darwinian principles do not yield explanations of cultural change superior to those offered by historians. Third, these evolutionary theories disappear the complexities of culture. I argue that Fracchia and Lewontin's challenges ought to be taken seriously, not as damning in-principle objections to the very project of "evolutionizing" culture, but as useful guides to the kinds of things for which successful evolutionary models must account. I argue that at least two current research programmes for evolutionizing culture, although young, can meet Fracchia and Lewontin's challenges: memetics and developmental systems theory (DST). Surveying these approaches, I show that each has, in fact, identified a unit of culture – memetics, the meme, and DST, the life cycle - and can account for the complex realities of culture and cultural change. As the aims of history and of evolutionary accounts of culture are very different, applying the principles of natural selection to culture can do some interesting and useful explanatory work that cannot be done by the social sciences alone.

12:25–1:00 pm
Frédéric Bouchard, "How the definitions of community and ecosystem constrain how we define the evolution of symbioses"

Ecological communities are generally defined as the assemblage of all (or most) interacting species in a given area, ecological niche or environment. The most spectacular examples of deeply integrated communities are symbiotic communities where

organisms of different species interact in such a way as to increase the degree of interaction between the species involved. Communities are defined solely by the biotic entities included in it. This is not the case for ecosystems that are generally defined as the assemblage of all communities and their abiotic (physical, chemical, geological, climatic) environment. The analysis of these higher order systems often borrow conceptual tools from engineering to understand the interplay between the various components of ecosystems qua systems. By examining the case of the Hawaiian bobtail squid (a bioluminescent symbiotic community) I will argue that conceptualizing many cases of symbioses as ecosystems instead of communities offers novel explanatory benefits to understanding their evolution.

**Session I.3.c**
**2:00 – 3:55 pm**
**A602 Loeb**
***Darwinians and Non-Darwinians in Context***

2:00–2:30 pm
Hannah Gay, "Chemist, Entomologist, Darwinian, and Man of Affairs: Raphael Meldola and the Making of a Scientific Career"

For much of his professional career Raphael Meldola FRS (1849–1915) was professor of chemistry at the City and Guilds Technical College at Finsbury in London. Today he is best remembered for his work in dye chemistry, but his first love was field entomology. For the conference I propose to present a section from a longer paper with the above title, and one based on work carried out in several archives. Given that this is a Darwin anniversary year, I will focus on Meldola as a champion of the Darwinian cause. The paper will show something of Meldola's early interest in Lepidoptera and how, when he was in his early twenties, he promoted Darwinian ideas at the Entomological Club, despite the Club being run by senior entomologists such as J. O. Westwood and H. J. Stainton who were highly sceptical of the new ideas. But the young Meldola managed to attract a number of older mentors. One of the first was the adventurer John Keast Lord who had spent several years as a naturalist in British Columbia. Alfred Russel Wallace, too, became a mentor and close friend, as did Henry W. Bates. But Meldola's most important patron was Charles Darwin who encouraged his work and paved his way into the Royal Society, the *sine qua non* of his later career success. The paper will show both how Meldola was able to attract the attention of Darwin and how, after Darwin's death, he saw it as his duty to promote Darwinian ideas. Indeed, Meldola was another of Darwin's bull dogs. Meldola's closest ally in the Darwinian cause was Edward Poulton whom he met at the Entomological Society in 1883. At that time Meldola was already well known in entomological circles for his work on insect mimicry, and for his translations of works by August Weismann and Fritz Müller. Poulton, later Hope Professor at Oxford, was then at the start of his career as an entomologist, but the two formed a close and lifelong friendship. Some of the battles they fought in the Darwinian cause will be discussed, including those against a younger generation, especially the Cambridge followers of Hugo de Vries and William Bateson, for whom natural selection was simply one idea among many, and by no means the most important.

2:30–3:00 pm
Georgy Levit, "Bernhard Rensch's Panpsychistic Identism and the Modern Evolutionary Synthesis"

Towards the end of the 1930s, Bernhard Rensch (1900–90) turned from Lamarckism and orthogenesis to selectionism and became one of the key figures in the making of the Synthetic Theory of Evolution (STE). He contributed to the Darwinization of biological systematics, the criticism of various anti-Darwinian movements in the German lands, but more importantly founded a macroevolutionary theory based on Darwinian gradualism. In the course of time, Rensch developed his version of the STE into an all-embracing theoretical system combining Darwinian methodology which Rensch labeled

"Pansychistic Identism." Pansychistic Identism, propagating the idea of gradual development of the psychic side of the universe beginning with the pre-phenomenal stage of matter represented a Spinozism of sorts. Thus being a Darwinian ("Synthetic") at the purely empirical-descriptive level, Rensch became a controversial philosopher, whose claims went far beyond the conventional "biophilosphies" of other major figures in the Synthetic movement.

Since Rensch's Identism is not "a philosophy of a biologist," but a meta-methodological principle underling the entire system, my analysis of Rensch's methodology can be seen as a case study of the problem of the heterogeneity of the Modern Synthesis. The scale of this heterogeneity is, in fact, so significant that the picture of the Synthesis as a unified movement needs to be deconstructed.

3:00–3:30 pm
Ian Hesketh, "Mythologizing the Oxford Debate of 1860"

The famous Huxley-Wilberforce debate that occurred during the BAAS meeting of 1860 has become a key event in crude historical narratives written by the likes of "New Atheists" and other popular writers who simplify the relationship between science and Christianity as one of incommensurability. Christopher Hitchens, for instance, recently argued that the debate was a "tipping point" in the battle between science and Christianity, a battle where "Huxley cleaned Wilberforce's clock, ate his lunch, [and] used him to mop the floor." Professional historians have largely discounted this crude version of the debate, arguing that Huxley's "victory" was not so one-sided, and that the "debate" itself was of little consequence. How could this seemingly inconsequential debate become such a mythologized event in the popular imagination? This paper reconstructs the way in which the "Oxford debate" became a myth by focusing on the careful remembrance and dissemination of a particular version of events that was cultivated and communicated within a close circle of friends, a version of events that was later publicized in both Darwin's and Huxley's "Life and Letters" in the late nineteenth century only to be reproduced in early histories of science promoting the "warfare" between science and Christianity. The twentieth century continued this historical mythmaking through popular historical reconstructions such as the BBC produced series "Voyage of the *Beagle*" and the Down House heritage project. Just as professional historians demythologize the debate, Huxley's version of events continues to find space in the popular media.

3:30–4:00 pm
Trevor Pearce, "The Spencer-Weismann Dispute and Alternative Evolutionary Mechanisms in the 1890s"

The 1890s were a time of change for American biology and psychology: important universities, societies, and laboratories were founded, and scientists began to argue in earnest about the relevance of different factors in organic evolution. In this paper, I will argue that the alternative account of the evolutionary process presented by Conway Lloyd Morgan, James Mark Baldwin, and Henry F. Osborn in 1896 responded directly to the clash between Herbert Spencer and August Weismann over the mechanisms of heredity and evolution. Spencer's evolutionary views, as summarized in *Factors of Organic Evolution* (1887), were already influential by the time Weismann's theory of inheritance became well known through Alfred Russell Wallace's book *Darwinism* (1889) and the English translation of Weismann's *Essays* (1889). By 1890, Osborn was already discussing the Spencer-Weismann dispute in the *New York Times*, despite the fact that Spencer and Weismann did not engage in public debate over the inheritance of acquired characters until 1893–95. Morgan, Baldwin, and Osborn explicitly placed their new ideas in the context of this debate, stating that they had discovered a new factor of evolution not described by Spencer and Weismann. Today, biologists are still engaged in arguments over macroevolutionary mechanisms – arguments that mirror the debates of the 1890s. A closer examination of these historical debates can offer a new perspective on modern discussions of the role of the Baldwin Effect, plasticity, and variation in the evolutionary process.

Session I.4.c
4:10 – 5:30 pm
A602 Loeb
*Nietzche, Darwin, and Darwinism*

4:10–4:50 pm
Lukas Soderstrom, “Nietzsche on Exaptation, Heredity and Evolution”

This paper examines Friedrich Nietzsche’s interest in evolutionary science. It examines his reading of German embryology and physiology to show how he developed a conceptual understanding of what is now called “exaptation.” Nietzsche’s description of “exaptation” stems, most notably, from the works of the German embryologist Wilhelm Roux who stressed the importance of developmental biology for evolutionary theory and inserted what Darwin had initially called the “struggle for existence” into the organism. In *Der Kampf der Theile im Organismus* (1881), Roux argued that as an individual organism’s parts (cells, tissues, organs) grow and develop they come into contact and struggle with each other. According to Roux, this agonistic interaction between developing organic parts leads not to extinction but to one part eventually ascribing a function to another part, which structures the whole organism’s physiology. Nietzsche borrowed Roux’s account of an inner organic struggle and used it to argue that organisms are determined first by inner processes and only later by their interaction with their milieu. From his reading of Roux, Nietzsche also developed the idea that only a part’s ability to be affected and interact with other parts, its irritability, is hereditary and not its function. In this context, Nietzsche’s prefiguration of “exaptation” only pertains to the non-hereditary attribution of functions during an organism’s development. Thus, Nietzsche relegated adaptation by natural selection to a secondary role and rejected the heredity of acquired traits.

4:50–5:30 pm
Ben Mitchell, “The Ends of Science in the Shadow of Nietzsche”

The connection between the thought of the German philosopher Friedrich Nietzsche and the history of science is becoming increasingly clear. The sciences of his time, Darwinism, thermodynamics, materialism and the idea of progress all shaped his thoughts on the nature of science, truth, history and causality. The first part of this paper will be dedicated to establishing Nietzsche’s critique of science through his encounter with thermodynamics and evolution, and contextualizing it by looking at several of the well known scientific figures of his age such as Charles Darwin, Herbert Spencer, and Lord Kelvin. The second part of the paper will consist of a more thorough analysis of what Nietzsche has to say about science in general and how his specific concepts of the will to power and the eternal recurrence influence his claims. Using these twin concepts, we will then have the necessary background to see how Nietzsche viewed science as overcoming itself, and by which processes it achieves its downfall, and elevation.

**WEDNESDAY MAY 27**

**Session II.1.a**
**9:00 am – 11:00 am**
**235 MacOdrum Library**
***Models and Ontologies***

9:00–9:40 am
Ryan Samaroo, “What does ‘contained in but not derivable from’ mean?”

The title quotation is from Chapter 7 of *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence* by Robert Batterman. Here and elsewhere, Batterman claims that, although the governing equations of our more fundamental physical theories may be said to contain universal behaviours (often in the form of scaling solutions and other kinds of structural stability) that emerge in the study of their asymptotic domains, we require conceptual resources from our coarser theories in order to interpret or understand these behaviours. These resources are foreign to the more fundamental theories, and it is in this sense that they are not derivable from them.

I take the interpretation of this quotation to be the main source of confusion in Batterman's debate with Gordon Belot, Michael Redhead, and others over the explanatory role of "coarser" theories (typically older, macroscopic, phenomenological) in our consideration of various physical systems that are also described by allegedly more fundamental theories (typically newer and molecular dynamical). I propose to (i) identify and clarify the core of their disagreement, (ii) show that the meaning of this quotation turns on different underlying concepts of explanation, and (iii) clarify how coarser theories may explain without reifying or otherwise requiring the existence of the entities and structures over which they quantify.

9:40–10:20 am
Alex Manafu, "Configurational forces and the emergence of the chemical"

As Brian McLaughlin explains in his article on the rise and fall of British Emergentism (1992), this philosophical doctrine is committed to the existence of "configurational forces" – *sui generis* non-physical (*e.g.* chemical, biological, psychological) forces, which (i) occur only when certain configurations of physical particles obtain; (ii) are brute, fundamental and therefore irreducible to forces within the purely physical domain, and (iii) are capable of downward causation. While thinking that such forces do not conflict with the laws of physics, McLaughlin claims that "there is no scintilla of evidence that there are configurational forces in chemistry or in any other domain." However, Hendry (2006) has recently questioned this claim by arguing for the existence of "configurational Hamiltonians" occurring in the Schrödinger equation describing molecular systems. I show that the kind of emergentism which relies on configurational forces or Hamiltonians is not only at odds with the causal closure of physics, but also with the principle of conservation of energy. I argue that, in order to rescue the emergence and autonomy of the chemical, one need not go so far as to propose the existence of configurational forces and the incompleteness of physics they entail. I draw on the work done by others (Wimsatt 1997, Batterman 2002) to suggest that an interesting (and more refined) concept of emergence of the chemical can still be had without needing to appeal to problematic configurational forces or Hamiltonians.

10:20–11:00 am
Andrew Wayne, "Idealization and Explanation in Physics"

Scientists since Galileo have explained natural phenomena making central use of approximations. These explanations involve reasonably accurate models that are good (albeit simplified) representations of physical systems. Indeed, it is precisely the representational accuracy of models that is taken to underwrite their explanatory power. However, a signal development in contemporary physics is the widespread use, in explanatory contexts, of highly idealized models that do not seem to fit this "Galilean" approach. Examples include statistical mechanical models at criticality and limit cycle models in nonlinear dynamical systems. Scientists appeal to these sorts of idealizations in their explanations, but we lack an account that makes sense of this practice. The idea that non-approximative idealizations may underwrite bona fide scientific explanation goes against orthodox views of scientific explanation. Ultimately I want to claim that at least some highly idealized models in physics have genuine explanatory power, and I want to extend the explanatory role for such idealizations beyond the scope of current philosophical work.

**Session II.2.a**
**11:15 am – 1:00 pm**
**235 MacOdrum Library**
***Models and Ontologies II***

11:15–11:50 am
Anjan Chakravartty, "Fundamental Physical Entities and their Properties"

A number of recent philosophical examinations of quantum theory and general relativity have generated a provocative thesis regarding the ontology of the fundamental entities these theories describe. The idea is that things like subatomic particles and space-time points have relational properties only, and consequently, no intrinsic properties. Let us call this the Relational Ontology Thesis. On its face, this thesis appears to clash with seemingly widespread, common sense intuitions about the ontology of concrete things more generally: an entity must have at least some intrinsic property or properties in order to exist; and a fortiori, in order for us to make sense of the idea that it stands in any sort of relation. For in the absence of anything intrinsic, so the worry goes, what is it, precisely, that stands in the relevant quantum mechanical or general relativistic relations? In this paper, I consider the coherence of an ontology of very basic things whose properties are all relational. I will contend that arguments for the Relational Ontology Thesis incorporate certain equivocations in applying the predicates "intrinsic," "extrinsic," "monadic," and "relational" to fundamental physical properties. Once these confusions are resolved, motivations for the idea that basic physical entities have only relational properties are undermined. As a result, it would appear that in this respect, fundamental ontology as described by our current best theories in physics are reconcilable with common sense intuitions about ontology after all.

11:50 am–12:25 pm
Alcibiades Malapi-Nelson, "Cybernetics' machine ontology"

It is common to point to a lack of funding as the main reason for Cybernetics' demise. Artificial Intelligence (AI), an area that sprang from cybernetic research itself, having begun to compete for the same financial resources, was soon to take over its contender for consideration. This common account also suggests that, having constructed some successful models of abstract reasoning, the robustness of AI's modeling convinced the funding sources to choose the latter over Cybernetics as the better investment. This left the cybernetic project without needed resources for survival.

Considering the tremendous excitement that surrounded Cybernetics at its peak, followed by such an end, a question remains. What is the subsequent history of the philosophical framework(s) within which they developed? My thesis is that, besides the usual accounts indicated above, there is an intellectual factor that also contributed to the demise of Cybernetics. This has been missed by most commentators. It would seem that Cybernetics was a paradigmatic instance where the model of a machine, *tout court*, played an absolutely fundamental role in the theory's development. Further, it can be argued that a surreptitious transformation in the cybernetic understanding of both the nature and behaviour of its machine-model, gradually led to internal tensions that amounted to the eventual demise of the project. Indeed it might be the case that the role of Cybernetics' own operative model, which contributed to the success of – and to the hype about – the enterprise, could have also carried the philosophical seeds of its own later implosion.

12:25–1:00 pm
Eran Tal, "Simulation, Measurement and Accuracy"

Current discussions concerning the epistemology of computer simulations tend to assume that the accuracy of computer simulations is ultimately evaluated against the results of measurements. These discussions presuppose that measuring

instruments are themselves accurate. Here I argue that this presupposition puts the entire discussion at risk of circularity. This is because both types of accuracy are based on very similar inferences. That is, the accuracy of both measurement procedures and computer simulations is grounded in (i) structural analogies between abstract mathematical models and (ii) mappings between behavioural patterns of physical systems that approximately realise these models. The method of establishing analogies of this kind is known as calibration. By presupposing that calibration procedures are adequate, the discussion concerning computer simulations is assuming the validity of the very inference it is supposed to justify.

I illustrate this point with a study of the procedure by which the Bureau International des Poids et Mesures (BIPM) determines UTC time. The stability of UTC time depends on a complex aggregation algorithm that periodically compares results from hundreds of atomic clocks. Each of these results in turn depends on applying a cascade of corrections based on a hierarchy of mathematical models. As I show, the calibration of atomic clocks exhibits essentially the same structure of justification as the calibration of computer simulations.

**Session II.3.a**
**2:00 – 5:30 pm**
**235 MacOdrum Library**
**JOINT SESSION BETWEEN THE CANADIAN PHILOSOPHICAL ASSOCATION AND CSHPS-SCHPS**
***SYMPOSIUM***
***Inconsistent Models: Truth, Context, and Perspective***

Ronald N. Giere "Incompatible Theoretical Perspectives"
Margaret Morrison, "Inconsistent Models: Problems and Perspectives"
Alex Rueger, "'Incompatible' Models for Realists"
Anjan Chakravartty, "Perspectivism, Inconsistent Models, and Contrastive Explanation"

**Session II.1.b**
**9:00 am – 11:00 am**
**A720 Loeb**
***Methodological issues in Modern Medicine***

9:00–9:40 am
Tracy Finn, "Classification and Diagnosis of Mental Illness: Insight from Autism Spectrum Disorders"

I use autism spectrum disorders as a case study to investigate the conceptual and methodological problems that plague the current diagnostic system in psychiatry and clinical psychology. The *Diagnostic and Statistical Manual* (DSM) uses a symptom-based approach to diagnose particular mental illnesses and developmental disorders, including autism spectrum disorders (ASD). Basing diagnosis on symptomatolology alone, while ostensively improving reliability of diagnostic categories, leads to several confusions regarding the boundaries between categories of illness, and accurately identifying the particular disorder(s) from which an individual suffers.

Poland *et al.* (1996) and Murphy (2006) argue that diagnosis of mental illness must be based on causal etiology, rather than on symptoms, since this is the only way to improve the current conceptual and methodological shortcomings of the DSM. Poland *et al.* (1996) argue that the current symptom-based approach mistakenly assumes that mental illnesses are

"syndromes with unity," where the symptom profiles of each illness are good indications of a stable, discrete underlying disorder. I defend the arguments made by these critics of the current diagnostic framework, using ASD to illustrate the specific difficulties that can arise in the diagnosis of illnesses like ASD that are chronic, complex and highly co-morbid with other socio-cognitive impairments. Based on the diagnostic problems that arise in ASD, I also present a proposal for how to incorporate causal information into the diagnosis of these disorders, and how such information can improve and refine the current symptom-based criteria to better identify autism subtypes, co-morbid illnesses, and the relationship between illnesses in the autistic spectrum.

9:40–10:20 am
Roger Stanev, "Epidemiologic Causation: a Causal Connection between Smoking and Lung Cancer"

A central issue confronting both philosophers and practitioners in formulating an analysis of causation is the question of what constitutes evidence for a causal association. From the 1950s onward, the biostatistician Jerome Cornfield put himself at the center of a controversial debate over whether cigarette smoking was a causative factor in the incidence of lung cancer. Despite criticisms from distinguished statisticians such as Fisher, Neyman and Berkson, Cornfield argued that a review of the scientific evidence supported the conclusion of a causal association. Cornfield's odds ratio in case-control studies-as a good estimate of relative risk-together with his argument of "explanatory common cause" became important tools to use in confronting the skeptics. In this paper, I revisit this important historical episode as recorded in the *Journal of National Cancer Institute* and the *Journal of the American Statistical Association.* More specifically, I examine Cornfield's necessary condition on the minimum magnitudes of relative risk in light of confounders. This historical episode yields important insight into the nature of causal inference by showing the sorts of evidence appealed to by practitioners in supporting claims of causal association. This study will also lead us to suspect that for practitioners, causal notions may be revised in response to new problems and new techniques.

10:20–11:00 am
Robin Nunn, "Randomized Controlled Anecdote: A Story of What Works in Medicine"

In evidence based medicine, randomized controlled trials (RCTs) are said to be the best evidence of what works in medicine. While RCTs are highly valued, together with derivatives such as systematic reviews and meta-analyses of RCTs, individual stories ("mere anecdotal evidence") have low value or are not considered to be medical evidence at all. Similar hierarchical views of evidence have infected other disciplines, such as evidence based education and evidence based government. In this discussion, I explore the artificial divisions of acceptable from unacceptable evidence, numbers from narrative and science from humanities. In particular I challenge the deprecation of stories in medicine. Evidence of what works in medicine depends on the context, the story, the history. The accepted story is provisional until a more plausible story comes along. Some stories are based on experiments while others are based on more or less plausible theories. Some stories offer vast and impressive statistics gathered from many observations while others single out one noteworthy event. Reports of RCTs are themselves stories of what experimenters did. Systematic reviewers generate their own observations of the collected stories of RCTs. Reviewers of systematic reviews in turn report their observations of systematic reviews. In practice, all of these stories become evidence of what works in medicine.

**Session II.2.b**
**11:05 am – 12:30 pm**
**A720 Loeb**
***Methodological issues in Modern Medicine II***

11:10 - 11:50 am
Frank Stahnisch, "German-Speaking Neuroscientists in Canada after 1933: Critical Reflections on Emigration-Induced Scientific Change"

Focusing on theoretical concepts and scientific applications of "interdisciplinarity" in neuroscientific research, the proposed paper discusses a time period, which has long been marginal to the scholarly work of historians of the life sciences and medicine. On the basis of ample biographies of neuroscientists, psychiatrists, and neuropathologists as well as case-histories of individual research institutions, emigration specialists have argued that the loss of nearly 30% of all senior neuroscientists in Germany between 1933 and 1945 ruined the country's foundation for investigations in brain research. It has also been emphasized that the reintegration of differing communities of neuroscientists into the research culture on the other side of the Atlantic initiated a strong enhancement of knowledge production and led to a gradual transformation in this scientific field. But like in many other contemporary disciplines and research areas, a firmly corroborated evaluation of the effects of the massive exodus of scientists on the growth, the content, and the international standing of the neurosciences in North-America and Germany is still lacking.

This paper concentrates particularly on the Canadian example. Although Canada, due to its tight immigration restrictions since the 1920s, did not receive as many émigré neuroscientists as for example the United States or Great Britain, the individuals who could recommence a scientific career in the Canadian neurosciences made however a remarkable difference in various regards (*e.g.* in research, teaching, and institution building). While looking at the cultural, social, and institutional levels of "emigration-induced scientific change" (C. Fleck 1996), a number of biographies shall be discussed (such as Heinz Lehmann, Karl Stern, Robert Weil, Martin Silberberg, *etc.*) and put into the perspective of changing neuroscientific cultures in the 1940s and 1950s. Then, certain types of "gains" and "losses" in differing research styles and programs are evaluated in the light of those biographies and their new work situation as refugees in Canada.

It is the objective of this paper to (a) put forward a more adequate model for evaluating emigration-induced scientific change in the field of the neurosciences, which also encompasses a cultural and social description of the research activities. It shall further be argued that (b) the traditional focus on outstanding "revolutionary neuroscientists" among the refugees proves to be insufficient to explain major changes in the development of the field, when the intermediary level of investigation, scientific societies, and research institutes is left out. Here, the collective biographies of the émigré researchers and clinicians can help to develop a fuller picture of the emerging field of neuroscience in the middle of the 20th century.

11:50am–12:30 pm
Patricia Liu, "Persuading Outsiders: The Case of R.J. Reynolds and Its Support of the Prion Research Program (1980–88)"

This paper explores the tobacco company R.J. Reynolds' (RJR) support of Stanley Prusiner's research program on prions, particularly its role in sustaining the research program in the early 1980s. Prions were novel protein-only infectious agents hypothesized to cause transmissible spongiform encephalopathies (TSEs), a class of neurological disorders including scrapie and mad cow disease. Although not tobacco-related, Prusiner's research suited well the objective of the RJR grant program to fund more unorthodox research in the field of degenerative diseases. RJR support was critical to the success of the prion research program. It provided the financial means to attain the technology, research personnel, and animal resources for Prusiner to embark on innovative and large-scale experiments. Prusiner was able to collaborate with renowned scientists, thereby reshaping his research program and TSE research more generally into an interdisciplinary enterprise. This case study also provides insights into the means by which Prusiner persuaded outsiders. He emphasized the novelty and unorthodoxy of his research and constantly made appeals based on the "wonder" and "application" of prion research. A constant dialogue between Prusiner, scientific consultants, and RJR executives was fostered. As a result, Prusiner garnered

a small yet influential number of supporters including Frederick Seitz and Maclyn McCarty who constantly promoted prion research to private funders and the wider scientific community. They provided Prusiner with credibility and were a persuasive force in the appeal to outsiders, particularly in the early years of the research program when criticism was harshest at the specialist level.

**Session II.3.b**
**2:00 – 3:20 pm**
**A720 Loeb**
***Modern Canadian Science and Technology***

2:00–2:40 pm
Ian Slater, "The Taegeukgi and the Maple Leaf: The Pursuit of South Korean Export Markets by Atomic Energy Canada Ltd"

In the 1980s Canada's nuclear technology company, Atomic Energy Canada Limited (AECL), designed and attempted to sell a next-generation, small-scale nuclear reactor called the Slowpoke Energy System (SES). AECL pursued export markets for the SES, and by far the most promising was South Korea. The SES project was forced to compete for funding, and this necessitated the formation of partnerships with private and public sector agents in South Korea. AECL's experience in South Korea suggests that Crown Corporations are more commercially oriented than established policy scholarship allows, and that in some cases competitive forces work to blunt innovation rather than reward it.

2:40–3:20 pm
Richard. Milligan & Tyler McCreary, "The Historic and Contemporary Use of Indigenous Knowledge in Northern Resource Management Studies"

The incorporation of Indigenous knowledge into the systemized framework of the bureaucratic and scientific aspects of resource management is neither novel nor disentangled from the complexities and violence of our colonial history. Indeed the initial colonial encounter was itself a period marked by colonial administrators thoroughgoing engagement with Indigenous knowledge, which was appropriated to serve the aims of expansionary European enterprises such as the Hudson's Bay Company. In this paper we situate contemporary efforts in Canada to incorporate the knowledge of Indigenous peoples into resource management processes not as a break from but rather in congruence with the last two centuries of colonial knowledge relations in the north. We begin with an exploration of the travelogues of Samuel Hearne, taking a close and critical look at the mechanisms and discourses through which eighteenthcentury colonial exploration literature, equipped with new global enlightenment schemes of cataloguing knowledge (*e.g.* Linnaeus), readily incorporated the kind of Traditional Ecological Knowledge (TEK) that has recently been advocated in resource management practices in Canada. Engaging with Usher and Stevenson, two contemporary proponents of the use of TEK in northern Environmental Impact Assessments, we question whether incorporating TEK really serves as a means of neutralizing colonial power relations that continue to plague relations between communities and cultures, or whether the processes of reframing Indigenous knowledge within globalizing frames that permit and authorize administrative control from distant centres simply replicate the appropriation of environmental knowledge that first enabled colonization.

**Session II.4.b**
**3:30 – 5:30 pm**
**A720 Loeb**
**JOINT SESSION BETWEEN THE CANADIAN HISTORICAL ASSOCIATION AND CSHPS-SCHPS**

***Institutional Authority and the Authority of Science in Alchemical, Medical and Political Contexts***

Commentator: Trevor H. Levere

3:30–4:10 pm
Victor D. Boantza, "Alchemical Agendas, the New Science, and Institutional Authority at the Early *Académie Royale des Sciences*"

Samuel Cottereau Duclos (1598-1685) established the laboratory and the (al)chemical research program at the newly inaugurated *Académie Royale des Sciences* (est. 1666). In the years following his prestigious election, Duclos was among most active founder-members and enjoyed an unmatched level of intellectual authority within the royal institute. By the mid-1670s, however, Duclos' status and influence had weakened markedly. The origins of this institutional power decline are interpreted in light of Duclos' systematic preference of solution chemistry over the traditional distillation practices, highlighted by his research into Alkahest, the alchemical Universal Solvent. The assessment reveals metaphysical contentions within the Academy concerning the nature of matter as well as the scope of scientific research and the role of empirical evidence. Duclos' resistance to what he perceived as the unwarranted mechanistic reductionism of Cartesian thought was at odds with the precepts of an increasingly mechanist community of natural philosophers, which was closely dependent upon royal funding while openly committed to the glorification of the Crown. Drawing on alchemical and Hermetic notions, Duclos advanced an animistic and Neo-Platonic cosmology, considering solvents as vital-chemical and hence superior to physical-mechanical distillation. The contextualization of this commitment to solution analytic (al)chemistry evaluates the anti-alchemical propensities of the *Académie* during the 1670s, throwing light on the relations between Neo-Platonism, religion and institutional censorship during the tumultuous years which saw Malebranche's attempt to reconcile Augustinianism and Cartesianism.

4:10–4:50 pm
Erich Weidenhammer, "Reputation, Patronage and Natural Knowledge: John Pringle and the Royal Society"

John Pringle (1707–82) was a profoundly influential writer, physician, and experimentalist within the gentlemanly world of Georgian medicine and natural philosophy. At the height of his career, he was president of the Royal Society (1772–78) and personal physician to King George III. His seminal work, the *Observations on the Diseases of the Army in Camp and Garrison* (1752), argued for hygienic improvements in an Enlightenment attempt at institutional reform. Throughout his subsequent career he promoted and defended this medical doctrine, which drew upon a series of significant chemico-medical experiments for which he won the Royal Society's prestigious Copley Medal. During the British Enlightenment, scientific influence was closely related to personal reputation and institutional affiliation. As a prominent and active member of the Royal Society, Pringle was able to assist less established practitioners whose work supported and promoted his own agenda. During his presidency of the Royal Society, for instance, the Copley Medal was twice given for closely related work. His reputation as an arbiter of scientific knowledge was also tied to his position within the Society. The learned discourses given at the Medal ceremonies, a tradition that Pringle began, were published posthumously along with his biography. Like his portrait by Joshua Reynolds, donated to the Society in 1778, these discourses illustrate his efforts to establish a lasting legacy linked to this prominent institution, thus exemplifying the dynamics among institutional authority, patronage, and the status of scientific knowledge within the context of Enlightenment culture.

4:50–5:30 pm
Jaipreet Virdi, "Medical Authority and Medicalized Institutions: John Harrison Curtis & the London Asylum for the Deaf and Dumb"

Almost all nineteenth-century British institutions founded for the reception, education, and maintenance of the deaf and dumb were established only for purposes of instruction. Due to a medical and social prejudice that labelled congenital deafness incurable and to the efforts of teachers opposing surgical experimentation, students at these institutions were denied clinical treatments for their deafness. However, some institutions, such as the London Asylum for the Deaf and Dumb (est. 1807), saw medical experimentation and treatment as integral to their educational practice; by relying on socio-educational reform ideas of medical authority, these institutions transformed the governance of their pupils by moving away from a strictly instructional focus. This paper analyzes the impact of medical authority upon institutional governance and policies by looking at the career and influence of one of the Asylum's most important medical advocates, surgeon-aurist John Harrison Curtis (1778-1860).

Curtis published extensively on the state of aural surgery and the institutional treatment of the deaf and dumb. His *Treatise on the Physiology and Diseases of the Ear* (1817) underwent six editions and established his reputation as a skilful aurist-surgeon. Curtis' later works, especially his *Essay on the Deaf and Dumb* (1829) reflect his compassionate attempts to extend his medical authority towards the treatment of deafness within institutional settings. In 1817, Curtis played an instrumental role in transforming the London Asylum's guidelines for admission by recommending to the Committee of Governors to appoint an aurist to inspect all children entering the institution and if possible, administer relief for hearing loss. By examining Curtis's recommendations, this paper chronicles the earliest approach towards the medicalization of the deaf within the London Asylum, as well as the historical shift away from symbolic education (such as sign language) towards the medical and surgical treatment of cases of deafness in nineteenth-century Britain.

**Session II.1.c**
**9:00 - 11:30 pm**
**301 Paterson**
***Science, Culture and Authority***

9:00–9:40 am
Kristen Hardy, "Racialization, Animality, and the Boundaries of the Victorian Human"

Throughout the history of European societies, the nature and location of the distinction between human beings and other animals has not remained constant or stable, but has varied according to an array of social, political, economic, and philosophical priorities and circumstances. In Britain, concern regarding the human/other boundary became especially acute during the Victorian period. Often found concomitant with unstable boundaries, however, is pervasive anxiety, particularly when social order is perceived to be at stake. This paper investigates some of the ways in which the nascent discipline of anthropology provided Victorian Britons with a means of addressing the ambiguity and unease around contemporary perceptions of the human/nonhuman demarcation in a way that was undergirded by-and, in turn, fostered-the growing acceptance of racial science during the period. By examining the work produced and published by the Anthropological Society of London (ASL), I consider how the oft-elastic cultural line of delineation between humans and other animals was discursively reconstituted and reinforced in part by anthropological currents of thought which deployed racialization as a way of allaying the fears of white Victorians over the then-indefinite linkages between themselves and the nonhuman world.

9:40–10:20 am
Boaz Miller, "The Social Epistemology of Values in Science: Breastfeeding and the Science of Good Motherhood"

I offer a new perspective on the role of social values, *e.g.* political views and ideologies, in science. The value-ladeness of science is relatively uncontroversial among HPS scholars. It is commonly argued that social values "fill the gap" of underdetermination of theory by evidence. I explore another role of social values in science that has been largely overlooked: substituting evidential support of theories. I suggest that social values not only fill the gap between theory and evidence, they also determine what counts as good evidence in the first place. According to the underdetermination model, when some evidence allegedly contradicts a socially favourable theory, scientists can reinterpret the evidence to explain it away. The underdetermination model implicitly assumes that all evidence is equally strong. By contrast, under my model, which draws on Hacking's notion of styles of scientific reasoning, not all evidence is equally strong. Rather, each piece of evidence has some initial weight on its own, and social values add to or derogate from it. My model differs from both social constructivist and traditional epistemology theories of knowledge. While constructivists deny that evidence has independent weight outside any social context, traditional epistemologists deny that social values have an epistemic role to play. I illustrate my model by examining the changing scientific theories about the virtues and vices of breastfeeding versus bottle-feeding. I suggest that these changing views have more to do with changing views about the role of women and mothers in society than the scientific evidence for and against breastfeeding *per se*.

10:20–11:00 am
Vivien Hamilton, "Can Trading Zones Have Experts?"

Galison's trading zones have become popular for modeling interdisciplinary collaboration (Galison 1997). But these trading zones, developed initially to model the exchange of information, skills and instruments between the subcultures of physics, explicitly assume an equality between the trading partners. Neither group's language nor worldview is privileged and the pidgin language that develops between them can come to refer to objects in common without requiring an agreement on the full meaning of the terms in use. But interdisciplinary collaboration often involves a demarcation of expertise, with one group or both claiming epistemic authority over particular objects or properties of objects. Is it still a trading zone if one group is asked to teach the other the "right" way of describing an entity or of making use of an instrument? Using the interaction between British physicists and doctors in the first decades of radiology as a case study, I will argue that Galison's trading zone fails to capture the dynamics of interdisciplinary communication in which one group takes on the role of teacher or expert.

11:00–11:40 am
Michael da Silva, "Paddling towards cultural synthesis: the canoe in Ontario museums"

This paper examines the representation of the canoe in three Ontario museums in order to better understand how museums can help contribute to the perception, rooted in the work of C.P. Snow, that the arts and the sciences constitute two separate and distinct "cultures." After recapping Snow's position, it notes that his political thesis is similar to the epistemological thesis of the social constructivists and examines the implications of the belief that knowledge of a given object is constituted by its cultural position. This leads to an examination of a specific object, the canoe, and its representation in three museums, each of which is perceived to have a distinct cultural agenda. First hand accounts, newspaper reviews and primary documents from the construction of the exhibits are used to explore how the canoe is represented in each museum. Ultimately, it is argued that the representation of canoes in Ontario helps to sustain the division between the arts and the sciences.

**Session II.2.c**
**11:40 am – 1:00 pm**
**301 Paterson**

***History and Philosophy of Mathematics***

11:50 am–12:25 pm
Geordie McComb, "The Mathematical Aesthetics Thesis in the Philosophy of Science"

This paper critiques the justification provided for the "mathematical aesthetics thesis" in Eugene Wigner's *The Unreasonable Effectiveness of Mathematics in the Natural Sciences* and Mark Steiner's *The Applicability of Mathematics as a Philosophical Problem.* The mathematical aesthetics thesis – the claim that the human aesthetic sense is essential to the classification of concepts as mathematical – is an essential premise in Wigner;s argument for the miraculous appropriateness of the language of mathematics for the formulation of the laws of physics. Additionally, it is also an essential premise in Steiner's challenge to naturalism; that is, he argues that because anti-naturalistic Pythagorean and formalist classification schemes grounded crucial analogies in the fundamental discoveries of modern physics, our universe appears to be "user friendly." Wigner and Steiner justify their respective versions of the mathematical aesthetics thesis differently. Regarding Wigner, I argue (1) that his claim that mathematical concepts arise either from experience or from the human aesthetic sense is a false dichotomy, and (2) that he provides insufficient justification to deny that mathematical concepts arise from experience. Regarding Steiner, I question his justification for the claim (1) that the distinction between mathematical and non-mathematical structures is non-logical, and (2) that mathematicians classify conceptual structures as mathematical because they please the human aesthetic sense. Ultimately, I argue that both authors' main arguments in their respective works are weakened by these criticisms.

12:25–1:00 pm
Jean-Philippe Villeneuve, "The Abstraction Process at the Beginning of Measure Theory in the late 19th century"

We find several ways of calculating the measure of a set in the late 19th Century. Peano presented the geometrical magnitudes in 1887, and used them to calculate the length (the area, the volume) of a set of points in $\mathbf{R}$ (in $\mathbf{R}^2$, in $\mathbf{R}^3$ respectively). In 1892, Jordan proposed the notion of content to calculate the measure of a set in $\mathbf{R}^n$. In 1904, Lebesgue generalized the notion of content by introducing the outer measure. After having a quick look at these generalizations, we will analyze Borel's work of 1898 in which we found the first abstraction of the notion of measure. Indeed, Borel proposed a new way of defining the measure of a set: instead of providing a way of calculating the measure of a set, he defined the notion by a list of properties. We will see that the choice of these properties is not arbitrary but based on properties that are used in proofs. In 1904, Lebesgue provided a new list of properties that extended the scope of application of Borel's definition; he successfully linked the theory of integration with the theory of measure. We will thus conclude by comparing Borel's definition to Lebesgue's definition and also to the modern definition of measure.

**THURSDAY MAY 28TH**

**Session III.1.a**
**9:00 am - 1:00 pm**
**517 Southam**

***SYMPOSIUM***
***Entanglements of Instruments and Media in Investigating Organic Worlds***

This panel will explore the entanglements between instruments and media in the investigation of organic worlds. We take a broad view of instrumentation, to include diverse exploratory practices in the life sciences, some more material than others. In experimental practice, an instrument cannot be separated from the media that it probes. And yet, any experimental

apparatus relies on a range of different kinds of media and instruments whose identity and relationship to one another is unstable.

In some experimental settings in the life sciences, the media being probed is an object or an organism. In this sense, organic materials are treated as kinds of excitable media that can manifest responses to inquiry. Yet, organisms or their parts can in turn act as both media and instruments. They can be used as measuring apparatus, their responses indicating a sensing or reading of phenomena otherwise imperceptible. They can function as models or exemplars of natural processes, organic or non-organic, aiding the conceptualization of particular phenomena. In some situations instruments and media become indistinguishable, as in the digital media through which computer graphic models and simulations are rendered. In this sense, instruments are media for enacting vital processes, artefactual iterations of the phenomena of life.

The symposium will explore the roles of instruments and media for investigating organic worlds at various scales and in various settings. Individual papers will examine organic worlds from cells to tissues, organisms to environments. The sites of these experimental investigations will also be examined, to highlight the scenes of inquiries, the actors engaged in them and their social roles.

Tara Abraham, “On the mind and brain: Investigative practices in 20th-century neurophysiology and psychology”

A common narrative in the history of 20th-century human sciences states that prior to the so-called “cognitive revolution” of the 1950s, something called “the mind” was considered anathema among scientific psychologists, and following the revolution, the mind became a legitimate object of scientific study. This paper aims to problematize this narrative by re-examining the extent to which the relation of mind to brain was deemed a legitimate scientific question among both psychologists and neurophysiologists in mid-twentieth-century America, the forms that this question took, and the investigative practices used to pursue it. I will argue that the distinction between the pre-1950s human sciences and those that emerged later was less about “no mind” *vs*. “mind” than about interrelated changes in epistemic goals, practices, and disciplinary relations. While psychologists had generally rejected discussions of biological or physiological functions prior to the 1950s, neurophysiologists embraced discussions of higher psychological functions, using a variety of investigative practices: anatomical, surgical, physiological, experimental, and theoretical. However, by the end of the 1950s, psychologists such as Donald O. Hebb had signaled a pronounced shift: the mind, Hebb argued, was indeed amenable to scientific study, and explicable in terms of physiological phenomena; for Hebb, psychology is a decidedly biological science. In examining the reasons for this shift, the paper will comment on the contrasting meanings of “mind” in psychological and neurophysiological discourse and provide a nuanced picture of the disciplinary and epistemological relations between neurophysiology and psychology in mid-twentieth-century America.

Aryn Martin, “Recalcitrant instruments, objects and investigators in the non-invention of a non-invasive prenatal genetic diagnosis technique”

This paper tells the story of “NIFTY,” a multi-laboratory, multi-million dollar, decade-long clinical trial whose aim it was to isolate fetal cells from pregnant women's blood for prenatal genetic testing. Despite initial optimism about the simplicity of the task at hand, an abundance of both resources and good will, and constant efforts to discipline the humans, machines and cells involved in the network, the outcome was a judged by the participants to have been a failure in its aim of demonstrating the feasibility of such a technique. When interviewed, participants agreed that the results were disappointing, but there were as many justifications for the failure as there were interviewees. Explanatory resources ranged from funding rubrics (“it should never have been called a clinical trial”), to problems of trust, to differences in tacit knowledge, skill and patience (“some researchers just have better hands”), to problems of consensus (“we lacked a common protocol”), to instrumental sensitivity, to recalcitrance of the phenomenon (“it’s like looking for a very small needle in a very large

haystack"). Hence, the failure was distributed across the delicate socio-technical apparatus of the collaboration. While investigators treat the huge study as a somewhat embarrassing footnote in their careers, it presents a number of puzzles for S&TS analysis. Inspired by Hans Jorg Rheinberger's ideas, this paper explores the effect of this trial on the status of the fetal cell as an epistemic thing, and the unexpected success of this collaboration as a "generator of surprises," both material and social.

Nicole Nelson, "Generating 'anxiety like phenotypes' in the elevated plus maze: A measure of mouse anxiety or a model of human anxiety?"

In the laboratory, mice are often used as models for humans, entering into experimental configurations in ways that humans cannot. But, they are also organisms with their own natural histories and behaviors. In this paper, I will explore how these two visions of the mouse are blended together in the study of anxiety. Using ethnographic data from a behavior genetics laboratory, I will look at how researchers measure anxiety in mice with a behavioral test called the elevated plus maze (EPM). The EPM consists of two long platforms arranged in a "plus" shape, where one platform is protected by walls and the other platform is open. The mouse's level of anxiety is measured by comparing how much time it spends in the protective closed arm versus the exposed open arm. Researchers argue that this test is valid both because takes advantage of the natural instincts of the mouse (mice are naturally fearful about exploring exposed places like the open arms of the maze) and because it has been confirmed by using human anti-anxiety drugs (when human drugs are given to the mice, they will spend more time in the open arms). These two explanations show two different senses in which the mouse is being used as a model: as a biological detector for drug effects, and as an entity capable of experiencing something "anxiety like" that is similar to human anxiety.

Joan Steigerwald, "Defining life in the eighteenth century: Instrumental reasoning, excitable matter and living subjects"

This paper will examine the debates over irritability in the mid-eighteenth century. Albrecht von Haller is attributed with making irritability central to animal function in the 1750s by defining it as the capacity of muscle fibers to respond to stimulus and contract. But many physiologists resisted the introduction of a new capacity of irritability that did not fit into extant conceptual frameworks. Haller's definition of irritability and the place he gave it in the animal economy was particularly contested by the "nerve patrons" as it denied sensibility and the nervous system the governing role in all animal function. Indeed, irritability had an equivocal status amidst the competing physiological systems of the eighteenth century. The dispute over irritability also became entangled with other areas of natural philosophy and medicine from chemistry and pharmacology to electricity.

Haller's definition of irritability was instrumental, based on the perceived responses to stimulus in living animals. The many trials provoked by the dispute brought to attention new phenomena of organic vitality and the problems arising in the experimental investigation of organisms. Despite Haller's repeated attempts to provide definitive instrumental demonstrations, to inscribe his conception of irritability into the organic material, the phenomena manifested in experiments on living animals and organic materials remained variable and unstable. The debates surrounding irritability not only made it into a significant matter of concern, but also enlivened it as a phenomenon, making it more complex than Haller's initial definition suggested. In the process, the instrumental reasoning introduced to define the organic properties of irritability and sensibility by Haller was shown to involve fundamental indeterminacies; both the instruments and the judgments made with their assistance were opened up to critical interrogation. In asking questions of organic parts, instrumental investigations gave life to organic matter; but life also answered back, posing questions of instrumental reasoning. Reading and indeed writing the signs of organic vitality involved entanglements of instruments, organic material and living subjects whose meanings were ambiguous.

Mathieu Charbonneau, “Extended Thing Knowledge”

In his book *Thing Knowledge*, Davis Baird claims that 1) scientific instruments constitute scientific knowledge and 2) that viewing instruments this way yields a better understanding of scientific change. This is a radical claim. It is not that instruments yield knowledge when used properly, they are knowledge *simpliciter*. If Baird is correct, we have to change the traditional conception of knowledge, namely that knowledge is justified true belief. In this paper, I argue that by using the extended mind concept introduced by Clark and Chalmers, it is possible and fruitful to offer an analysis of scientific instruments that allows us to keep the ingredients that seem to me positive in Baird’s analysis as well as a less radical modification of the traditional conception of knowledge.

The extended mind concept implies that any entities falling under a functionalist account of a given cognitive process is to be considered a genuine part of the cognitive system in which the process takes part. This means that mental states such as beliefs might take place outside the brain of the cognitive agent if there is a material system that falls under the functional account of such mental states. I will show that many scientific instruments used by scientists do indeed play the functional role of belief for the scientist and that we must then understand scientific instruments as genuine beliefs.

**Session III.1.b**
**9:00–11:00 am**
**520 Southam**
**Epistemic Values and Evidence**

9:00–9:30 am
Wayne Myrvold, “Belief, Value, and Theory Choice”

There are a number of choices that scientific researchers have to make-such as, for instance, which hypotheses to investigate, or which experiments to undertake-in consideration of which we ought to take into account, not only tangible costs and benefits, but also possible gains in knowledge that may or may not have tangible consequences. It can be useful for some purposes to model such choices as attempts to maximize expected utility, with epistemic value of having a certain belief-state contributing to utility. For instance, having a high degree of belief in a true theory might be regarded as valuable, with a higher value accruing to high degree of belief in a theory that affords deeper understanding. There are a number of interesting issues raised by the introduction of epistemic values. One of these is the possibility that these values might play a role in setting our degrees of belief: do the so-called epistemic virtues, such as simplicity, explanatoriness, and the like, have a legitimate role to play in assessment of the credibility of a theory (as distinguished from other decisions we might make, such as whether the theory is worth pursuing)? Whether or not epistemic values role will play a role in adjusting an agent’s belief-state depends on the form of the epistemic value function, in a way that is easy to specify, and I will discuss some of the implications of adopting epistemic value functions that lead to their playing a role in adjusting degrees of belief.

9:30–10:00 am
Mike Thicke, “Bayesian Statistics in Gravitational Wave Astronomy”

In *Social Epistemology*, Steve Fuller accuses traditional epistemology of committing the fallacy of composition: “that any correct account of individual knowledge can be, *ipso facto*, generalized as a correct account of social knowledge.” Granting this claim, a question arises over whether criteria for individual knowledge are merely insufficient when applied to social

knowledge, or whether they may conflict with criteria for social knowledge. By examining a case in gravitational wave astronomy, my talk argues that the debate between the use of Bayesian and traditional statistics in science might hinge on such a conflict.

In 2002, a group of astronomers claimed to have found interesting statistical anomalies in their data suggesting their detector had been triggered by gravitational waves. Their colleagues argued that their data was "not exceptional by any of the usual standards of evidence for a significant result," while they claimed that Bayesian reasoning in relation to their data ought to significantly increase one's beliefs that gravitational waves have been observed. I argue that they are correct – there are good grounds for individuals to alter their beliefs – but their critics are also correct: there are not good grounds for making a publically-acceptable claim of detection. Bayesian criteria for adjusting beliefs are acceptable on an individual level, but in this case are not good criteria for social knowledge.

10:00–10:30 am
Antoine C. Dussault, "Putnam and science value-neutrality"

Putnam argues against ethical relativism by way of criticizing what he calls the "fact/value dichotomy." According to him, fact and value are not separated in a clear-cut dichotomy, but rather "entangled." This entanglement happens in two ways. First, epistemic values such as "coherence, simplicity, preservation of past doctrine, and the like" guide scientists in their choice of one theory over another (*The Collapse*, p. 31). Secondly, thick ethical concepts are not classifiable in a sharp fact/value dichotomy. A concept like "cruel" is sometimes used to evaluate (*e.g.* "My child's teacher is very cruel"), and sometimes used to describe (*e.g.* "Vlad the Impaler was an exceptionally cruel monarch"). Putnam suggests that this entanglement compels us to break with science value-neutrality. Because of the role of epistemic values in theory selection and that of thick ethical concepts in defining important notions in social science (*e.g.* Amartya Sen's "capacities"), we can no longer hope, or even require (like Max Weber did) that science be value neutral (*The Collapse*, p. 63). However, Putnam does not want to be a relativist, and that makes it harder for him (harder than for philosophers like Rorty and Feyerabend) to break with science value-neutrality. In my paper, I want to show that if we look closer at Putnam's arguments, we see more ambivalence than what appears at first sight. Furthermore, I want to show that we can in fact grant Putnam all his arguments without breaking with the essential core of science value-neutrality.

10:30–11:00 am
Jacob Stegenga, "Paradoxes of Amalgamating Multimodal Evidence"

Rain today, I reckon, given the grey clouds above, the falling barometer, and after all, it is an autumn day in England. My conjecture is supported with multimodal evidence: the clouds, the barometer, the season. The term "multimodal evidence" will be unfamiliar to most, and so I begin by introducing and describing the notion. Multimodal evidence must be aggregated. Once evidence is thought of in this way, it suggests an analogy between aggregating preferences and judgments – a burgeoning topic in social choice theory – and aggregating multimodal evidence – a neophyte topic in philosophy of science. Aggregating individuals' preferences into a group decision faces several well-known impossibility theorems, including Condorcet's voting paradox and Arrow's theorem, and aggregating individuals' judgments into a group judgment faces what has been called the "doctrinal paradox," which has led to impossibility theorems for judgment aggregation. I briefly describe these paradoxes, and draw the analogy between amalgamating judgments and amalgamating multimodal evidence. The analogy is meant to pump your intuition that amalgamating multimodal evidence faces impossibility theorems similar to those of preference and judgment amalgamation. I end by demonstrating such paradoxes for evidence amalgamation. This paper makes small steps toward explicating the logical space of possibilities for multimodal evidence amalgamation functions. More hopeful, though, is the demonstration that the analogy between preference/judgment amalgamation and multimodal evidence amalgamation allows for a substantial import of results from the rich literature on

amalgamating preferences and judgments to the neophyte literature on amalgamating multimodal evidence. This paper merely hint at the possibilities.

**Session III.2.b**
**11:10 am – 1:00 pm**
**520 Southam**
***The Logic of Science and the Status of Laws***

11:15–11:50 am
Duncan Maclean, "Best Systems Analysis and the Problem of Undermining"

In 1986 David Lewis identified a problem for his Humean theory of objective chance. Lewis took chance to supervene on the global distribution of properties. The latter entails a theory of chance that gives maximal chance to actual history A coming to pass. But a theory of chance also gives some small chance to alternative history B coming to pass. If B were to come to pass, the theory of chance undermines itself, since B entails an alternative theory of chance that gives B the maximal chance of coming to pass. Lewis gave the problem of undermining proper expression as a contradiction that shows up in the Principal Principle, a principle of reason about how chance is related to credence.

Michael Thau's (1994) solution to the problem was to correct the (in)admissibility condition for the Principal Principle. A proposition stating a theory of chance is thus inadmissible relative to B, since the former provides direct evidence that bears on B and leads to undermining.

In my paper I argue that Thau's solution is ad hoc and leads to a new problem for Lewis. On Thau's solution, the criterion of fit for best systems with probabilistic laws cannot be defined. If "fit" goes undefined, then best systems for indeterministic worlds cannot be assigned. Following Thau, the best systems account of laws fails for indeterministic worlds, a result that seems as worrisome for Lewis's theory of chance as was the problem of undermining.

11:50 am–12:25 pm
Travis Dumsday, "Scientific Essentialism and the Global Laws Problem"

In the ongoing debates concerning the ontology of laws, scientific essentialism (SE) remains one of the most widely discussed options. Advocates of SE, most notably Brian Ellis (2001 & 2002), maintain that the laws of nature are grounded in the essences of natural kinds, and particularly the causal powers associated with those essences. The theory has faced a number of criticisms, one of the more important being the global laws problem: if laws are rooted in natural kinds, how does one explain the fact that some laws apply to all of them without exception? If laws do not govern kinds but instead are grounded in them, as SE maintains, this seems a remarkable coincidence. This point has been raised by Adams (1992), Foster (2004), Katzav (2005), Lange (2004), Lieb (1985), Lowe (2001), and Swinburne (2004). Ellis suggests in reply that global laws are rooted in the essences of worlds rather than of objects. But this idea has met with stiff resistance, notably in Chalmers (1999), Elder (1994), Katzav (2005), and Lange (2004). My contention is that this worry for SE can be addressed. However, doing so requires that SE be placed within one of two broader ontologies, which precisely mirror the major options raised in debates over the cosmological anthropic principle. My final conclusion is that discussion of the ontology of laws should no longer be conducted in isolation from those debates.

12:25–1:00 pm

Aaron Barth & Steve Bland, “The Goals of Carnap’s Reconstructive Program”

In her *Second Philosophy* (2006), Maddy argues that we must interpret Carnap as either engaged in the epistemological task of providing an account of a priori knowledge, or as engaged in the more comprehensive goal of ridding philosophy of its pseudo-questions. In light of what she sees as Quine’s definitive criticism of the analytic-synthetic distinction, she opts for the latter reading. On this reading, Carnap ends up advocating for a sophisticated version of epistemological relativism: answers to philosophical questions amount only to our pragmatically motivated decision to adopt one linguistic framework over another. Our view, however, is that Maddy has created a false dilemma. We will argue that Carnap’s project of ridding philosophy of its pseudo-questions rests on his account of a priori knowledge as non-factual knowledge. For Carnap, philosophical questions, such as those at the heart of the crisis in the foundations of mathematics, admit only of pragmatically motivated decisions precisely because their answers lack empirical content. The goals of Carnap’s rational reconstruction of scientific theories are, consequently, both to vindicate this conception of a priori knowledge, and on this basis, to provide a motivation for replacing traditional metaphysics with the discipline that he calls the “logic of science.” This being the case, Carnap’s position stands or falls with the analytic-synthetic distinction.

**Session III.1.c**
**9:00 am – 10:55 am**
**417 Southam**
***Natural Kinds***

9:00–9:40 am
Neil Williams, “Must Natural Kind Essences be Intrinsic?”

Recently, a number of views have appeared in the literature defending or describing what might be called “neo-essentialism” about natural kinds. These views are essentialist to the extent that they endorse the claims that members of a natural kind possess an “essence” that is both necessary and sufficient for membership in the kind, and that essences causally explain other properties commonly associated with the members of the kind, but they purport to break from (and improve upon) the traditional essentialism of Kripke and Putnam by rejecting the claim that essences must be comprised of intrinsic properties. However, I argue that this so-called break from traditional essentialism is not a break at all, as the widespread interpretation of Kripke and Putnam according to which they take essences to be intrinsic is mistaken. Putnam makes no claim to the effect that essences must be intrinsic, and offers at least one example of an essence that is relational. And despite being largely silent on the matter, Kripke says nothing that should lead one to think that essences must be intrinsic; if anything, his associated claims about the necessity of identity have the opposite flavour. I conclude that Kripke and Putnam’s traditional essentialism has been misinterpreted, and consequently that neo-essentialism is not neo at all.

9:40–10:20 am
James Overton, “Domain Ontologies and Philosophy of Science”

Domain ontologies are a fusion of modern information technologies with philosophy of science traditions dating back to Aristotle. In this paper I argue that philosophers of science need to examine the use of these tools in the sciences. Computer scientists use the word “ontology” to refer to taxonomies of terms that are enriched with a network of well-defined relations. Their main interest in such ontologies is to promote data exchange, search, and analysis, often under the rubric of the “semantic web.” Domain ontologies are the application of this approach to the sciences, where the terms of the ontology are supposed to name the natural kinds in a scientific domain. Scientists can annotate their data using terms from the domain ontology, and take advantage of semantic web tools for exchange, search, and analysis. Biomedical sciences have been among the first to make wide use of domain ontologies, with ontologies being developed for genetics, cell biology, anatomy, human disease, and many more domains.

Many philosophical debates over the special sciences hinge on the status of natural kinds. With domain ontologies scientists are trying to catalogue these kinds, to circumscribe scientific domains, and to describe links between kinds in different domains. It is important to square this with philosophical work: do natural kinds exist? If so, how are they distinguished and related? Is nominalism viable? Domain ontologies offer a wealth of fresh examples and an opportunity for philosophers to apply their skills directly to the practice of science.

10:20–11:00 am
Serife Tekin, "Understanding Mental Illness: Moving Beyond Natural Kinds"

Ian Hacking (1994, & 1995) argues that some mental illnesses are human kinds; a classification that includes a variety of people, their behaviour, their condition and modes of action. What distinguishes human kinds from natural kinds is that in human kinds, classification generates a looping effect, *i.e.* it results in a self-awareness in the subjects so classified, which in turn, has an impact on the classification rubric itself. Rachel Cooper (2004) opposes this and asserts that the feedback effect in human kinds is comparable to the one found in natural kinds; suspecting that some types of mental disorder are natural kinds. Thus, psychiatrists should pursue empirical research programs to understand these "natural" mental illnesses. However, remaining skeptical, Cooper acknowledges that even if types of mental disorder are natural kinds, there are epistemological and practical reasons to doubt that the *Diagnostic Statistical Manual* (DSM), the widely accepted criteria for mental illness diagnosis, will ever reflect their natural structure. Hacking (2007), abandons the notions of natural and human kind and proposes a "framework for analysis" for the kinds of people studied by human sciences, in which, the looping effect occurs between five axes. There are not only the names of the classifications, and the people classified, but also the experts who classify, study and help them, the institutions within which the experts and their subjects interact, and there is an evolving body of knowledge about the people in question.

This paper analyzes Cooper's account of mental illness, in light of Hacking's more recent arguments. I agree with Cooper that the DSM schema cannot fully account for mental illness, but I disagree with the assertion that some mental illnesses are natural kinds. Instead of classifying mental illness as a natural kind, I suggest we use Hacking's new framework to illuminate the phenomenon. By itself, focusing on the person's symptoms to pick out the "nature" of her illness with the help of empirical research is not sufficient. The phenomenon of mental illness is multiplex and cannot sufficiently be captured in purely scientific terms.

**Session III.2.c**
**11:05 am - 1:00 pm**
**417 Southam**
***Philosophy of Quantum Mechanics***

11:15–11:50 am
Michael Cuffaro, "The Electron as Noumenon"

I argue that it is possible to gain a better understanding of Niels Bohr's philosophy of quantum mechanics by analyzing the relation between his philosophy and the theoretical philosophy of Immanuel Kant: that certain aspects of Bohr's interpretation stem from a roughly Kantian epistemology that distinguishes appearances (results of experiments, for Bohr) from things-in-themselves.

The idea that some of the founders of the Copenhagen interpretation of quantum mechanics were influenced in their thinking by aspects of Kant's theoretical philosophy is not new. Heisenberg is explicit about the link between his interpretation of quantum mechanics and Kant's philosophy. The case for Bohr himself, however, is more controversial. Folse, for instance, flatly denies that there is any link, while Honner, for example, argues that both thinkers have in common their use of transcendental arguments. I believe the link is stronger than this. I think it can be shown that there are strong similarities between Bohr's method of arguing for his principle of complementarity and Kant's method of resolving the antinomial conflict between, *e.g.* freedom and determinism; I think it can be shown that Bohr conceived of the objects of study in quantum mechanics analogously to the way Kant conceived of noumena as "problematic concepts;" and I think it can be shown that Bohr's renunciation of the principle of causality for quantum mechanics follows from such a conception (a view which, I will argue, Kant would have held as well).

11:50–12:25 pm
Mark Shumelda, "The Hole Argument in Quantum Gravity"

Einstein's hole argument – as reintroduced by Stachel, Earman and Norton in the late 1980s – has had a profound impact on our understanding of space and time. This is especially true in the context of classical general relativity, where the hole argument arises naturally from the gauge freedom of the theory. The hole argument urges us to interpret points on the spacetime manifold which are mathematically gauge-related as representing a single, indistinguishable physical state of affairs (*i.e.* Leibniz equivalence). The manifold of spacetime points itself has no physical significance. This usually leads one to adopt a relationalist or "sophisticated" substantivalist view of space and time.

In my paper I will discuss the extent to which the hole argument carries over from classical general relativity to the burgeoning field of quantum gravity. Dean Rickles and Oliver Pooley have recently engaged in a debate over whether an analogue of the hole argument can be in fact be constructed in the loop quantum gravity programme (2005). In my paper I will evaluate this debate and extend the question of the applicability of the hole argument to other research programmes in quantum gravity, including string theory and Julian Barbour's Machian geometrodynamics. The question of whether or not each quantum gravity programme admits of a hole argument should serve as a useful tool for assessing that programme's ontological commitments.

12:25–1:00 pm
Laurent Jodoin, "Causality, quantum mechanics and entropy increase"

David T. Pegg (2006 & 2008) and John G. Cramer (1980) present two causality principles derived from special relativity. Though presenting a non explicit formulation, it suggests a naturalist approach. Pegg shows that a retrocausal account of quantum mechanics is preferable. However, this account is not ill-founded but rather illjustified and leads to undesirable consequences – still, it describes commonly (macroscopic) causality as manipulability and considers quantum collapse theories. Yet, the causal asymmetry can be reduced to entropy increase (Eckhardt 2006). Eckhardt argues that fine-grained influence of future upon past is continual and ubiquitous but that reverse influence lacks the focused relationship to conditions that would qualify it as reverse causality. But a fine-grained fact is indescribable. Fine-grained influence is t-symmetric in accordance with fine-grained t-symmetry; earlier and later states constrain one another equally although not necessarily totally. There can be reverse influence, but not reverse causality. In such case, fine-grained set is assimilated to quantum state where physical influence is bidirectional. It is argued that a (pseudo-)complete description – such as the state vector – is incompatible with a causal manipulability in an entropy increasing context. And a (brief) sketch of an explication of the causal differences between microscopic and macroscopic systems is presented.