A Fault in Our Star: Science, Sunspots, and the Economics of W. S. Jevons

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British economist and philosopher of science William Stanley Jevons published a series of papers in the 19th century alleging a connection between sunspots and commercial crises in England. Jevons' work on sunspots has often been criticized by his contemporaries working in statistics and economic--and even historians of economics--for being wrong, silly, or ridiculous; a peculiar blemish on an otherwise exceptional career. In the words of Ekelund and Hébert, his sunspot theory was "the most fanciful and, ultimately, ridiculed idea of his life." Jevons, a trained chemist and the foremost economist and logician in Britain, appears to have made the most basic of statistical errors: confusing correlation and causation--an error that he would never recant.

However, an examination of the literature published on the causes of sunspots, and their effects on the weather in the 19th century makes it clear that Jevons's theory was not unprecedented, and had strong theoretical justification. Jevons's work on sunspots was a work of synthesis--an attempt to both make economics scientific, and unite scientific knowledge with economic theory.

The Varieties of Scientific Expertise

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What does it take to achieve scientific expertise? Is it an entirely epistemic achievement turning on one's ratio of true and/or justified beliefs to false and/or unjustified beliefs? Alternatively, is it entirely a social status, so that knowledgeable people overlooked or socially excluded thereby lack expertise? Is expertise a matter of specialization, as Feyerabend feared, or perhaps successful acquisition of the tacit knowledge characteristic of a scientific domain? Beyond the notion that expertise is in some way epistemologically significant, there is extraordinarily little consensus across disciplines studying scientific expertise on what makes an expert and whether this achievement should be regarded as a positive, negative, or neutral thing. Here I survey divergent analyses of scientific expertise in three fields – social epistemology, philosophy of science, and sociology of scientific knowledge – attempting to disentangle points of genuine agreement and disagreement on the social and evidential significance of expertise in scientific practice. In so doing, I emphasize the risk of mere apparent agreement (masking underlying disagreement) on scientific expertise between philosophers and sociologists of science, particularly given the recent "realist" turn in rethinking expertise advocated by Collins and Evans.

Writing Global Knowledge: Oceans and Texts

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"I must sit down quietly in London – to work at the materials collected in this voyage[.] I am obliged to turn writer (I will not say Author) as a point of duty – on subjects connected with Hydrography--and have a number of Spanish works to translate--which together with charts and other matters will keep me occupied during two or three years." Robert Fitzroy to his sister, Frances, on board HMS Beagle at St. Helena, 11 June 1836.

The Narrative of the Surveying Voyages of HMS Adventure and Beagle (1839) is a complicated work of multiple authorship and audiences, combining navigational detail, records of meteorology and magnetism, lists of natural history specimens with history, politics and ethnography. Its bulk is usually encountered now through Darwin's disparaging eyes ("no pudding for little school boys was ever so heavy"). But its unwieldy literary form qualities makes this work -- and the genre of voyage narratives to which it belongs -- especially revealing as a record of projects of global knowledge. Here I use the Narrative and two parallel texts: a manuscript spoof of the Narrative (c. 1844) and Charles Babbage's Ninth Bridgewater Treatise: A Fragment (1837) in order to examine the idea of global science as it emerged in concert with the re-orientation of trade and politics in the opening of South America. Spurred by the heterogeneity and unevenness of the Narrative, we can see how nineteenth-century global model emphasized completeness, but also instability. The global point of view was robust, exact and comprehensive – the kind of picture supplied by Admiralty charts -- but it was also uncertain and impermanent. The uncertainties of the global point of view were understood as both social and natural – that is, uncertainties derived from a combination of incomplete information, the cultural or historical or linguistic distance separating the observer from the observation, political and economic changes, as well as the permanent fluctuation of natural forces.

Drug Addiction, Self-Experimentation and Insider Knowledge: A New Epistemic Role for the User?

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As Nancy D. Campbell demonstrates in Discovering Addiction, human experimentation played an important role at the Addiction Research Centre (ARC) in Lexington, Kentucky in the 1950s and '60s. At the ARC, 'postaddicts' were re-addicted to opiates in order to test the 'addiction liabilities' of different analgesics in the hope of finding an alternative to morphine. These researchers also experimented on themselves as a crude 'control group' and thus can be understood as part of the history of self-experimentation in medicine. Importantly, these researchers already possessed epistemic credibility stemming from their cultural authority as scientists. This authority was, and remains, unavailable to the addicted subject. The laboratory logic at the ARC recognized that postaddicts possessed tacit, experiential knowledge about drug use that was unavailable to the drug-naïve researchers themselves, but they still insisted on controlling the experiments for scientific rigor. Drawing on feminist standpoint theory, this paper will explore the potential for addicted persons to be appreciated as reliable sources of 'insider knowledge' from auto-experimental evidence. How would results differ if drug users were able to participate in the research designs of addiction recovery studies? Can the writings of experienced addicts be meaningfully understood as self-experimentation? For example, well-known beatnik William S Burroughs systematically documented his drug use in both his fiction and a 1957 British Journal of Addiction (currently published as Addiction) article that clearly demonstrates the epistemic value of self-experimentation. While these studies would not reveal the neuro-physiological aspects of addiction, they could potentially have important implications for drug policy formation by treating user experiences as credible evidence. Finally, including addicted users in the scientific discussion may help to de-stigmatize this population by allowing meaningful participation and thereby minimizing their subjugation as research material.

Wounds, Readers, and Statecraft: Military Medicine in 17th-Century China

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During the last two decades of the Ming dynasty before its fall to Manchu armies in 1644, Chinese literati sought ways to save the dynasty from its increasingly difficult military position. For many literati, political strife during the 1620's prevented them from demonstrating their commitment to the state by holding political office. Some took their frustrations to the pages of books, seeking to transmit life-saving knowledge through writing. In 1628, one such literatus, Mao Yuanyi (1594-ca.1641), submitted his Treatise on Military Preparedness to the newly enthroned Chongzhen Emperor (r. 1628–1644). This voluminous military encyclopedia contains an extensive section on military medicine. My paper uses this section as a case study to examine the textual reproduction of technical knowledge in the late Ming. In this section of Mao's book, Mao's authorial practices construct multiple, coexisting ideas about soldiers' bodies and disease. I argue that Mao translates these ideas into one coherent hierarchy of dangers to soldiers' bodies. For each disease, he combines multiple authors' ideas about triage, treatment, and prognosis to put the most useful range of possibilities in the hands of his readers, who can then select between multiple understandings of diseases and bodies, and thus between multiple possible healing practices. By manipulating this information and laying it at the disposal of his peers, Mao's technical treatise becomes a poignant expression of his normative conception of the state--a state whose bureaucrats would have access to flexible, active, and practical knowledge when they needed it most.

The Ontario Woman's Christian Temperance Union and eugenics: An uneasy alliance

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The Ontario Woman's Christian Temperance Union (WCTU) began its long crusade against the dangers of alcohol in 1877. Believing intemperance to be the underlying cause of many pressing problems of the day -- poverty, illness, crime -- they sought to eradicate these social evils at its root. By 1900, the Ontario WCTU had a membership of 5,500, and the Dominion organization had over 10,000; the WCTU thus represented an important women's reform organization at the turn of the century. The method of the WCTU is revealed by anecdotes from its national publications, The Woman's Journal, and White Ribbon Tidings; intemperance was to be eradicated by the moral reform of the individual. This emphasis on the ultimate possibility of individual redemption thus put the WCTU at odds with a contemporary eugenics movement that increasingly advocated after 1900 the segregation and sterilization of individuals thought to be responsible for a moral, physical, and intellectual decline of the nation's population. This paper examines the Ontario WCTU's relationship with the Canadian eugenics movement between
1880-1920, drawing especially on their official publications as well as material from the meetings of the Ontario Provincial Union.

"The Waters I am Entering No One yet Has Crossed": Alexander Friedman and the Origins of Modern Cosmology

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Ninety years ago, in 1922, Alexander Friedman (1888-1925) demonstrated for the first time that the General Relativity equations admit non-static solutions and thus the Universe may expand, contract, collapse, and even be born. The fundamental equations he derived still provide the basis for the current cosmological theories of the Big Bang and the Accelerating Universe. Later, in 1924, he was the first to realize that General Relativity allows the Universe to be infinite. Friedman's ideas initially met strong resistance from Einstein, yet from 1931 he became their staunchest supporter. This essay connects Friedman's cosmological ideas with the 1998-2004 results of the astronomical observations that led to the 2011 Nobel Prize in Physics. It also describes Friedman's little known topological and astronomical ideas of how to check General Relativity in practice and compares his contributions to those of Georges Lemaitre. Recently discovered corpus of Friedman's writings in the Ehrenfest Archives at Leiden University sheds some new light on the circumstances surrounding his 1922 work and his relations with Paul Ehrenfest.

On the Mathematical Constitution of Physical Facts

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Modern physics is highly mathematical, and this may suggest that mathematics is bound to play some role in explaining the physical reality. Yet, there is an ongoing controversy about the prospects of mathematical explanations of physical facts and the nature of such explanations. A popular view has it that mathematics provides a rich and indispensable language for describing the physical reality but could not play any role in explaining physical facts. Even more prevalent is the view that physical facts are to be sharply distinguished from mathematical facts. Indeed, both sides of the debate seem to hold this view. Accordingly, the idea that mathematical facts could explain physical facts seems particularly puzzling: how could facts about abstract, non-physical entities possibly explain physical facts? In this paper, I challenge these common views. I argue that (1) in addition to its descriptive role, mathematics plays a constitutive role in modern physics: some fundamental features of the physical reality, as reflected by modern physics, are essentially mathematical; and that (2) this constitutive role is the source of mathematical explanations of physical facts. On the basis of this argument, I suggest a new account of mathematical explanation of physical facts. I conclude by comparing this account to other existing accounts of mathematical explanations of physical facts.

Ancient Atomism and the Mechanical Philosophers

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In twentieth century scholarship, the earliest atomists are commonly said to have a 'mechanical' natural philosophy, or to describe nature 'mechanistically.' Although this is recognized to be somewhat anachronistic, reasons for applying the term to the ancient atomists include the sympathy seventeenth century scholars felt towards ancient atomism, the seventeenth century practice of labelling the ancient Greek atomists 'mechanistic,' and also perceived similarities between ancient and modern corpuscularian theories.* I argue that these apparent justifications for describing ancient atomists as 'mechanistic' are weaker than might appear.

Those who drew on ancient atomism for inspiration in the seventeenth century altered it considerably from the ancient version. And while Henry More and the Cambridge Platonists did describe ancient atomism as 'mechanical', their reasons for doing so depend on an implausible and idiosyncratic reading of the history of philosophy. By contrast, Boyle, the major proponent of 'the mechanical philosophy,' is equivocal about admitting ancient atomists into the fold. Third, the reasons for assigning some properties to matter and others not are quite different in ancient and modern corpuscularian theories.

There are other dangers inherent in applying modern terminology to the ancients, not least the fact that the notion of 'the mechanical' is a historical artifact that is not as perspicuous as is commonly assumed. Further, labelling often imports hidden assumptions. An additional danger is that calling ancient atomism 'mechanical' could lead scholars to overlook the real impact of ancient mechanics on natural philosophy in antiquity.

* I use 'corpuscularian' as the broader category here: Boyle, for instance, was agnostic on whether the corpuscles he posited were indivisible.

What's in a (Plant) Name? The Maori-Latin Index and Scientific Botany in New Zealand

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By the end of the nineteenth century, science had become ensconced as an arbiter of cultural authority in the extra-European colonial world. In reciprocal measure, Europeans consigned indigenous knowledge to the realm of "primitive superstition" destined for cultural extinction. My paper examines this phenomenon with reference to botany in metropolitan Britain and New Zealand over the course of the long nineteenth century. Throughout this period, indigenous plant names made it possible for Europeans to draw upon Maori knowledge of the less Europeanized parts of the colony to collect and classify flora. In the mid-1860s, however, the Kew-based German botanist Berthold Seemann suggested another use for Maori plant names – that, when considered in conjunction with both their Latin and Polynesian equivalents, they could shed light on Maori historical migrations across the Pacific, and thereby assist in ranking Maori as a race by descent from an ostensible Indo-Aryan point of geographical and cultural origin. I treat this relationship between marking plants and measuring peoples as a metrological one – in its simultaneous promotion of both a metropolitan-centered imperial collecting and classifying enterprise, and an anthropological frame of reference according to which metropolitan botany held sway over indigenous, putatively "subjective," ways of knowing. My analysis also attends, however, to the fundamental weakness in this metrological framework: the Maori knowledge and peoples entailed in New Zealand botany but obscured by such ethnological abstractions, and the variously ambivalent, dissonant and performative nature of these concealments.

Towards An Inclusive Scientific Epistemology

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Abstract: While scientific epistemologists agree that epistemological questions ought to receive scientific answers, they disagree about which scientific methods are appropriate for this task. The Logical Positivists, led by Carnap, maintain that epistemological questions must be answered by purely logical means. Quine, on the other hand, insists that epistemology ought to be replaced by psychology, while more recent naturalists think that the empirical sciences more generally ought to be doing the job. I will argue that all of these positions are misguided, not because they take scientific approaches to epistemology, but because they fail to recognize the variety of scientific methods that are applicable to epistemological questions. Though there is now a wealth of empirical results that are relevant to epistemology, the forms of a priori linguistic analysis to come out of mathematics and physics at the turn of the twentieth century should not be neglected. More specifically, it will be argued that Frege's logical analysis of arithmetic, Hilbert's axiomatic analysis of geometry, and Poincaré's conventionalist analysis of physics constitute three indispensable scientific methods of arriving at epistemological conclusions.

Why it is important to distinguish between different types of scientific expertise

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The topic of expertise has played a large role in contemporary discussions related to the epistemic authority of scientists in broader social contexts. Most of these analyses have stemmed from Science and Technology Studies literature with a strong sociological bent. The consensus that emerged from that tradition is that expertise is constructed on social recognition contingent on various social, economic and political arrangements, not on any privileged epistemic access to independent facts about the world. Philosophy of science has said little about the topic of expertise, with some very notable exceptions (including Hardwig, Goldman, Kitcher, Longino and others). Most of these treatments of expertise focus on debates about the realism (ontological and epistemic) tied to some experts claims, or whether we can still speak of privileged epistemic access of scientific experts if their scientific claims are as value-laden as any other knower's claim. After a very brief survey of some of these issues, I wish to make a related but separate philosophy of science point: different scientific disciplines are value-laden differently; some offer explicitly instrumental and non-realist claims (e.g. some modelling in economics), while others are explicitly tied to ontological realist commitments (e.g. some theories in fundamental physics). These differing regulatory ideals should inform our treatment of scientific expertise. Although many have already recognized the pluralism in scientific ontological and epistemic commitments, I wish to show how this pluralism should inform our views about epistemic expertise. Scientific expertise is not right or wrong, it is large, it contains multitudes. We will see that it is that multiplicity itself that warrants (in certain cases) the genuine epistemic authority of scientific experts.

Pragmatism and the scope of the problem of induction

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Hume's account of inductive involves a special kind of inference beginning with premises reporting observed matters of fact. The justification of such observational claims is generally regarded as more in doubt than are conclusions based on logical or mathematical reasoning. Further, the justification of inductive reasoning itself is also widely regarded as more in doubt than that of the observational premises of inductive reasoning.

Pragmatists have emphasized the central role of reliable agreement as a criterion for evaluating the justification of cognitive practices. See, for example, Pierce, "The Fixation of Belief", and "Empiricism and the Philosophy of Mind", where Sellars presents a pragmatic account of how individuals come to justfiably regard themselves as reliable observer. On Sellar's account, inductive inference is essential to an observer's being justified in taking her own observations to be reliable.

This paper focuses on a key consequence of the pragmatists' focus on the question of whether a cognitive practice reliably leads to stable agreement: from the pragmatist's point of view, inductive reasoning lies at the root of the justification of all claims, including logical, mathematical and observation-based claims, not just claims about unobserved material facts. This pragmatic view of the role of induction in justification reinforces Strawson's "analytical solution" to the problem of induction (cf. An Introduction to Logical Theory), viz. that the meaning of 'justified' includes justification by induction, since, on the pragmatist view taken here, all epistemic justification relies on induction.

Reconsidérer la pratique de théorisation du point de vue de l'« activity-based analysis » / Reconsidering theorizing practices from the "activity-based analysis" view point

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[For English, see below]
Pour étudier l'activité théorique, il est d'usage dans une approche de la science-en-pratique de s'appuyer sur certains ingrédients conventionnels : « croyances, compétences, instruments, identités, valeurs partagées, méthodes, aspects culturels, contextes locaux, etc. » [Galison 1998, Hacking 1992, Pickering 1998]. En se reposant seulement sur une collection restreinte de

ces éléments, spécifique à chaque étude, les approches classiques de la pratique scientifique échouent à fournir un cadre d'analyse transposable d'un cas d'étude à un autre et, solidairement, à ramasser l'ensemble des éléments dispersés de la pratique dans un système cohérent.

L'objectif de cette présentation est de discuter une perspective récente, l'« activity-based analysis », qui, reconnaissant l'impalpabilité problématique de la notion classique de pratique scientifique, propose de la reconcevoir comme une activité composée d'un ensemble d'actions sous-jacentes. Avec cette série d'éléments désormais comparables en main, les analystes de cette tendance avancent qu'il est maintenant possible de concevoir un cadre général de l'activité scientifique et d'en discerner une logique d'agencement ; une « grammaire » [Chang 2011, Schatzki 1996 et à certains égards Giere 2006].

Adhérant à cette perspective, je montrerai comment l'activité de théorisation conçue dans le cadre de l'« activity-based analysis » permet (1) de dépasser les conceptions formelles des théories (aussi bien syntaxiques [Carnap 1966], que sémantiques ou « modélistes » [Van Fraassen 1980]) par l'élargissement au processus de théorisation et donc par la prise en compte de nouveaux facteur constitutifs (intentions, institutions (wittgensteiniennes), outils, inscriptions, supports) ; et (2) de pouvoir fournir une nouvelle compréhension des aspects formels des théories conçus non plus comme des représentations structurelles, mais plutôt comme des modes d'accès au réel à travers des activités spécifiques.

In order to study the theorizing activity in a science-in-practice approach, it is customary to use some conventional ingredients such as "beliefs, skills, instruments, identities, shared values, methods, cultural aspects, local contexts, etc." [Galison 1998, Hacking 1992, Pickering 1998]. By relying only on a small collection of these elements, specific to each study, conventional approaches of scientific practice fail to provide an analytical framework that could be transposed from one case study to another, and, consequently, to gather all the scattered elements of practice together in a coherent system.

The aim of this presentation is to discuss a recent perspective, the "activity-based analysis", which, because it acknowledges the problematic intangibility of the classical notion of scientific practice, aims at conceiving it as an activity consisting of a set of underlying actions. With this series of comparable elements in hand, proponents of this trend claim that it is then possible to conceive a general framework of scientific activity and to disclose its logical layout, its "grammar" [Chang 2011, Schatzki 1996 and to some extent Giere 2006].

Along with this perspective, I intend to show how the theorizing activity developed in the "activity-based analysis" framework allows (1) to move beyond the formal conceptions of theories (both syntactic [Carnap 1966] and semantic conceptions [Van Fraassen 1980]) by extending them to theorizing process i.e. by taking into account new constitutive factors (intentions, institutions (in a wittgensteinian sense), tools, inscriptions, materials) and (2) to provide a new understanding of the formal aspect of theories conceived as now as means of access to reality through specific activities, rather as a structural representation of reality.

The Role of Cases in Arguing for (Anti-)Realism

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Case studies of past and present science are often invoked as evidence for and against the viability of scientific realism. Though such evidence is often presented as highly consequential for these debates, it is nonetheless an open question how probative it can be. I consider this question in the light of three independent but mutually reinforcing arguments. The first concerns the likely robustness of disputes about realism and antirealism under historical reflections regarding the methodologies and practices of scientists. A second asserts the immunity of realist and antirealist stances to historical inductions based on considerations of the fates of past scientific theories. A third targets the apparent inability of case studies to adjudicate between different (and mutually incompatible) versions of scientific realism currently prevalent in the literature, by means of reflections on the ontologies of theories as discussed in what is now commonly referred to as the "metaphysics of science".

"The Irrelevance of History of Science to Philosophy of Science": Fifty years later

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Must philosophy of science be informed by the history of science? I claim that philosophy of science does depend in a significant sense on the history of science. In particular, I argue that N.R. Hanson was correct when he claimed, "philosophy of science without history of science is empty," but that his argument for the necessity of history of science has been successfully criticized in the literature. I consider, for example, three prominent objections to historicist philosophy of science due to Ron Giere. Citing David Malament's work concerning the relative simultaneity relation in relativity theory, I show that Giere's objections undermine Hanson's positive argument. By drawing on three distinctions made by Kuhn concerning different types of philosophy of science, I propose a modified framework for doing historicist philosophy of science that incorporates Giere's objections. I argue that work in general philosophy of science (i.e. that area that addresses generally the nature of scientific explanation, confirmation, theory change, etc.) that is not informed by the history of science is indeed empty. But I also argue that all philosophers of science should attend to the history of science. I conclude by claiming that philosophers of science need to be trained in history of science and that there is a significant conceptual rationale underlying the practice of history and philosophy of science.

Information Preserves Structure Across Scientific Revolutions

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Thomas Kuhn and Paul Feyerbend introduced the issue of semantic incommensurability across major theoretic changes that we call scientific revolutions. Feyerabend recognized that the problem of semantic comparability arose because of problems in empiricism itself. I argue that the problem arises from two widely held assumptions. The first is Peirce's criterion of meaning according to which any difference in meaning must make a difference to possible experience. This is a sort of positivism, but it is not verificationist. The second assumption is the verificationist view that the meaning of any statement is given by the conditions under which it can be taken to be verified. Together these assumptions entail the infamous Quine-Duhem Thesis that any two theories have extensions that are equally compatible with the evidence. This leads less directly to Kuhn's Incommensurability Thesis, that two theories can be both incompatible and semantically incommensurate, notoriously across major "scientific revolutions", undermining the idea of cumulative progress in science. One of the more promising attempts at resolution is the Structuralist Approach to Theories, in which theories are model theoretic structures isomorphic to parts of the world. This approach was shown fairly early to permit incommensurability. Further restrictions are required. I will argue that a resolution using the theory of Information Flow developed by Jon Barwise and Jerry Seligman can provide the extra restrictions, allowing even incommensurate theories to share evidence. A consequence of this perspective is that the meaning issue is a red herring. Another is the rejection of verificationism.

The 'indisputable authority' of the Greenwich Observatory: experiments with clock coordination in Victorian Britain

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In 1852, George Biddell Airy, then Astronomer Royal at the Greenwich Observatory, established a system of telegraphically distributing Greenwich Mean Time (GMT) throughout Britain. This system, which fused meridian astronomy, metrology, and telegraphy, was intimately connected with cultural values concerning punctuality and time-thrift. While the system was intended to improve clock coordination there were, of course, errors. When the signal performed as intended it guarded the safety of railway passengers and navigators, and created a standard against which punctuality could be judged. That the signal should be trustworthy was not, however, self-evident. This paper examines Airy's efforts to manage instances of signal error and malfunction in the Greenwich time service in the mid-nineteenth century. In the context of time distribution accuracy became a matter of public concern. In addition to public safety, what was at stake in these early trials of clock coordination was the authority of Greenwich measures and the reliability of electrical science.

Situating Natural Capital in the History of Economic Thought

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In economics, the relatively new concept of "natural capital" denotes natural phenomena such as the regulation of the atmosphere's chemical composition, basic climatic stability, photosynthesis, and pollination. Such articles of natural capital provide humans with welfare-enhancing goods and services in a manner that is relatively detached from human agency. In this paper, I investigate how natural capital relates to other central theoretical concepts in the history of economics. I argue that natural capital is a hybrid concept – it shares characteristics with two key concepts in classical political economy: land and capital. Since natural capital is an original factor of production (it does not need to be produced by humans) it bears a striking resemblance to land, the third factor of production of classical political economy. Unlike land, however, natural capital depreciates. This characteristic is more obviously shared with capital goods, such as man-made machines. Unlike ordinary capital goods, however, instances of natural capital have no intelligent designer. Moreover, while economists might attribute natural capital with a final cause, natural capital is not, like machines, generated for human purposes.

Déjà vu all over again: Newton and the Newton papers

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In 1727 when Newton died, he left a massive collection of manuscripts behind him, on subjects ranging from theology, church history and alchemy to mathematics and natural philosophy. Most had never been read by anyone else. Voluminous, varied, and, in the case of his theological works, often passionately argued, these writings cover a much broader and more heterodox set of topics than Newton broached publicly during his lifetime. In the nearly three hundred years since his death, Newton's papers remained largely, but not entirely, out of sight.

From the 1830s onwards, dedicated Newton-seekers such as Samuel Horsley, Jean-Baptiste Biot, David Brewster, John Couch Adams, George Gabriel Stokes, and John Maynard Keynes managed to acquire intermittent access to the papers. The brief glimpses they gained served to perpetuate a paradoxical myth of Newton that remains current today: that of the rational natural philosopher who devoted the majority of his vast energies to subjects outside of science.

In this paper, I examine how this 'paradoxical' Newton--blending science, religion and alchemy--has been produced and re-produced in successive iterations. How has the tangled history of his private papers helped to sustain a continually surprising Newton, whose dark secrets and obsessive manias titillate us as they subvert our expectations? Why do we seem never to learn that his range of interests was much wider than that consigned to him by succeeding generations? Why, with Newton, is it always déjà vu all over again?

Moderate Locationism and Natural-Kind Essentialism

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Sam Cowling (forthcoming), developing an idea suggested by Bas van Fraassen (1967) and Robert Stalnaker (1979), argues that the relationship between objects and their properties is not that of instantiation, but rather occupation. He presents the notion of an abstract quality-space, with locations on that quality-space constituting complete qualitative profiles. Objects in the actual world can be seen as occupying locations in quality-space rather than as instantiating a property or set of properties. Cowling dubs this theory locationism, and argues in favour of it by reference to its parsimony (the instantiation relation can be dumped from ontology and the occupation relation retained, which relation we need anyway to explicate spacetime) and certain other theoretical advantages. Here I argue that locationism faces certain difficulties which suffice to motivate the development of an alternative version of the theory, moderate locationism. I then argue that moderate locationism carries a hefty theoretical advantage of its own: it provides for novel solutions to two important problems facing natural-kind essentialism, problems recognized by such essentialists as Brian Ellis (2001; 2002) and E.J. Lowe (2006), but which so far have not been adequately addressed.

"Atoms for Peace?" The Role of Science in Global Governance at mid-Twentieth Century

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The political scientist James Rosenau has argued that the scientific community is the prototype for a new "sovereignty-free" community, agents of a nascent global culture which should produce in time a new and improved model of governance. Instead of "muddling through" political problems, scientists will take the lead and "model through" global solutions.

This paper explores the history of international scientific collaboration, beginning with the first scientific networks at the turn of the twentieth century. It argues that global scientific cooperation has been characterized by a pattern of oscillation between the assertion of a cosmopolitan science culture by the scientific community and the deployment of science by governments as an instrument of interstate competition. The paper argues that 1957-58 was the critical moment when these issues were most dramatically in play, marked by the restructuring of UNESCO, the rollout of interstate collaborative activities under the auspices of the International Geophysical Year, and the publication of the Vienna Declaration on the role of scientists in public policy.

Among the agencies which will be addressed will be the International Council for Science (ICSU), the International Union for the Conservation of Nature, (IUCN) UNESCO and its sister specialized agencies of the UN system and the Pugwash Movement, which won the Nobel Peace Prize for its mobilization of the scientific community to fight nuclear proliferation.

Towards the Institutionalization of Applied Entomology

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The institutionalization of applied entomology in the Russian Empire officially began in 1894 with the creation of the Bureau of Entomology as part of the Scientific Committee in the Ministry of Agriculture and State Domain. However, long before that, as early as the 1840s, the Agricultural Department had been collecting information on pest outbreaks and on the methods by which to control them. The Ministry had also hired several experts to make inspections, answer queries of the landowners and provincial authorities, as well as to write both specialized and popular manuals. The Russian Entomological and Free Economic Societies, along with some Zemstvos, were involved in this work. By the 1870-80s several projects to create experimental stations in applied entomology were proposed, but their research programs were still quite crude.

However, in the first half of the 1890s quantitative growth shifted to qualitative. First, the landowners and administrators learned the language of scientific descriptions for pest insects, a process during which their requests to entomologists became much more clear. Secondly, entomologists became familiar with the methods of plant cultivation, harvesting, storage of the yield, etc., so that their recommendations became more useful for farmers. My paper will discuss this preparatory phase in which little was accomplished towards developing effective methods of pest control, but which was still an important process of forming a common language and the formulation of specific research programs. While it would take several decades for these realities to take root in the Russian Empire, it signaled the creation of applied entomology as a professional discipline that included the specific study of life cycles and the distribution of insects – i.e. ecology. This development was one of continuous dialogue; at one end were farmers, at another, biologists. Between these two groups agronomists as well as local and central administrators functioned as mediators, but their role was no less important than the "main actors" in this process of institutionalization.

The Theological Dimensions of Newton's Thought Experiments

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This paper discusses the link between theology and thought experiments in Newton. This discussion is motivated by the fact that the method of thought experiments in late medieval thought had a genuinely theological justification. For example, despite Aristotle's strong influence on Medieval thought, including his categorical rejection of possible worlds, scholastics frequently entertained even counterfactuals to conduct thought experiments. The theological justification for the entertainment of counterfactuals was drawn from God's omnipotence. Under the assumption that the possible worlds entertained in thought experiments on matters physical and logical are not real possibilities, thought experiments with counterfactuals to derive knowledge about the actual world seemed legitimate in light of God's power to bring those possible worlds into existence, at any time God wishes. In light of this, the questions arises: what is the situation in Newton who made use of thought experiments in matters related to his physical theory and wrote extensively about theology?

In the presentation of my paper I will first say a little bit about the scientific practice of thought experiments in order to identify its theological dimensions in general terms. In a second step I will review a recent proposal to distinguish between a divine and a mundane metaphysics in Newton. Newton insists on the logical priority of physical theory over mundane metaphysics. At the same time his divine metaphysics frames the relationship between mundane physics and physical theory. In a third step I will argue that this way of looking at Newton's metaphysics has important implications for our understanding of Newton's use of thought experiments.

Thin Ice: Trust, Pluralism and Polar Bear Conservation

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Issues of trust between scientific and lay communities are of increasing interest in philosophy of science. Often, as Naomi Scheman and Heidi Grasswick have argued, the distrust lay communities have of scientists comes from suspected bias, or a past history of exploitation. Strategies for repairing trust involving knowledge-sharing, participatory research and consensus building. I will examine trust issues between Inuit communities and scientists regarding polar bear conservation. In addition to social, economic and political issues complicating trust, there are also marked epistemic and ontological differences between these two groups. Some Inuit communities, as geographers Jeremy J. Schmidt and Martha Dowsley illustrate, take the position that scientific research--because it treats polar bears as objects of study, not subjects in their own right--cannot produce accurate knowledge with regard to polar bear populations. Thus, Inuit communities may reject scientific knowledge-claims not only because of a history of exploitation, but also because they do not accept the ontological and epistemic premises underpinning the research. If we are metaphysical pluralists, as Helen Longino suggests we should be, it becomes hard to see how to build trust when neither ontology nor epistemology is shared. The case of polar bear conservation illuminates this problem, but also suggests a solution. Instead of trusting the knowledge-claims, one can trust the knower. While some Inuit may disbelieve scientific knowledge-claims and vice versa, if both can trust the integrity of the other--if both accept the knowledge-claims of the other as, in her perspective, genuine--then a compromise can be reached.

Chimpanzee knowledge and some implications for analytic naturalized social epistemology

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In this paper I will first briefly examine why certain studies of chimpanzee behavior should persuade us that these animals are usefully regarded as epistemic subjects who engage in recognizable epistemic activities (e.g., evidence gathering). This, I will then argue, ought to have implications for analytic, particularly naturalized social epistemology, whether highlighting assumptions about knowledge production, epistemic activity or the nature of epistemic subjects. To illustrate, I will seek out a possible role for an individualist as well as social epistemology in understanding the knowledge of chimpanzees, perhaps providing an application of individualism that escapes recent attacks from those who see social epistemology as a more accurate and normatively tractable framework for theorizing about human knowledge or epistemic activities. A further virtue of this approach is that it places analytic epistemology in contact with the animal cognitive sciences. Many in these sciences seek to ascribe both knowledge and active cognitive engagement to their subjects. There appears to be a need, however, for theoretically robust conceptions of epistemic success and epistemic activity that can be applied to animals, and on their own terms. I hope to show that analytic epistemology offers tools to accomplish these tasks.

On the Epistemological Analysis of Modeling and Computational Error in the Mathematical Sciences

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Interest in the computational aspects of modeling has been steadily growing in philosophy of science. This paper aims to advance the discussion by articulating the way in which modeling and computational errors are related and by explaining the significance of error management strategies for the rational reconstruction of scientific practice. To this end, I first characterize the role and nature of modeling error in relation to a recipe for model construction known as Euler's recipe. I then describe a general model that allows us to assess the quality of numerical solutions in terms of measures of computational errors that are completely interpretable in

terms of modeling error. Finally, I emphasize that this type of error analysis involves forms of perturbation analysis that go beyond the basic model-theorical and statistical/probabilistic tools typically used to characterize the scientific method; this demands that we revise and complement our reconstructive toolbox in a way that can affect our normative image of science.

Carnap as Conceptual Engineer?

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Using formal syntax and semantics, Rudolf Carnap suggested how it is pos- sible to replace vague or imprecise concepts, the explicandum, with exactly defined concepts in some logical language, the explicatum. Philosophers of science, like André Carus or Richard Creath, have recently suggested that this method of explication forms the kernel of an alternative conception for doing the philosophy of science. In particular, Creath has argued that Car- nap's method of explication can be seen as forming a " positive project" which allows for notions of philosophical progress and for a fruitful relationship between philosophy and science. Central to Creath's understanding of this project is the suggestion that explications can be understood as a sort of con- ceptual engineering: just as there are better or worse engineering projects, there are only better or worse explicatums of an explicandum. In my paper, understood as either a metaphor or analogy, I analyze what could possibly be meant by this comparison between engineering and explication. I first pro- vide a characterization of what engineers actually do and then pinpoint those places in Carnap's work that would seem to be examples of such engineering. I then argue how the two are similar or dissimilar. I then suggest various ways in which this engineering conception is related to Carnap's accounts of empiricism and linguistic conventionalism (viz. as a mature version of his earlier principle of tolerance). I then reassess Creath's proposal in light of this analysis.

What is the Internal Logic of Constructive Mathematics? The Gel'fond – Schneider Theorem in Transcendental Number Theory

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The question of an internal logic of mathematical practice is examined from a finitist point of view. The Gel'fond-Schneider theorem in transcendental number theory serves as an instance of a proof-theoretical investigation motivated and justified by a constructivist philosophy of logic and mathematics. Beyond the Gel'fond-Schneider theorem, transfinite induction is put to the test and is shown to be operating in most foundational programmes, from Voevodsky's univalent foundations and Martin-Löf's intuitionistic type theory or Mochizuki's inter-universal geometry for the abc conjecture. I argue finally that intuitionistic logic is not sufficient to handle

constructive mathematics and a polynomial modular logic is proposed as the internal logic of Fermat-Kronecker « general arithmetic » (see Gauthier 2013) for constructivist foundations of mathematics. The foundational perspective is briefly contrasted with a naturalistic philosophy defended by the philosopher of mathematics Penelope Maddy.

Beyond the Magnetic Earth: A Re-assessment of Experimentation in William Gilbert's De magnete

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Whenever William Gilbert's De magnete (1600) is given scholarly attention, its experimental character shines forth. However, the functions Gilbert's experiments play in specific contexts of problem solving have never been addressed. This paper intends to rectify that by reconstructing some of Gilbert's experiments and analyzing how he used them to address a range of questions about magnetic attraction (or, in Gilbert's terms "magnetic coition"). As a consequence of his intensive experimentation, Gilbert concluded that the traditional conceptual apparatus was not suited to handling the phenomena of magnetic attraction. A new conception of magnetic attraction was needed! I claim that Gilbert's solutions to this problem were formulated in strict dependency with the interpretations he gave to the experiments he performed. For Gilbert, magnetic attraction had the following distinctive properties: the mutual action of the bodies; immateriality (i.e., it involves no material exchange); and that it acts through a "sphere" (or "orbe") of influence, whose strength depends on the distribution of the magnetic "vigor" throughout a magnetic body, on its mass and on its shape. I show the degree to which the experiments were constitutive to the formulation of these properties.

Empirical Equivalence

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Most of us agree in dissenting from inductive scepticism. So let "h" abbreviate the statement of a hypothesis rendered probable by a body of observational evidence conjunctively reported by the proposition that e. In such a case,

e & h

and

e & ~h

will be alike consistent with – and both will in fact entail – all observational evidence to date. But (1), we have said, is supported by the evidence, and so (2) is not. Does empiricism rule out favouring one proposition over another even though both alike are consistent with the evidence – and indeed entail it? In that case, so much the worse for such empiricism.

How is being alike logically consistent with all possible evidence a different enough matter to make a difference? If consistency alike with all possible evidence precluded either of two conflicting propositions from being preferable from the standpoint of the goal of truth (on the matter in question), consistency alike with the evidence available now would prevent either proposition from being "alethically" preferable now, surely.

"Underdetermination" objections to scientific realism merely trade on the (traditional) Problem of Induction.

Scientific Institutions and Responsible Trust: Understanding the Implications of Situated Knowing

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This paper offers a contribution to understanding the epistemic relationship between lay persons and scientific experts by examining how networks of trust and trustworthiness are required for experts to be capable of conveying reliable knowledge to lay persons. Though many social epistemologists have attended to the epistemological underpinnings of trust in the testimony of other individuals, this paper investigates trust in the testimony of scientific institutions, considering the requirements of a 'responsible trust', where the trust in the institution matches the trustworthiness of the institution. The feminist thesis of socially-situated knowledge suggests that the trustworthiness of scientific institutions may not be the same from all vantage points (Scheman 2001). This point complicates issues of trust and trustworthiness, and threatens the ability of scientific institutions to carry legitimate cognitive authority across social locations. Adopting a situated approach to knowing, this paper argues that scientific institutions need to earn their trustworthiness across a broad range of social locations if they are to maintain their claims to cognitive authority. It examines the kinds of expectations that must be fulfilled by scientific institutions in order to earn epistemic trust, expectations that go beyond the production of reliable knowledge and include making sound choices of what kind of knowledge is important to produce, and communicating and filtering the results of knowledge pursuits. The paper uses two examples to help illustrate the depth of the challenges to establishing the epistemic trustworthiness of scientific institutions when they operate in climates of social marginalization: the genetic research on the Havasupai tribe in the late 1990s that resulted in a breach of trust between scientists and research subjects, and relations between southern Canadian environmental researchers and Inuit communities with respect to wildlife management and climate change.

Isaac Newton and Classical Theism

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Isaac Newton's theological writings have been the subject of intense interest and scholarship, revealing his heterodox leanings, his fascination with Biblical prophecy and eschatology, and even his search for an original, "Noahic," religion. However, in his published works Newton focused more on a form of natural theology, which is concerned with knowledge of God through the correct understanding of his created works.

While Newton's discussion of God's nature in the General Scholium of the Principia (1713) only hints at his heterodox leanings, it reveals directly his engagement with classical theism. What begins as an argument from design develops into a complete consideration of the nature and attributes of God. Major themes include God's dominion and the nature of his being (eternal, omnipresent, omniscient). Newton broaches what it means to refer to a being as "God" and presents the nature of the true God according to categories common to theological considerations in earlier Christian and Jewish thought: God's power, duration and place. These themes raise the question of what exactly Newton's views of God's attributes were and how they are related to previous thinkers in the Judeo-Christian tradition.

In this paper I look at Newton's engagement with some of the major themes of classical theism as outlined by thinkers such as Augustine, Anselm, Aquinas, Duns Scotus and William of Ockham. Medieval Jewish ideas on the nature and place of God (as found in Maimonides' Guide for the Perplexed) and the context of early modern classical theism in the works of the Reformers, the Book of Common Prayer and the Westminster Confession are also considered. Newton's emphasis on God's dominion is compared to the concept of sovereignty in Augustine and Calvin and the medieval debates on voluntarism and intellectualism. Additionally, in my evaluation of Newton's concepts of God's omnipresence and omniscience, I focus on how Newton treats the basic theistic attribute of God as all-pervasive spirit. The superlatives (supreme dominion, knowledge of everything, presence everywhere) make God the one true God, but there remains a further basic nature to "God" which is compared to earlier formulations of God's nature by the architects of classical theism.

'Supported by mathematics, yet...communicated without': J.T. Desaguliers and the Meaning of Public Demonstration for Newtonian Natural Philosophy

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In the preface to his Course of Experimental Philosophy (1745), John Theophilus Desaguliers wrote that his audience was those "little versed in mathematical sciences."1 Yet, that did not mean that he intended his course simply to satisfy casual curiosity. Instead, Newtonian physics was "supported by mathematics, yet its physical discoveries may be communicated without."2 What Desaguliers offered was an experimental demonstration of Isaac Newton's mathematical theories that allowed the expansion of Newton's audience beyond the tiny group of mathematicians for whom Newton had originally written.

In my paper, I will contend that Desaguliers' argument that Newtonian philosophy could be demonstrated without the math is a profound example of a transformation in how Newtonian philosophy was conceived as a philosophical framework. Desaguliers is indicative of a change from a philosophy which derived its authority from the strength of mathematics to one that was expressed in the material reality of the physical experimental demonstration. There was a transition from Robert Boyle's matters of fact, grounded as much in social status as in physical demonstration, to Newton's mathematical model of certainty, and finally to Desaguliers' experimentally demonstrated, physical and objective fact. This shift was crucial for the final establishment of the Newtonian hegemony in eighteenth-century Britain. Desaguliers showed, rather than told, the matters of fact he wished to prove. In doing so he reconciled the demonstrability of Newton's experimental philosophy with the mathematical difficulty that had previously made Newton unapproachable. By removing the mathematical veil that had obscured Newton's philosophy, Desaguliers suggested that anyone could participate.

The Regulation of Scientific Research in Publicly Funded Institutions: The Case of Pluripotent Stem Cell Research in Ontario

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Stem cell science is seen as potentially paradigm-changing in medicine with great promise for eventually treating a range of diseases and conditions like diabetes, cancer, spinal cord injury, stroke, heart diseases and neurodegenerative diseases. Stem cells were discovered in Ontario in 1961 and the province has a high concentration of world-class researchers. At the same time, exceptionally, Canada has criminalized certain classes of experiments relevant to the stem cell field. This paper is based on doctoral research for an ethnographic study of pluripotent stem cell research undertaken in publicly funded laboratories in the Toronto area. Taking the greater oversight and ethical controversy over stem cell research as its point of departure, the research explores the points of contact between Canadian laws and regulations and stem cell scientists' everyday research as they are mediated by university and hospital research services such as ethics review boards and technology transfer offices. The paper will focus on the following questions: What are the institutional processes through which ethical concerns, as manifested in research regulations and institutional norms, are embodied in specific research programs and practices? The controversy over stem cell research has been over the moral status of the embryo; as more and more stem cell research projects move into clinical trials, what new ethical concerns are emerging? I hope to contribute to a broader discussion on the impact of regulation on bioscience discovery and innovation and the interplay of scientific research and societal values.

Rudner's Challenge

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I defend Richard Rudner's thesis that value judgements are necessary in science by arguing that his opponents have failed to recognize both the scope and force of Rudner's argument. The challenge Rudner issues is to answer the question "How much evidential justification is sufficient for theory acceptance?" without appealing to non-epistemic values.

In response, many philosophers have pointed out that scientists need not accept or reject theories at all; they might merely assess probabilities of truth, leaving judgements of evidential sufficiency to those who need to apply, and hence accept, hypotheses in practical application. Distinguishing between practical and epistemic judgements does not answer the epistemic question Rudner poses about sufficiency of evidence, however. Philosophers of science wonder when a hypothesis is sufficiently justified even if scientists do not; philosophers cannot simply play the skeptic and suspend epistemic judgement on all scientific claims.

Not all judgements of sufficiency are probabilistic either. Scientific methods of justification vary qualitatively, not just quantitatively, in part due to the variation in goals across research contexts. Laboratory research and field observations in ecology, for instance, yield different kinds of evidential support, not just different probabilities of truth. While many opponents of Rudner's argument address only probabilistic questions, Hugh Lacey has recently attempted to account for both quantitative and qualitative variation in epistemic standards while preserving a value-free account of scientific reasoning. I argue that his view, like past responses to Rudner's challenge, does not provide a plausible value-free alternative account of evidential sufficiency.

Categorizing a Cabinet of Curiosity: Analyzing "The Preface" of John Tradescant and the Royal Society's Catalogues

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The notion of historia in Early Modern England describes how the early disciplines of science and the arts were inherently interconnected (See Pomata 2005). Medicine, natural history, philology, and antiquarianism, to name a few, were practiced by many men, and this multidisciplinary practice continued through to the past time of collecting. Examples of men whose collecting reflect this encyclopaedic notion of historia include English naturalist John Tradescant the Elder, as well as the various Fellows of the Royal Society. Respectively, each owned a cabinet of curiosity which, coinciding with the notion of historia, spanned many categories and disciplines. Further, each cabinet published and distributed a catalogue for public consumption, which is the focus of this study. I examine the preface of each catalogue and explore the author's explanations and influences for his categorizations of the wide variety of objects found within the cabinets. It is in these prefaces that each author dictates the categories used within the catalogue, the scholarly influences, as well as giving reasoning for the information divulged within the specific catalogue entries for each object. From this, we can learn and compare the epistemic practices between a private and intuitional cabinet, which each then sought to impose on their audience via printed distribution of their catalogues.

Watts Across the Border -- Technical Standards and Continental Integration

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The rise of science-based industry during the Second Industrial Revolution drew firms, beginning with the railways, into increasingly exacting programmes of standardization. Technical standards were developed and implemented using laboratory tools and the language of science and were important means by which scientific control of production was achieved. In North America, the development of such standards took place in an economic context of the integration of the Canadian and United States economies, in particular the manufacturing sectors. To put it simply, American light bulbs had to screw in to Canadian sockets, Canadian prongs had to fit in American plugs and a watt and a volt had mean quite closely the same thing in Saskatoon and Tuscaloosa. This paper examines the means by which such a scientific and technological convergence was achieved.

The fire without light and the missing foundations of Descartes' physiology

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I argue that Descartes' physiology is not the foundational, hierarchical enterprise it appears, but works on the basis of interdependency. Initially, it looks as though physiology should be an archetypal exercise in Cartesian foundationalism: in the living body, it has a tightly-delineated object of study, whose concomitant phenomena should be wholly explicable on the basis of a solid foundational principle, as Descartes appears to claim. It turns out, however, that what ought to be the solid foundation (the 'principle of life') is anything but. His accounts of this 'principle' are vague, fragmentary, and changeable. I claim we can make better sense of this treatment if we take Descartes' hints at the interdependency of bodily systems seriously: the principle of life is just as dependent on other systems as they are on it. Following the logic of Descartes' physiology through, the account of the living body gets constructed through interdependency, rather than hierarchically. Accordingly, Descartes never needed a solid principle of life, because it was never foundational.

Quality Space Theory: An "Objective Phenomenology"

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Sensory experiences possess certain phenomenal qualities such that there is--as Thomas Nagel (1974) puts it--'something that it's like' to undergo that experience. Nagel's 1974 paper captured the view of phenomenal qualities that has dominated thinking in philosophy of mind and philosophy of science ever since: the worry that scientific ("objective") accounts of sensory experience leave something out; namely, its qualitative phenomenology. Nagel argues that in order to make progress on the problem of phenomenal qualities, we need to develop an "objective phenomenology"; an empirical, third-person approach to phenomenal qualities. In this paper, I examine a proposed theory of phenomenal qualities that promises to offer just the kind of objective phenomenology that Nagel calls for: quality space theory.
At its essence, quality space theory (QST) is primarily intended to be a radically empirical theory of phenomenal qualities; one which holds that phenomenal qualities can be investigated by the empirical sciences and thus integrated into a scientific-naturalistic worldview. Although QST has enjoyed somewhat of a resurgence in contemporary philosophy of mind (Rosenthal, 2010; Clark, 1992, 2000; etc.), it also has a long history, going back at least as far as Carnap (1927) and the logical positivists.

In this paper I examine quality space theory and its importance to philosophy of mind. I describe the empirical methodology of constructing a quality space (using psychophysics and neuroscience), and I describe how this approach provides an explanation for phenomenal qualities by showing how quality space theory fares from the perspective of philosophy of science. More specifically, I examine (i) how QST fits into the tradition of structuralism in the philosophy of science and its roots in logical positivism; and (ii) how QST fits into the programme of mechanistic explanation that has become popular in contemporary philosophy of neuroscience (e.g., Bechtel 2007, Craver 2008).

Why Can't We "Test" Scientific Realism Against History of Science? A Disagreement to Realist Gambits

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In order to rescue scientific realism from the challenge from history of science (Pessimistic Induction, articulated by Larry Laudan), Stathis Psillos claims that we may entertain differentiated degrees of belief towards constituents of past scientific knowledge. He believes that the scientific realist need not have to endorse a positive epistemic attitude across history of science (or even present science). This simply means that the scientific realist can be non-realist at times. But Psillos does not specify the kind of non-realism we should adhere to. Firstly, I argue that, when we become selective in our epistemic attitudes, then there is no way by which we can filter out antirealisms. This means that the realist slogan of 'unobservables exist' does not apply across science. That is, we are forced to entertain negative epistemic attitudes to revision-prone constituents in the history of science, and thus antirealisms also become intellectually appealing. Secondly, Psillos calls for a naturalistic approach in raising our epistemic attitudes to scientific knowledge, i.e. he claims that the scientists themselves are capable of spotting those constituents of theories which are well supported by evidence. I argue that, since scientific realism is a generic position which endorses a positive epistemic attitude across science, naturalizing and splitting the epistemic attitude into differentiated degrees of belief, in a certain sense, delivers the idea that scientific realism is a position that can be tested against history of science. I show that the very idea of 'testing' or even weighing a philosophical formulation like scientific realism against history of science is flawed from the very start. This is because the generic nature of its tenets itself is a hindrance in splitting the positive epistemic attitude the realist presupposes.

An Ethnography of Mutual Aid

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Peter Alexeyevich Kropotkin's explorations in Siberia and Scandinavia between 1864-1873, and the international scientific fame that resulted, placed him squarely within the 19th century discourse of the travel narrative, a framework I will utilize to construct an ethnography of mutual aid within the Darwinian and Humboldtian tradition. As Kathleen Roberts has discussed in her work on alterity and the Western narrative, identity is always negotiated toward

Otherness. However, unlike the expedition of his English contemporary, and later intellectual foil, Thomas Henry Huxley, Kropotkin's encounter with indigenous populations is notable in its lack of colonial attitudes of superiority and racialized discourse. By first considering the different ethnographic perspectives on indigenous and cultural "others" held by Huxley and Kropotkin during their travels (at the same time that both worked on behalf of their respective empires and relied on military power for their research) I will examine how nineteenth century racial views contributed to the development and critical reception of their respective scientific theories of human evolution. My paper will trace the early development of Kropotkin's theory of mutual aid as it related to indigenous populations through the correspondence with his brother during his expeditions and his Siberian travel diaries (neither of which have been translated into English) as well as recently discovered archival material.

X-Phi, Explication, and Formal Epistemology

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Recent work in experimental philosophy (henceforth, x-phi) challenges the role of intuitions in concept determination, and in philosophical and scientific theorizing generally. But considered as a single movement, x-phi offers few unequivocal answers and leaves important questions about proper positive methodology in philosophy unanswered. For example, several commentators have argued that experimental results can be interpreted in distinct ways, some of which vitiate their philosophical import. The general significance of x-phi therefore remains unclear. And with intuition-based approaches to philosophical theorizing only recently tarnished, alternative methods remain largely unexplored. Here (and elsewhere) the explicative methodology Carnap championed bears salutary philosophical fruit. Using concept determination in formal epistemology as a case study, we argue the following. First, explication clarifies x-phi's philosophical import, a hitherto highly contentious issue. Second, explication does the same for formal epistemology by supplying a cogent naturalized rationale for favoring formal approaches to epistemological issues over traditional ones. This rationale shows how formal epistemology can avoid the criticisms made of intuition-based philosophical methodology, and how x-phi can play an important role in its doing so. And last, applying explication to formal epistemology reveals a problem with Carnap's account of the former, and how it can be fixed.

False Models and True Predictions; the Role of Maxwell's Ether Models

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I argue that even though fictitious models entail descriptive falsity, they are nevertheless conducive of scientific truth, to the extent that they are accurate with respect to the essential features that determine the validity of the reasoning. I examine the role played by Maxwell's ether models in the discovery and mathematization of the displacement current, and evaluate the prima facie problem of how a false model can lead to correct predictions. In the pursuit of this goal, I emphasize the need to distinguish more rigorously between modeling assumptions and models of representation; we must not fail to distinguish between asking whether modeling assumptions represent the world and asking whether the resultant set of model equations represents the world.

If this distinction is observed, we are in a position to recognize that there are both relevant and irrelevant modeling assumptions for a given model, and that moreover, there will be mathematical or physical reasons why certain assumptions are irrelevant. To illustrate I draw on a geometrical construction problem posed by Polya, and compare it to the relationship between the ether models and Maxwell's equations. I conclude that if the modeling assumptions that result in the falsity of the model are mathematically or physically irrelevant, then the prima facie problem of fictitious models disappears. Drawing on work from Batterman, I will explain the notion of relevance in terms of multiple realizability. I conclude that Maxwell's use of the ether models demonstrates a much more sophisticated conception of epistemic entities than either realists or anti-realists, a conception that captures the epistemic relationship between an entity and its instantiations.

Post-War Scientific Politicking and the Acquisition of the Electron Linear Accelerator at the University of Toronto

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This paper will contribute to our understanding of the development of experimental science in Canada after the Second World War by examining the purchase of an in-house linear accelerator for the Department of Physics at the University of Toronto. The research is based on the personal notes and private correspondence of Kenneth McNeill, who led the nuclear laboratory during the 1960s and 70s, and on oral history interviews with Emeritus Professors. The paper argues that it was not the scientific merit of having a linear accelerator that won funding from the National Research Council and the University, but McNeill's skills at networking and bureaucratic politicking.

The project was beset by difficulties from the very beginning. When the machine was installed in 1966, it failed to reach the required energies. The Linac Committee was forced to decide whether to accept the machine or to reject it at a $2,000,000 loss. The unsurprising choice was made to accept it, but not all of the planned experiments were possible. This led to a unilateral takeover of the Linac Committee by the university administration in 1969, who pressed new, high-profile experiments onto the researchers – experiments that were beyond the accelerator 's capacity. Disappointing results and the demands of inter- institutional competition drained the Toronto linac of its resources, leading to its decommissioning in 1978. This shows that while networking and political skills were instrumental in bringing the project about, it was these same bureaucratic forces that ultimately hobbled the project.

Natural Kinds, Social Kinds, Eternal Kinds, and Copied Kinds

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Do the social sciences aim to discover social kinds, as the natural sciences aim to discover natural kinds? Or are there fundamental differences between the two realms? There is a provocative distinction due to Millikan (2000; 2005) between "copied kinds" and "eternal kinds." Millikan suggests that many kinds in the natural world are eternal kinds, whose members resemble one another as a result of natural law, but other kinds, in both the biological and social realms are copied kinds, whose members resemble one another as a result of a copying process.

I will argue that on closer inspection, this distinction does not provide us with a way of discriminating the two kinds of kinds. To begin with, it seems that some physical and chemical kinds can be regarded as copied kinds. Consider the chemical compound DNA. All molecules of DNA are similar, at least in part, as a consequence of the operation of the laws of chemistry, which specify which elements can combine in certain combinations and which compounds are stable. But all existing molecules of DNA are apparently also copied (often imperfectly) from the same original DNA molecule. Conversely, some instances of social kinds, for example, government, marriage, and racism, are arguably not the result of a straightforward copying process, but arose independently in different human societies as a result of widespread human capacities or nearly universal human tendencies (albeit not ironclad laws of human nature). Further interrogation of the distinction suggests that it does not enable us to make a fundamental distinction between natural kinds and social kinds.

Problems with Pluralism and Emergent Causality
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Causal approaches to explanation are widely considered by philosophers of science to be the best accounts in many areas of science. But some philosophers of biology have claimed the complexity of biological systems makes the phenomena in question inherently in intractable. Sandra Mitchell has been very influential in arguing for a kind of pluralism in which an explanation would feature various integrated partial causes. She has argued that emergent phenomena and higher level causal dependencies are necessary for these explanations. In this paper I argue against the need for this kind of emergence and pluralism which supposedly results from studies on complex biological systems. The error lies in the veridicality of causal accounts of explanation. The pluralists are correct that there are limits to predictability in complex systems, but I will be arguing that pluralists mistake the rather uninteresting need for shortcuts and predictions in scientific practice as a reason to adopt emergentism. Pluralists take the necessarily abstract and idealized models used in explanations as truthful representations of higher level, and even inter-level, entities and causal dependencies. Because of the necessity of abstraction and idealization involved in all explanations, the argument fails to demonstrate the need for higher level entities and causes. This implies that a deductive account which is not concerned with complete truthful representation is still capable of handling such cases.

Schematic Representation in Hertz's Principles of Mechanics
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In Scientific Representation van Fraassen purports to solve the problem of coordinating theoretical structures with reality by treating them like maps and appealing to self-attribution of location during theory-laden observation. He claims the historical Bildtheorie view that science gives us pictorial representations of reality is closely connected with his structuralism and faces the same problem. However, his interpretation of this view in Hertz's Principles of Mechanics makes a common mistake of running together subtle notions of "inner image of external objects", "model" and "scientific representation". Rather than succumb to a problem of coordination, the Principles shows how it arises.

In this paper I reveal how the Principles builds upon a neo-Kantian idea implicit in Hertz's Electric Waves. Influenced by Helmholtz, Hertz's starting position is that we access reality through sensation by producing "inner images of external objects". Physicists are further conditioned by their education to construct images that are theoretically informed. These images exhibit a fundamental structure, or schema, that captures a physical interpretation of equations constituting a theory. Just as an architect might begin with a house and reconstruct the blueprint for its construction, a scientist can begin with her theory-laden image and

reconstruct a schema for its projection. This "scientific representation" is a general image correlated to basic principles relating fundamental concepts interpreted through "laws of transformation". When given mathematical form, it is taken to project idealized "models" of external objects. For Hertz a problem of coordination can arise only if philosophers reify a reconstruction from theory-laden observation.

Savants, Amateurs et Curieux en France au XVIIIe siècle : à la frontière de l'utile et de l'agréable

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Depuis les années 1980, des historiens comme Daniel Roche utilisent le concept de « sociabilité intellectuelle » pour étudier les académies et autres cercles savants au XVIIIe siècle (Van Damme 1997). La notion de « plaisir intellectuel » ou « plaisir de l'esprit », c'est-à-dire le plaisir comme motivation de l'intérêt scientifique, passe encore aujourd'hui complètement inaperçue. Pourtant, elle est peut-être une voie d'entrée pour comprendre la construction des savoirs scientifiques. Sous quelles formes se manifeste le plaisir intellectuel, comment l'exprime-on, quel rôle ou fonction lui reconnaît-on au XVIIIe siècle?

La rupture idéologique entre plaisir et savoir rationnel ne s'est pas encore produite au XVIIIe siècle. Curiosité, amateurisme, collectionnisme : voilà autant de réalités alors en expansion (Glorieux 2002), et qui illustrent cette zone grise qu'occupaient alors les sciences à une époque où celles-ci croisaient davantage l'univers du sensible. Comme l'exprime à juste titre Alain Corbin : « l'histoire des techniques a une sorte de dette, mal perçue ou encore peu reconnue à l'égard de l'histoire des sensibilities (Heuré 2000). » Les catalogues des cabinets de curiosité, les ouvrages philosophiques sur le plaisir et même la correspondance des administrateurs de la maison du roi en font foi.

Dr. Thomas Beddoes (1760-1808) and his library

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Beddoes is mainly remembered for his researches in pneumatic medicine, and especially for the Pneumatic Institution in Bristol, where Humphry Davy was his chemical operator. He is also remembered for his political activism, and for his friendship with many leading men of science. He read voraciously, and assembled one of the finest private libraries in medicine, chemistry and belles lettres. Beddoes assembled an extraordinary collection of German scholarship in the period of the French wars, when shipments were chancy at best. When Beddoes died, his library was sold by Leigh and Sotheby, in an auction that lasted ten days, and where the catalogue lists over two thousand lots. I shall discuss both the contents of Beddoes's library, and the ways in which he managed to acquire foreign books, using diplomatic bags, business offices, members of parliament, and more.

The VIBE Theory of Public Languages

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A commonsense view says that languages are publicly shared things, and that linguistics is about them. The received view in Chomskyan theoretical linguistics, however, is that a) there are no such things and that b) linguistics is, instead, about concrete psychological states of individual language users. Several philosophers have tried to resuscitate the old-fashioned view on the grounds of 1) the inability of the psychological philosophy to explain linguistic communication and 2) its inability to explain the notions of correct and incorrect language usage.

While much has been written on both sides of this philosophical divide, there exists a surprising hole in the research that has been put forward. Chomsky and his followers have responded, rather dismissively, that linguistics simply has no interest in linguistic communication and normativity. However, this practical response fails to address what I take to be the real philosophical thrust of the objection. The real concern is that the psychological philosophy of language might be inconsistent with the existence of linguistic norms and communication. An original contribution of my paper will be to explain thoroughly how the psychological philosophy of linguistics is fully consistent with the existence of linguistic norms and communication and, indeed, that this philosophy offers a helpful paradigm for explaining and understanding these phenomena.

The Faith of Scientific Naturalism

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Just three years before his death, the biologist Thomas Henry Huxley, a leading agnostic, drew a parallel between a key Christian belief and the scientific theory he had defended so fiercely since the publication of Darwin's Origin of Species in 1859. If the doctrine of Providence was held to imply that in some "remote past aeon" the cosmic process was started by "some entity possessed of intelligence and foresight" superior in degree to our own, and if it was held that every event was foreknown, "scientific thought ... has nothing to say against that hypothesis." Such a hypothesis was "in fact an anthropomorphic rendering of the doctrine of evolution." Huxley's point, that there was a significant affinity between the Christian concept of Providence and the scientific doctrine of revolution, is telling. Here I will argue that Huxley, and two of his closest allies within the ranks of the scientific naturalists, the physicist John Tyndall and the philosopher Herbert Spencer, all drew on several Christian theological concepts to articulate, in a secularized form, some of their deepest beliefs about nature and the human condition. These beliefs were integral to their science and their vision of scientific progress. Through an examination of Huxley's views on teleology in nature, Spencer's presentation of an evolutionary theodicy, and Tyndall's thoughts on the implications of the first two laws of thermodynamics for earth's future, the close links between Christianity and scientific naturalism in the second half of the nineteenth century come into clear focus.

Observation and Simulation in Atmospheric Science

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Computer simulations have long resisted classification by philosophers. Thus far, debates regarding computer simulation have focused on comparing and contrasting simulation with traditional experimentation. While casting the debates in these terms has drawn attention to questions regarding simulation's materiality and its ability to replace traditional experimentation, the roles that computer simulations play in other kinds of scientific activity have largely been ignored. To fill this gap, I examine simulation's role in the "correction" of observational data, and argue that doing so sheds new light on questions regarding materiality while complicating the distinction between simulation and traditional measurement.

To draw attention to the role that simulation plays in other kinds of scientific activities, I examine the use of computer simulations in atmospheric physics and how they are used to stitch together disparate collections of historical climate data. This process, called reanalysis, is designed to overcome problems that stem from changes to the global observing system and create a homogenous dataset that can be used to discern climate trends. Reanalysis is philosophically interesting because it provides a novel example of how simulations can be connected to the physical world; enabling simulations to produce new empirical knowledge. This empirical knowledge seems to constitute, and in atmospheric science is sometimes used as, observational or measurement data. Such a conclusion further complicates simulation's relationship with traditional scientific practices and prompts a reconsideration of simulation's possible role in measurement.

United by Science and Harried by Revolution: Thomas Beddoes' Swiss Friends at Edinburgh

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When Thomas Beddoes (1760-1808) went to Edinburgh in 1784 he encountered the International Enlightenment. His medical and science courses brought him into contact with students from Europe, the Americas, and many parts of Britain. Two of Beddoes' Swiss classmates were Abraham Guyot (1743-94) and Dr Pierre Sylvestre (b. 1759). These three men were united by science, but came to be harried by revolution.

A preview to this persecution occurred in 1782. Sylvestre, a physician from Geneva, was expelled after participating in its truncated revolution. At Edinburgh he met Guyot, an itinerant tutor and amateur scientist from Neufchâtel, and Beddoes, a translator from Shropshire, who had studied at Oxford. The three men immersed themselves in learning at the university, and public science in local societies. They also joined a nascent British-Franco-Swiss network. It connected them to groups transcending Enlightenment science and industry, like Birmingham's Lunar Society. A chance friendship in Scotland, in a country to which none of them were native, demonstrates the breadth of this network. Despite great diversity, its members were harassed on multiple sides by late eighteenth-century revolutions. However, the network sustained Beddoes, Guyot, and Sylvestre as their careers, and even lives, where threatened during the turbulent 1790s.

Volunteristic Epistemology and the Current State of the Scientific Realism Debate

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The realism debate has seen a voluntaristic turn in recent years, with parties on both sides of the debate adopting it in one form or another. Psillos for example acknowledges that his response to van Fraassen's empiricism requires adopting an "epistemic optimism". Van Fraassen himself allows that his constructive empiricism is not a set of beliefs, but rather a "stance" one voluntarily takes toward beliefs. Nevertheless, both Psillos and van Fraassen argue for the adoption of their preferred respective positions in spite of their voluntaristic nature. This move has also been mirrored by structural realists as well. Ross and Ladyman follow van Fraassen in adopting his so called "stance stance" (as well as his rejection of "analytic metaphysics") yet advance a case for their version of structural realism. In this paper I will address the plausibility of this voluntarist turn. Although I will address a number difficulties with voluntarism, I will argue that its adopters share a common mistake. This is to seek global stance to the realism question. We will argue that the realism question can be addressed more satisfactorily by adopting certain deflationary views that permit local rather than global solutions to the realism question. I will also try to show that when such a position is taken, the

reasons for adopting realism or empiricism in a given situation can be more compelling than voluntarism in general or the stance stance in particular.

Two Pictures of Thought Experiments
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Each scientific thought experiment is a picturesque argument, says John Norton. Michael Bishop objects. Norton, he argues, can't account for a historical fact. Namely, when Bohr disputed Einstein's Clock in a Box thought experiment, one thought experiment was reconstructed as two arguments. Norton replies that this isn't how it went. Rather, two thought experiments were reconstructed as two arguments. I argue that Norton and Bishop talk past each other--for each relies on a different picture of thought experiments. I then consider two objections. One is that Norton's picture makes Einstein and Bohr's disagreement irrational. This picture, however, isn't in the business of historical description. The other objection is that I forego the explanatory power of a unified account of thought experiments. Parallel accounts, however, might do just as well. After all, Alisa Bokulich's account, which relies on Bishop's picture, and accounts like Timothy Williamson's, which rely on Norton's, may run in parallel and explain more phenomena than either one alone.

Scientific Expeditions as the Core of Russia's Colonial Science Project: Borderlands of the Empire in Mid-Nineteenth Century
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The nineteenth century saw the highest point of the geographical expansion of the Russian Empire, and it was at this time that the polity which had been incorporating neighbouring territories for several centuries acquired a distinctive character of a colonial power that began to understand itself as such. As new territories were annexed to the Russian state, a demand for their scientific exploration grew steadily, as the questions of governance, national glory and scientific advancement became intermingled in the creation of a Russian colonial science project.

Two disciplines were most salient in this project: geography and ethnography. No scientific project involving exploration of imperial spaces could do without any of them, and even primarily geographical expeditions reported on ethnography of a region and vice versa. The proposed paper analyses four specific texts pertaining to the colonial science project: Matthias Alexander Castrén's notes on his explorations of Northern Russia, Lapland and Siberia in the 1840s, Richard Maack's notes on his expedition to the Amur river in 1855, ethnographic notes on Siberia made by Ippolit Zavalishin in the 1860s, and Alexander von Middendorff's notes on his Siberian expedition published throughout 1860s and 1870s. The paper argues that these texts are ultimately united by their colonial nature: typically perceived either as scholarly works far removed from imperial politics or, in Zavalishin's case, as a popular ethnography-cum-polemic, they in fact represented the colonial drive for knowledge of imperial edges. The quest for knowledge of imperial frontiers, borderlands and recently colonised spaces ultimately followed the colonial logic of dominating the nature and its inhabitants pro majore imperii gloria. In following this quest, imperial geographers-cum-ethnographers performed the role of imperial scientists as they understood it, and the paper argues that their vision of what such science and scientists should be was instrumental in creating and congealing the overarching colonial science project.

Dynamical systems and graph-theoretic approaches to the brain in explanation and discovery

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Explanation in biology and neuroscience is mechanistic in that it consists in identifying the components, activities and organizational features of the system that produce, underlie or maintain a given phenomenon (Bechtel & Richardson, 2011, Kaplan & Craver, 2011). This widely accepted view has been challenged in cognitive and systems neuroscience in the case of particular dynamical systems of graph-theoretical cognitive networks, as these explanations violate localization and decomposability that are required in the standard view (Silberstein & Chemero, 2012). This challenge draws on practices in systems neuroscience that views such systems not as vectors of activity or neural signals, but as dynamically evolving graphs, and that these networks of the brain are basic units of explanation (Sporns 2011). Consequently they cannot be accounted for by dividing the practice into complementary topological and mechanistic explanations (Huneman, 2010). I argue that the "topological explanations plus mechanistic explanations" picture is satisfactory by considering a case of their use in explaining Alzheimer's disease.

Gendering Animals

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In sketching a new possible paradigm for sex difference research, Rebecca Jordan-Young introduces the idea of gendered norms of reaction, which describe how from any given point in a person's development--from conception to old age--there are a range of possible sex/gender/sexuality outcomes, depending on a person's experiences, activities and environment (2010, 271-86). In effect, Jordan-Young is joining other feminist critics in calling for scientists to abandon essentialist, dualist approaches (specifically brain organization theory) and begin to study sexual development and variation as a psychologically complex, physiologically and phenomenologically embodied, socially located, life-long journey of

becoming using methods that are systematic and do not presuppose an essentialist two-sex model.

The concept of gendered norms of reaction opens up a tantalizing possibility regarding future studies of sex differences in nonhuman animals, which remains underexplored. When investigating the sex-typical behaviors of intensely social nonhuman animals, it may be that a reductionist biological two sex model is just as misleading as it is in the human case. Using the lens of gendered norms of reaction, I will consider what it might mean to say that animals other than humans are gendered in a scientifically respectable sense that does not simply reduce gender to sex. I will also explain why rhetorically such an account may be useful for feminist ends.

Science, Death, and the Eighteenth-Century Vampire Debates

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In the early eighteenth century, reports of vampire outbreaks began to emerge out of Eastern Europe. Perhaps the most famous case was that of Arnold Paole, a Serbian villager whose return from the dead was chronicled in great detail by Austrian army surgeon Johann Flückinger in Visum et Repertum (1732). Reports of vampiric activity, including Flückinger's , circulated widely in Western Europe, where they were the subject of intense interest. Some scholars dismissed the very possibility that vampires could exist. Others, however, insisted that the reports merited serious consideration, due to their consistency and the reputation and social standing of their authors. This paper will explore the role that science and medicine played in the eighteenth-century vampire debates. This role was significant, as the debates hinged on the question of whether the events described in the reports were physically possible. The paper will focus on Dom Augustin Calmet's best-selling Treatise of Vampires and Revenants (1746). Calmet draws on eighteenth-century medicine, physiology, and physics in an attempt to provide "natural" explanations for stories of revenants returning from the grave and exhumed corpses "in whom are still found signs of life: the blood in a liquid state, the flesh entire, the complexion fine and florid." In examining these stories, Calmet is forced to consider the limits of nature and natural powers.

Peirce and Smolin on Cosmological Evolution

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Lee Smolin, theoretical physicist at the Perimeter Institute, credits Charles Sanders Peirce with anticipating (by about a hundred years) some of his own ideas about cosmological evolution. To back up this claim, Smolin sometimes cites the following passage from Peirce's famous 1891 Monist article called "The Architecture of Theories":

"To suppose universal laws of nature capable of being apprehended by the mind and yet having no reason for their special forms, but standing inexplicable and rational, is hardly a justifiable position. Uniformities are precisely the sort of facts that need to be accounted for. Law is par excellence the thing that wants a reason. Now the only way of accounting for the laws of nature, and uniformity in general, is to suppose them results of evolution."

Smolin sometimes refers to his own theory as "cosmological natural selection" and suggests that a process analogous to Darwinian natural selection takes place in the large-scale development of the universe. When black holes collapse, this collapse give rise to a new universe, with each universe giving rise to as many new universes as it has black holes. Some new universes will suffer heat death without successfully reproducing.

The aim of this paper is to explore the similarities and differences between Peirce and Smolin on questions related to laws of nature and to cosmological evolution.

Causation by Omission and Causal Judgments

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Opinions diverge on whether an omission can be a cause. On the one hand, there is the broad acceptance of causation by omissions, suggesting that there are many more causes by omission than what we would normally accept at first glance. On the other hand, there are those who want to deny causation by omission because of its lack of parsimony or the employment of a non-event as a cause. Regardless of which endorsement one makes concerning the metaphysics of causation by omission, it is generally accepted that omissions do play a role in our causal judgments. This paper investigates, without the standard appeal to pragmatics, why some omissions are accepted as causally relevant in our judgments while others are not.

I evaluate two attempts to establish a principled method for more parsimonious judgments concerning causal omissions. The first is from Sarah McGrath, who suggests a standards-based condition wherein the standards we employ in causal judgments are situation-relative and are used to distinguish between situations in which our expectations are met and those in which they are not. The second view is James Woodward's appeal to the sensitivity of counterfactuals as potential indicators for why we judge certain omissions as relevant and others as not. Contrasting each view with a set of examples, I propose that McGrath's method better explains our causal judgments concerning which omissions are causally relevant and which are not, without mere appeal to pragmatics.

Seeing Canada with Scientific Eyes: The Western Expeditions of the 1897 and 1924 Toronto Meetings of the British Association for the Advancement of Science

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The annual Meeting of the British Association for the Advancement of Science was hosted by the University of Toronto both in 1897 and 1924 (in conjunction with the International Mathematical Congress of that year). In both years the conferences were followed by Western Expeditions, by train from Toronto to Vancouver, then by boat to Victoria and back, followed by a return to Toronto by rail.

These expeditions, like the Meetings in Toronto, mobilised major support from the University of Toronto but also other Canadian Universities. Furthermore, government (local, territorial, provincial and federal) provided solid help in displaying Canada's natural, scientific, agricultural, and industrial resources to car loads of distinguished foreign visitors. Transportation companies (rail, water and road) offered preferred rates to Canada's scientific guests.

There were many stops and side trips en route, several catering to specific disciplines such as Geology or Anthropology. The travelers were overwhelmed by official and unofficial hospitality from Provincial Legislatures, City Councils, universities, research stations, mining companies, fraternal organisations, golf clubs, etc. Their passage was thoroughly reported in the local press especially when they spoke publicly about their scientific work.

How bacteria socialize: individuality, cooperation, and conflict

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Individuals in selection are expected to contain parts that cooperate without conflict. The exact relation between individuality and conflict is a debatable topic, however. Paradigmatic individuals like ourselves and other mammals can contain parts that compete with each other, such as cancer cells; and in non-individuals such as human societies both competition and cooperation can occur. A more precise account of the relation between individuality and conflict is provided by 'policing' theories of individuality, which maintain that individuals in selection should contain mechanisms that repress within-individual competition (e.g., Clarke in

press, Michod 1999). The goal of this talk is to evaluate policing theories of individuality by using multispecies biofilms, a type of bacterial community, as a test case. Multispecies biofilms are particularly suited to address this topic because, in addition to being a significant mode of life of bacteria, biofilms can exhibit tight cooperation relations despite lacking policing mechanisms. As far as we know, cooperation in multispecies biofilms is forged by shared interests without enforcement mechanisms. Biofilms thus suggest that policing mechanisms may be an unnecessary feature of individuals in selection.

How big do infinitesimals need to be in infinite fair lotteries?

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Bayesian confirmation theory is often seen as requiring regularity, the assumption that every possible situation has non-zero probability. Infinite fair lotteries seem to be a counterexample to regularity, since any positive real number assigned as the probability of a ticket winning yields a violation of the finite additivity and/or total probability axioms. Yet such lotteries come up in science. If we live in a multiverse or infinite universe with infinitely many observers, then the empirical conditions that I observe seem to be a function of the outcome of an infinite fair lottery where the tickets are all the observers, and thus to come up with empirical predictions from scientific theories that involve such infinities seems to require an account of infinite fair lotteries. One solution to the regularity problem for infinite fair lotteries is to consider hyperreal probabilities and assign non-zero but infinitesimal probability to each ticket. I show that no hyperreal infinitesimal (or more generally, no infinitesimal in a real closed field extending the real numbers) is large enough to be the outcome probability for a countably infinite fair lottery. The paradigmatic example of an uncountable lottery is where one picks out a point on a target by throwing a dart with a perfectly defined point in a uniformly distributed way. But in such cases, I will use a target-rescaling argument to expand on Elga's undetermination objection by arguing that the size of the infinitesimal probabilities would have to depend in an implausible and inscrutable way on the laws.

Aboriginal Contributions to Science on the Northwest Coast between 1826 and 1860

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My work will detail the Aboriginal participation in, and contribution to, science in the context of Northwest Coast between 1826 and 1860. It will be shown that scientific work was significant and on-going during this early period and that Aboriginal people made substantial contributions to British imperial science and early American colonial science. It will be demonstrated that there was an intimate and enduring relationship between Aboriginal people and men of science. The cumulative effect of this intimate and enduring relationship over time shows a

substantial inter-action between knowledge systems. The assistance and employment of Aboriginal people in the collecting of scientific information illustrates that the production of science was a collaborative effort.

The Problem of Unbeneficial Features in Aristotle's Parts of Animals

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Aristotle's Parts of Animals is a rich resource not only for those interested in Aristotle's biology, but also for contemporary scholars who seek to take on board Aristotelian insights to varying degrees. For Aristotle, teleological explanations for organisms' characteristics are primary, and it is only through teleological explanations that material and mechanistic developmental explanations are intelligible. This view differs starkly from contemporary mechanism, according to which function-independent specifications of processes are all that is needed for biological explanations. To expose best the interesting features of Aristotle's account, I approach the discussion through considering a problem for Aristotle: He says repeatedly that organisms' formal natures direct development for the good of the organism. This is crucial for Aristotle's account because such optimization principles are explanatory bedrock for demonstrations in his biology. However, as Aristotle mentions, sometimes organisms develop features that either make no contribution to their good or detract from it. This tension threatens to eviscerate the entire teleological explanatory scheme. I will argue that for Aristotle, organisms' formal natures are per se causes of beneficial parts and accidental causes of unbeneficial parts. As long as Aristotle's optimization principles are understood (plausibly) as referring only to what formal natures cause per se, the problem posed by unbeneficial features for the explanatory status of the principles is solved. This discussion will perhaps give some indication of what the theoretical consequences would be of adopting Aristotle's view of the priority of teleological explanation over mechanistic explanation in biology.

How Are Models and Explanations Related?

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Recently there has been an increasing interest in the use of idealized models and the activity of modeling (Batterman 2002, 2009; Bokulich 2011; Craver 2006; Godfrey--Smith, 2006; Rice, 2012; Weisberg 2007a, 2007b). Within the modeling literature there is often an implicit assumption about the relationship between a given model and a scientific explanation. Unfortunately, an adequate analysis of these relationships has yet to be provided. In this paper, we distinguish two fundamental kinds of relationships between models and explanations. The first is metaphysical, where the model is identified as an explanation or as a partial explanation. The second is epistemological, where the relationship between the model and the explanation

is of great importance to the modeler's discovery of an explanation. Our analysis reveals that there are several importantly different ways that a model might instantiate these relationships. For example, we identify three ways that a model might be an explanation and two ways that a model can be a partial explanation. Furthermore, our analysis shows how models that involve idealizations can still be explanations and partial explanations--even when those idealizations are essential to the model. In addition, we show how models can be epistemologically related to explanations when they are used to discover the appropriate kind of explanation, or justify important background beliefs. Understanding these various relationships is key to analyzing the roles models play in scientific theorizing. Moreover, our analysis casts light on the nature of idealization and its role in scientific modeling.

Is it a Human Right to not be Contaminated by Radiation or Threatened by Nuclear War? Why Linus Pauling Thought So

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In the 1950s and 60s, during the fallout controversy, scientists and citizens destabilized the belief that Atomic Energy Commission authorities could objectively determine the safety of fallout from atmospheric nuclear weapons tests. A mass public education effort against testing was led by many scientists, including chemist Linus Pauling.

Pauling and his wife, Ava Helen, raised their opposition as a matter of both science and human rights. As lead plaintiff of the little known "fallout suits" from 1958 to 1964, Pauling sued government agency representatives personally, such as Willard Libby, chair of the Atomic Energy Commission, in an effort to stop nuclear weapons tests. While unsuccessful, the suits link what are seen by historians as isolated antinuclear movements. The Paulings' legal efforts also connect to the emerging human rights legal framework, placing the politics of cold war science in a more holistic context.

More than Mathematics: Wallis, Newton and the Limits of Reason

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Historians have long known that Isaac Newton took inspiration from the mathematical work of John Wallis, the Oxford Professor of Geometry and founding member of the Royal Society who was twenty-six years his senior. Few have considered whether the connection between them runs deeper. Like Newton, Wallis was a polymath who wrote extensively on natural philosophy and theology in addition to mathematics, but it seems unlikely at first blush that Wallis's work in these fields was relevant to Newton. Firstly, Newton's Principia mathematica rendered many of Wallis's contributions to physics unnecessary. Secondly, the two could scarcely have been more distant on doctrinal matters; Wallis ardently defended the doctrine of the Trinity and Newton denied it. Nevertheless, there are indications that they applied similar principles to the interpretation of both the Book of Nature and the Book of Scripture. Wallis had a hand in developing the empirical tradition in seventeenth-century English natural philosophy from which Newton's work emerged, and both thinkers also found empirical techniques useful for biblical hermeneutics. Both Wallis and Newton grounded this empirical approach in considerations of the limits of human reason, which, they believed, reflect the relationship between finite human minds and an infinite Deity. Drawing attention to ideas that Newton may have picked up directly from Wallis, as well as those that they shared as members of a wider intellectual culture, this paper will identify epistemological and methodological principles that could transcend major doctrinal differences among English thinkers in the seventeenth century.

Locke, Providence, and the Limits of Mechanism:

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There has been an ongoing debate amongst early modern scholars about the limits of mechanistic explanation in Locke's natural philosophy. The basic problem is that it seems that a pure mechanist would argue that all the powers of bodies are fully explicable in terms of material structures and impulse, much like the functions of a clock are explicable in terms of the structure and movement of its mechanical parts. And while generally sympathetic to mechanistic philosophy, Locke seems to waver from a full-fledged commitment to mechanism by holding that God arbitrarily annexes secondary qualities to bodies, that it is within the divine power to superadd thought to matter, and that we must reason about gravity simply as a determination of God's positive will. My argument is that the concept of a covenant, whereby God freely chooses to behind himself to a particular natural order, lurks beneath the surface of Locke's metaphysics, and that there are indications of this in both his published and unpublished writings. In this picture, we can reliably gain experimental knowledge of the world not by virtue of any intrinsic necessity in natural phenomena but by virtue of God's providential maintenance of the natural world. And this picture, so I argue, can help to explain the limits of mechanistic explanation in Locke's natural philosophy.

A Structuralist Account of Complex Biological Systems in Ecology

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In a recent paper, Shifting to Structures in Physics and Biology: A Prophylactic for Promiscuous Realism, Steven French outlines the various difficulties of applying structural realism to biology. He maintains that unlike physics, mathematical equations and laws are rare in biology, thus a structuralist interpretation of biology faces the difficulty of representing the relevant structures without reference to such mathematical equations (French, 2010). In the absence of such features, French suggests that models can play the same role in biology as equations and laws do in physics, i.e. they can be used to characterize the relevant structures. This paper shows that biological models do indeed provide the means of characterizing structure. To show this, I utilize the Lotka-Volterra model that is used to describe predator-prey interactions in ecology (Sinclair et al 2006); this particular model utilizes an equation to describe the relations that obtain between the populations of organism that are of interest, thus it allows for a rather straight forward representation of the structure that obtains within the relevant systems. In order to represent this structure, I employ the set-theoretic approach (da Coste and French 2003) to characterize the relations that obtain. I then move to examine two case studies in ecology and show that these relations do indeed represent the real world ecological systems. Based on this discussion I show that the worry that ontological structures of real world systems may be quite different from the structure described by the model is unwarranted and that structural realism can indeed be successfully applied to biological systems.

The Banksian Empire in British North America

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European expansion into the Pacific was a dual-pronged endeavor with military and economic elements often intertwined;. for example, the Royal Navy was employed to settle the dispute between Britain and Spain over Nootka Sound along the Northwest Coast in the 1790s, but the core of the dispute was trade relations with the local indigenous people. Maritime expeditions, whether mercantile or military in nature, often incorporated a scientific component. European imperial expansion thus provided a means for scientists to gather information about nature in places that were hitherto inaccessible. It was not always self-evident, however, to the decision makers of the state (or investors) that resources should be expended in pursuit of goals tangential to a particular expedition's . Hence, someone with influence and credibility was necessary to convince institutions like the Royal Navy that scientific work was important enough to be included. The naturalist Joseph Banks best filled that role in the 18th and early 19th centuries. As a number of historians have shown, Banks used his connections to the British state to place scientists on voyages all over the globe, effectively placing Banks at the center of a network of collectors, naturalists, and correspondents who funneled scientific information to him, enhancing his position as a scientific administrator and advisor. Furthermore, Banks was a supporter of using science to further the aims of the British state, particularly those aims that Banks saw as economically beneficial to his social class. This paper examines the confluence of political and economic aims that underlay the extension of this Banksian "empire" to the western portion of British North America that made the Banksian empire manifest there, particularly with respect to the operations of the Hudson's Bay Company.

The Successful Tipton Works of Mr. Keir: Networks of Conversants, Chemicals, Canals and Coalmines

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The development and growth of James Keir's Chemical Works at the close of the eighteenth century can be attributed to its unique position within a network of intense scientific communication, practical chemical materials and rich geographic resources.

Keir's chemical industry was first and foremost enmeshed in an extensive intellectual and scientific grid consisting of the Lunar Society of Birmingham and its peripheral members. Fruitful correspondence and frequent meetings wove together the skills of savants and fabricants, fomenting scientific, industrial and legal dialogues that definitively shaped the birth and growth of Keir's alkali and soap making pursuits at the Tipton Chemical Works.

Additionally layered upon this savant-fabricant network was a material web of chemical reagents and products. The development of Keir's synthetic soda process rendered useful the industrial waste from nearby factories. The resulting soda of commerce was then employed in saponification and glass making, signifying that Keir's manufactory was just one point on a larger interrelated web of chemical industry in the West Midlands.

Finally, the physical location of Keir's Chemical Works epitomized the necessity of carving out prime territory in a burgeoning industrial landscape. His chemical manufactory was geographically situated at the heart of an ideal network of expanding transport canals and rich coal seams. This micro history of Keir's chemical business will expose the necessary overlap of human, material and geographic networks that stimulated eighteenth century industry in the West Midlands. While it is meant to be a slice within the greater historical landscape of this era, Keir's extensive networks will invite a teasing out of wider social, scientific and economic themes.

What Does Feminist Epistemology Look Like?

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The theoretical underpinnings and motivations for feminist epistemology have been well-developed by Donna Haraway and Sandra Harding, among others, but there are still practical questions that we can raise, and potential practical issues that can arise for the view. One important question is also a fairly basic one: What would it mean to do science from a feminist standpoint, or how would this change our current scientific practices? The project is to "start from women's lives", but what do we do from there? A related, and equally important question, is just who has access to the standpoint. In addressing these questions, we can get a

much clearer picture of what a feminist science would look like and, crucially, deal with the serious objections of those like Janet Radcliffe Richards who argue that there can be no such thing as a uniquely feminist epistemology. The question for this paper, then, is not, "Why should there be a feminist epistemology?", nor is it, "Is there a uniquely female/feminist perspective?", but rather, "How would we do feminist science?"

This paper considers this question and examines what it would mean to do science from a feminist standpoint, and how this would change the ways we understand the role of values in scientific inquiry. I argue that taking on the feminist standpoint leads us to a more honest and correct understanding of how science is actually done.

Science and Industry in the Classroom: The Scientific Manpower Problem between Korea and Sputnik

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The relationship between science and the state has been a pre-occupation of historians of American science during the Cold War for at least two decades. Historians have worried that the course of the physical sciences was distorted by military patronage and that American scientists refused to recognize, and thus correct, their role in the emerging National Security State. In at least one area, however, American scientists were forthright about their relationship to the state and to the public good: in the discussions of scientific manpower that gained momentum during the Korean War and then exploded after Sputnik, provoking the National Defense Education Act. Historians such as David Kaiser and John L. Rudolph have shown how concerns for scientific manpower structured elite physics departments and led to widespread attempts to reform high school science education. Less well studied is the period prior to Sputnik. Drawing on records from the Manufacturing Chemists Association and the American Physical Society's Division on Manpower and Education I show that scientists' explicitly addressed their obligations to provide more scientists for the Cold War and public benefit with a surprising interlocutor: industry. Indeed, prior to Federal action, research based corporations intervened in high school education in a variety of ways; sponsoring and distributing enrichment material, for instance, or organizing role reversal days, where science faculty got to spend the day in a cutting edge research lab and research scientists spent the day teaching high school science. Recognizing the activity of industry in the ubiquitous discussions of scientific manpower in the period will help us to complicate scientists' understanding of their own relationship to the public good and to patronage.

William Huggins, Evolutionary Naturalism and the Nature of the Nebulae

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William Huggins is now generally regarded as one of the great pioneers of the new science of astrophysics that emerged in the middle of the nineteenth century. By applying the spectroscope to the study of stars and nebulae, Huggins transformed himself within a few years in the 1860s from a scientific nobody into a leading figure, and his 1864 paper on the analysis of light from nebulae propelled him to the forefront of one of the most highly charged debates in nineteenth century British science, a debate that engaged moral, political, and religious issues as well as scientific ones. Although he was later to become an evolutionary naturalist, in 1864 Huggins rejected evolution and his opinions on the nature of stars and nebulae were strongly shaped by natural theological arguments to do with unity of plan and unity of operation as well as his views on the existence of extraterrestrial life. An enthusiast in his later years for the nebular hypothesis in which nebulae transform themselves into stars and planets, in 1864 he believed that the nebulae were a separate order of creation. Ironically, Huggins's 1864 paper became a key resource for advocates of the nebular hypothesis. Both Herbert Spencer and T.H. Huxley termed the nebular hypothesis the theory of evolution and John Tyndall contended that those who held the nebular hypothesis would probably agree that "all our philosophy, all our science and all our art...are potential in the fires of the sun."

Science and Religion in Newton's General Scholium to the Principia

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Isaac Newton's General Scholium to the Principia not only engages with both science and religion, but speaks of associations between science and religion. On the tercentenary of its publication, this paper examines science and religion themes in the General Scholium and offers suggestions as to how they relate to and offer insight on Newton's broader thought. The theological portion of the General Scholium begins with a statement of the design argument and concludes with an affirmation that discoursing about God is appropriate within natural philosophy. Why did Newton add a discussion about science and religion to the conclusion of the second edition of the Principia? How much does this discussion build on natural theology in his thought before 1713? How is the natural theology in the General Scholium clarified by his other published articulations of natural theology in the Queries to the Opticks? To what extent does Newton recognise disciplinary distinctions between divinity and natural philosophy? Given any disciplinary distinctions, in what ways might religion have informed his natural philosophy and in what ways did Newton believe his astronomical physics revealed the "hand of God" in Creation? Is the natural theology in the General Scholium in any way reactive to contemporary debates? Just how is Newton's natural theology informed by ancient Greek philosophy, Medieval Scholasticism and contemporary "physico-theology"? Although some of the evidence

is elusive and potentially ambiguous, this paper will assess these questions and suggest some answers. This assessment will include an examination of Newton's 1692-93 correspondence with Richard Bentley on the theological aims of the Principia, Newton's private theological papers, the natural theology in Roger Cotes' preface to the second edition of the Principia and the use of Newton's natural theology by eighteenth-century Newtonians.

Modus Darwin Redux

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Recently, Elliott Sober has examined a kind of inference – similarity, therefore common ancestry – that he dubs "Modus Darwin," due to the frequency with which Darwin employs it. The Galapagos finches are similar; therefore, they share a common ancestor. Sober explicates and defends a set of probabilistic conditions (based on Reichenbach's principle of the common cause) that are collectively sufficient for an observed similarity to favour the common-ancestry (CA) hypothesis over the separate-ancestry (SA) hypothesis.

However, one of the conditions that Sober specifies – that the two ancestors postulated by the SA hypothesis must have character traits that are probabilistically independent of one another – is problematic. I argue for two related points in my paper. First, a historical point: I present evidence to show that some of Darwin's most important targets, such as Geoffroy and Cuvier, would not have accepted this condition. A better representation of Darwin's reasoning would be to think of modus Darwin as an inference about both trait matching and biogeography (or fossil evidence). This leads to my second main point: if we understand modus Darwin in my alternative way, we can relax the problematic assumption about probabilistic independence to allow for some correlation.

I then prove that this new condition, combined with Sober's eight other conditions, are still collectively sufficient for an observed similarity to favour common ancestry over separate ancestry. This new set of conditions provides a more accurate picture of modus Darwin, in both its historical and contemporary guises.

Square holes and round pegs: why Cassirer's structuralism isn't realism

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There is a trend in the structural realist camp these days that seeks to establish a history for their view. Due to these efforts, thinkers such as Duhem, Poincaré, Bertrand Rusell, Moritz Schlick and Ernst Cassirer have been co-opted to the structural realist cause as ostensible early proponents or predecessors. The last of these thinkers, Cassirer, seems to have been first co-

opted to the cause by Barry Gower in his (2000) article "Cassirer, Schlick and 'Structural' Realism," and this paper has been repeatedly cited since its publication by the major thinkers of the structural realist movement (e.g.: French, Ladyman, etc). In my presentation, I intend to show that Cassirer can only be considered a structural realist given a superficial reading of his work. While he does emphasize the ontological importance of structure in science, his notion of structure is remarkably different from that of the structural realists. Cassirer is just not a realist, of any stripe, and this comes out clearly when one examines more closely his notions of representation, objectivity, and truth. To take Cassirer seriously is not side with the structural realists: it is to fundamentally undercut the realist–antirealist debate in which the structural realists have stake.

This paper is part of a larger ongoing project to show how Cassirer presents an alternative to the scientific realism–antirealism divide. It may be of particular interest to the CSHPS crowd because of how centrally the history of science figures in Cassirer's philosophy, and how central a role scientific history plays in the ability of structural realism to avoid the pessimistic meta-induction.

Reviving Thomas Beddoes

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In the late 18th century, Dr. Thomas Beddoes was synonymous with the criticism of entrenched elites, both political and medical. Regarded by many as a dangerous incendiary, in the age of Joseph Priestley and Tom Paine, Beddoes sought to manufacture hope out of thin air. His notions of the medical promise of pneumatic chemistry were widely derided. Despite the early enthusiastic support of many, including the anti-democrat James Watt, Beddoes' hopes came to naught. This paper intends to reveal the breadth of Beddoes' network and explore the range of those who provided apparent evidence of the success of pneumatic medicine. Even when the promise had evaporated, Beddoes continued to receive much praise for his desire to use the new chemistry against what Tom Paine once called "the catalogue of impossibilities."

Cyborg Environmentalist: the confluences of system, technology, and the environment in the work of Dr John Todd

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The 1960s saw the emergence of modern environmentalism and spread of cybernetics beyond its military beginnings. Although seemly from different worlds, these two developments intersected in the 1970s. In that decade, the work of computer scientist Jay Forrester, anthropologist Gregory Bateson, architect R. Buckminster Fuller, and ecologist Howard Odum spread the "cyborg sciences" to both the counterculture and the environmental movement. Drawing on the work of these pioneers, a Canadian biologist and environmentalist, Dr John Todd, attempted to re-think humanity's relationship with nature and technology.

Convinced that a sustainable future could be created by melding human, environment, and machine into single system Todd employed cybernetics and systems ecology achieve this integration. Seeing flows of energy and the intentionality of technology as fundamental to human society and its relationship with the environment Todd designed and constructed structures on Prince Edward Island with the assistance of the provincial and federal governments. Calling these structures "Arks" Todd argued their combination of solar technology, greenhouse agriculture, and energy efficient housing would fit within the stable bounds of the world's ecosystems and save humanity from environmental catastrophe.

My talk will use Dr Todd's work to highlight the importance of Cold War science to Canadian environmentalism in the 1970s.

How to Attain Reliable Inferences from Unrealistic Models in Climate Science

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This paper will discuss the scientific task of detecting and attributing the causes of global climatic changes. It will draw on philosophical literature in confirmation theory--particularly work on consilience--to investigate case studies of climate science that raise a set of prima facie puzzling issues. Computer simulation models are instrumental in scientific studies about climate change. In a variety of ways, these mathematical models are invariably unrealistic representations of the climate system. Given how unrealistic these models are, how, and to what extent, can scientists draw on them to confirm hypotheses in climate change detection and attribution studies?

Due to the complexity of target systems such as the global climate, scientists often model reality in ways that greatly simplify the systems under study. Whether their simplifications are legitimate depends on their relations to the target of inquiry, and the practice of the science surrounding the model in question. While one can apply a range of methods to evaluate models of complex systems, the question of whether a given model result is the consequence of artifactual contingencies of model-construction, as opposed to its
'skill,' is often a subject of scientific debate. Some representations of reality avoid this problem; i.e., ones that have unambiguous relations to reality. In the case of mathematical models, however, it is sometimes less clear when one can use a model to attain reliable knowledge. In order to overcome this challenge, scientists often treat the same problem with several alternative independent models. Despite unrealistic aspects of their design, if such mod- els are significantly independent and still yield similar results, one can infer some degree of

confirmation. "Hence our truth is the intersection of independent lies" (Levins, 1966, p. 423). In climatology, this kind of approach is exemplified by climate model ensemble studies.

Can we get more reliable information from multiple models than a single model? If so, how does this work? Is there any reason to believe that model agreement or model averaging increases the probability of a given estimate about the climate system? What are the conditions that must be met in order for such agreement to improve the reliability of climatological hypotheses? The paper will also address questions about the extent of model independence in current climate model ensembles, the roles of different metrics of model performance, and the importance of weighting models unequally according to skill and independence. In this regard, the paper will focus particularly on the role of climate model ensembles in detecting and attributing the causes of global warming.

A Criticism of Scientific Relativism of the Kuhnian Variety

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The focus of this paper is to shed light on aspects of Thomas Kuhn's The Structure of Scientific Revolution that, I claim, lead him to commit to scientific relativism, a notion that he is vocal to resist. The culprit is Kuhn's notion of incommensurability, masked in what I consider to be a dominant neo-Kantian commitment to the distinction between the world as it appears, that is, the phenomenal world, and the world-in-itself, that is, the noumenal. A further analysis of the implications of this theory and some possible responses, I claim that two readings of the incommensurability thesis arise: the first reading in which scientists have epistemic access to the world-in-itself; and the second reading in which scientists are not afforded this access. Both readings lead to inconsistencies within Kuhn's account, and, by that measure, I consider there to be no interpretation through which a defender of Kuhnian incommensurability can soundly reject my charge of relativism.

Generative linguistics: Re-viewing the assembly of a human science

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Noam Chomsky is among the most frequently cited scholars alive. In the discipline of linguistics, the influence of his voice is without compare. According to the Web of Science, his most cited article--increasing steadily even now, over half a century since its publication--is his (1959) review of B. F. Skinner's Verbal Behavior. The review is referenced in over one thousand bibliographies. For those attentive to the history of the linguistic sciences in the twentieth century, that information perhaps comes as no surprise. Skinner's book, published the same year as Chomsky's Syntactic Structures (1957), represents for many a crossroads in the field,

whereupon its focus shifted from a behaviorist model of the mind to a mentalist one. The popular narrative has it that Chomsky's review was the final nail in the behaviorist coffin, and indeed the review's continued salience, long after behavioral linguistics has ceased to be practiced, affirms its status as an ontological manifesto. My paper resists that narrative. Rather than taking for granted the review's singularity, it explores the burgeoning sociality that developed around (and through) Chomsky's writings, becoming what is known today as generative linguistics. It asks: How was Chomsky's review immediately received? Was there more resistance than commonly assumed? Where did the journal that published him circulate? Where did his arguments find uptake? By following the review along its circuit of publication and reception, my paper better situates how a scholarly community generated around it.

Two Approaches to the Integration of Feminism with Evolutionary Theory

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Since the early 1990s, there have been discussions among evolutionary theorists about integrating feminism with their research. Joining this conversation, this paper will address the nature and potential for success of these discussions. Here I provide what I take to be the two dominant approaches in evolutionary theory which try to incorporate feminism into its disciplines: the collaborationist approach and the evolutionist feminist approach. The collaborationist approach proposes that the best way to tackle shared issues across feminism and evolutionary theory (e.g., the nature of gender, social roles, social hierarchies, rape, sexual behaviour, aggression, etc.) is to overcome their epistemological differences and work alongside one another. The evolutionist feminist approach, on the other hand, reflects an indirect engagement with feminism. They have defined feminism in their own terms and have incorporated feminist knowledge in evolutionary theory by offering to be the source of this knowledge themselves. In my paper I side with the evolutionist feminist approach and reject the collaborationist. Some of the evolutionists' criteria for collaboration, I argue, are too demanding since they require some feminists to suspend core epistemological values. Moreover, I argue that the benefits expected to be necessitated by feminist collaboration can be attained just as easily through a collaboration with evolutionist feminists.

Reexamining the Problem of Demarcation

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The demarcation problem aims to articulate the boundary between science and pseudoscience. Solutions to the problem have been notably raised by the logical positivists (verificationism), Karl Popper (falsificationism), and Imre Lakatos (methodology of research programmes). Due, largely, to the conclusions drawn by Larry Laudan, in a pivotal 1981 paper which dismissed the

problem of demarcation as a "pseudo-problem", the issue was brushed aside for years. Recently, however, there has been a revival of attempts to reexamine the demarcation problem and synthesize new solutions. My aim is to survey three of the contemporary attempts and to assess these approaches over and against the broader historical trajectory of the demarcation problem. These are the efforts of Robert Pennock (methodological naturalism), Nicholas Maxwell (aim-oriented empiricism), and Paul Hoyningen-Huene (systematicity). I suggest that the main virtue of the new attempts is that they promote a self-reflexive character within the sciences. A modern demarcation criterion should be sensitive towards the dynamic character of the sciences. I argue that there are both good theoretical and good pragmatic grounds to support further investigation into a demarcation criterion and that Laudan's dismissal of the problem was premature.

Radiation in Biology and Medicine: Before and After the Atomic Bomb

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Although we commonly associate the atomic age with nuclear weapons, atomic science had many potential uses. Following World War II, nuclear weapons development continued under the newly created U.S. Atomic Energy Commission (AEC) and became one of the defining features of the Cold War. While the AEC was responsible for managing a growing nuclear weapons complex, the agency also had a mandate to develop atomic energy for civilian purposes. To this end, the AEC established an extensive program in biology and medicine and nurtured hopes that radiation might revolutionize both biomedical research and medical practice. The AEC's biomedical agenda was greatly influenced by and, in fact, helped create a Cold War culture in which national security threats were tempered by scientific, technological, and medical advances. However, as this paper argues, it was also very much shaped by the objectives of researchers who, prior to World War II, were already developing biomedical programs focused on the study and use of radiation. Using the University of Rochester as a case study, I argue that a partnership between the AEC and the University allowed for the institutionalization of recent advances in biomedical radiation research within a new Department of Radiation Biology. By examining the continuity in the University of Rochester's biomedical initiatives this paper illustrates that, in a political economy of science in which the AEC possessed considerable authority, Rochester's biomedical scientists successfully competed for a share of that authority and drew the AEC into an ongoing process of discipline-building.

SPECIAL SESSION

Session in Honour of Eric L. Mills: From Biological Oceanography to The Fluid Envelope of our Planet and Beyond

Session Organiser: Hannah Gay

This session has been organized with the dual intention of discussing some of the book's major themes and honouring its author, Eric Mills. Mills, an emeritus professor of oceanography at Dalhousie University, is a distinguished and long-time member of CSHPS. He was our Stillman Drake lecturer in 1999, and has made many scientific contributions to oceanography, as well as being one of its major historians. An earlier book, Biological Oceanography: An early history, 1870-1960 (Cornell University Press, 1989) is, as the title suggests, about developments in the biology of the oceans. Among other things, it covers early developments in marine ecology. The book was reissued as a paperback by the University of Toronto Press in 2012. One of the papers in this session looks back to this book, while the other three focus on the more recent The Fluid Envelope of our Planet: How the Study of Ocean Currents became a Science – a book oriented more toward physical oceanography. In it Mills begins by discussing some pre-nineteenth-century ways of knowing about the oceans and their currents, and shows how such knowledge developed through the nineteenth and twentieth centuries. He discusses why oceanography grew in importance during WW2 with one consequence being that, after the war, oceanography departments opened in many universities. The book covers the theoretical ideas and working practices of some important oceanographers, as well as the pedagogy with which they were associated. As was the case with other sciences, mathematics entered oceanography in a serious way during the twentieth century. Some of its practitioners sought mathematical models for the dynamical behaviour of the oceans – no easy matter for something so complex. In his book Mills discusses work carried out in many centres, including ones in Canada, Scandinavia, Germany, France, Monaco and the United States.

In this session four people will read short papers that relate to Mills' books. Mills will then respond.

Did French Oceanography Fail?

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In his 2009 book The Fluid Envelope of Our Planet, Eric Mills devotes a chapter to the development of French Oceanography. In it, he points to, what he terms, "the paradox of French marine science." The question he poses is this: why, at a time in which physical oceanography was the fastest growing branch of marine science elsewhere, did dynamic oceanography fail to take off in France? Mills concludes that the failure of physical oceanography in France may be attributable to "contingency", "personal eccentricities", and "narrow nationalism." Mills does not claim to provide a definitive answer to the question he raises, yet his important observation that physical oceanography did fail leads us to ask other questions about the development of the marine sciences in France. How does our understanding of the failure of physical oceanography shape our understanding of the fact that, with regard to marine biology and the development of marine stations, France was at the forefront for much of the nineteenth century? My paper will suggest an alternative question: why did marine biology succeed in France while physical oceanography failed? To answer this question I will examine efforts in France during the early nineteenth century to centralize scientific instruction and data collection. I will examine the importance of public support for the development of French marine science, and the role public exhibitions played in garnering that support. Finally, I will examine the development and function of French marine stations as sites for both experimental biology and scientific instruction.

The Creation of the Biological Boundaries of the Seas

Keith R. Benson
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A major contribution of Eric Mills's scholarship has been to the early development and subsequent elaboration of biological oceanography as a major subdivision of oceanographic investigations. Implicit in his work has been the foundation of biological oceanography from the generalized natural history or biology of the sea from the nineteenth century. This same foundation gave rise to marine biology; in essence, biological investigations of the sea became bifurcated into those focusing on open ocean studies from those of the littoral fringe of the sea. True to Mills's emphasis on the importance of contingency in history, the bifurcation actually obscures the shared foundation that biological oceanography and marine biology have, especially in terms of ecological ideas applied to these marine environments. Continuing the case for contingency, this paper will illustrate the parallel ecological developments in marine biology that mirror these same developments that Mills's scholarship has carefully demonstrated. Specifically, it will examine three case studies, the marine programs at Scripps, Hopkins, and the University of Washington, all of which combined traditions in oceanography and marine biology.

Lost at Sea: German Oceanography in the Period 1900-1925

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Eric Mills's The Fluid Envelope of Our Planet (2009) devotes a detailed chapter to the late development of dynamical oceanography in Berlin, and in Germany more generally. In addition to the sound scientific reasons Mills gives for this late development, there are some interesting, contingent, and highly idiosyncratic reasons as well. Foremost among these are three. First, the departure of Wilhelm Bjerknes and his family from Leipzig back to Norway in 1915 removed the principal advocate of hydrodynamic modeling in Germany. Second, the theoretical work done at the German Marine Observatory in Hamburg from the 1890s into the 1920s was overwhelmingly concerned with marine meteorology, and latitude-based climatology rather than dynamic oceanography. This was the case when Wladimir Köppen was chief scientist there, and even more when his son-in-law, Alfred Wegener, succeeded him. Third, Germany was blockaded and embargoed physically after 1915, had no merchant or research vessels at sea, and did not resume international scientific cooperation until the later 1920s; the Meteor Expedition (1925) was the first scientific expedition to leave Germany since 1914. Finally, Germany's repeated failure to mount successful polar expeditions, combined with a pattern of marine research emphasizing the aerological study of the prevailing winds at different latitudes, led to a preference for expeditions following East-West rather than North-South tracks, at least until the departure of the Meteor.

Slipping Back to Norway: Terrestrial Physics and Polar Currents

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This paper draws upon two of the most significant themes in Mills' Fluid Envelope of Our Planet – certainly for my own work and thinking and, I would argue, generally for the history of modern science. These are, first, the development of a sense of institutional context as a part of historical practice (transcending typical institutional histories) and, second, the application of this insight to the history of our understandings of terrestrial sciences as a matter of the large-scale, complex physics of the earth. Mills' account draws on these insights to provide a North American story, taking up parallel histories in Canada and the United States, with the latter case depending on mathematical practices that were imported in a process of "slipping away from Norway." Here, I will draw upon Mills' exemplary work, extending it by "slipping back to Norway" during the interwar and the immediate postwar periods, and to the problem of Arctic and Antarctic currents as it was pursued by Scandinavian scientists. I will argue that, despite the international if not transnational character of ocean science, Mills' essentially nationalist approach is successful for good reasons.

SPECIAL SESSION

Joint Session of the Canadian Society for the History of Science and Technology and the Canadian Society for the History of Medicine

Experimenting with «Fluid Objects» in Late Nineteenth- and Early Twentieth-Century Laboratory Physiology

Session Organizer: Frank W. Stahnisch, University of Calgary

Chair and Commentary: Delia Gavrus, McGill University

This explores the "fluid nature" of scientific objects and experimental practices in late nineteenth and early twentieth century research laboratories that emerged in institutes of physiology and pathology--as well as in clinical medical departments. "Fluid objects," to use a notion from Hans-Joerg Rheinberger, form the interface between scientific representations and research-oriented interventions; they may be visible entities or invisible assumptions, models or instruments, among other things. Notions such as "humoural fluids," "microscopic traces"or "point values", introduced into the experimental physiological laboratories during the nineteenth century, were among the working units and test objects of the experimental systems in physiological, pathological and clinical laboratories. They constituted both the research trends and the constraints of experimental practice in medicine and biology. This panel addresses the history of research on fluid objects and scientific notions from multiple disciplinary and interdisciplinary perspectives.

* *This session has received financial support from Canadian Federation for the Humanities and Social Sciences Aid for Interdisciplinary Sessions*

Fluid Objects and Unruly Things: Experimenting with Living Animals and Humans in Nineteenth-Century Nutrition Physiology

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This paper explores the problematic and fluid nature of experimental objects in nutrition physiology. In the late nineteenth and early twentieth centuries, physiology struggled with the place and role of the living objects of its experiments. Trying to define itself as an exact science, it looked to chemistry and physics for its experimental models, yet the objects of these sciences differed strongly from the living subjects of physiology experiments. Humans and animals were experimental objects that could not be stabilised, controlled or manipulated to the same degree as non-living objects, and they were subjects, frequently exhibiting behaviour and characteristics that resisted the constraints of disciplined, precise experimentation. The living, agential status of these objects was particularly problematic in nutrition and metabolism experiments. Eating is a voluntary activity and metabolism is affected by the emotions, so

experimenters were very aware of the need to avoid force, coercion and anxiety or discomfort-producing conditions. Nutrition and metabolism studies thus required both the physical and the psychological cooperation of these objects, who became active participants in the experimental process. These unruly things "kicked back" at the experimental conditions, demanding explicitly or through behaviour that experiments be modified and adapted to fit their preferences and needs. Bodies resisted the requirements for precise experimentation, exhibiting physiological phenomena that forced experimental accommodation. Boundaries between experimenter and experimental subject became even more fluid in human experiments. Physiologists experimented on themselves, on colleagues, technicians, laboratory personnel and medical students, while subjects were informed about the goals and aims of the experiment, frequently contributed subjective protocols and were often responsible for taking measurements and ensuring the accuracy of various parts of the experiment. No passive objects, they became active participants, collaborators and co-experimenters.

Fixing Fluids, Fixing Practices: Clinical Cancer Research in early Twentieth Century France

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Pierre Masson, in his 1923 publication Les tumeurs, consecrated multiple pages to describe the fixing fluids used for tumours and growths. A full scene of Jean Benoit-Lévy's 1933 film La biopsie is dedicated to the preparation of a fixing fluid to submerge biopsy samples in. The outer margins of the pages of the laboratory records of the Institut d'Anatomie pathologique at Strasbourg's medical school also record the fixing fluids in which the biopsy or surgery samples were sent. Fixing fluids froze cancerous processes in time. This snapshot became the elemental definition of cancer, allowing it to be understood a process like a stop motion animation.

With cellular theory, Rudolf Virchow and Julius Cohnheim defined cancer as specific changes in tissues in the mid-nineteenth century. Cancers herein became objects of histology and histopathology research. Virchow emphasized the principles of biopsy and its value in the diagnosis of malignant tumours, but he himself did not promote it for diagnosis of patients. However, many pathologists previously studying post-mortem cancers embraced histology practices. Histology reposed on fixing cells, freezing their structure in time, before sectioning and staining them. In this paper, I will outline these practices, described by Masson (and other pathologists) in textbooks and laboratory handbooks, who particularly elaborated them in the context of clinical cancer research with researchers of radiation therapies. The diagnostic information was obtained through examination and analysis of fixed tissue samples. By the mid-twentieth century, pathologists were mediators between alternative therapeutic solutions. The histology information that defined cancers, as I will argue, was a codified form of scientific laboratory knowledge issue of practices fluid between botany, zoology, minerology research and clinical research, but also fluid between anatomical pathology and histo-pathology.

Physiological Aesthetics: Experimentalizing Life and Art in Fin-de-Siècle Europe
Bob Brain
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This paper examines how a family of experimental systems developed in nineteenth-century physiology provided avant-garde painters and poets with key material and conceptual resources that enabled the innovations of early modernism in the arts. I argue that the borrowings moved in two directions. First, I show how artists adopted the materialities of physiology -- instruments and techniques-- as a means to undertake new kinds of aesthetic experiments within the specific media of each cultural art. Second, I show how the converse occurred: early modernist experiments in poetry, the visual arts, dance, and music functioned as experiments on life, aiming to alter the human sensorium and to reconfigure both the artist and spectator. In order to make this argument I show how an array of people, concepts and practices that have not traditionally been discussed together belonged to common networks. I also introduce several new areas of nineteenth-century scientific culture that have not been discussed by historians, including the widely held protoplasm theory of life, the epistemology and social doctrines rooted on the physiology and psychology of movement, and more. I support my arguments with readings of works of painting and poetry that reveal the implementation of physiological aesthetics, including Edvard Munch's The Scream, George Seurat's late entertainment paintings, Francis Picabia's prewar cubist paintings, the free verse of Gustave Kahn, and the vocal performances of F. T. Marinetti and the Futurists. With fresh interpretations of canonical works I aim to challenge entrenched assumptions about the art/science "two-cultures" divide and invigorate dialogue between historians of science and specialists in the history of art, literature, and music.

Ousting Researchers and Transferring Things: On the Conditions of Neurophysiological Research in German-Speaking Refugee Neuroscientists in North-America, 1933 to 1963
Frank W. Stahnisch
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Until recently, the process of forced-migration of German-speaking physicians and medical researchers scholars has frequently portrayed by the "brain gain" theory of academics, intellectuals, and scientists, when most notably the United States (in North America) and Great Britain (in Europe) became "enriched" through receiving the émigré neuroscientists, and German-speaking science underwent the loss. With a view to the cultural and practical conditions in neurophysiology, the perspective presented in this paper challenges this received historical view by drawing attention to the often neglected immigration rules, social relations, and contingent patterns of re-adaptation into scientific working groups. By focusing on the travelling ideas and instruments – themselves being fluid objects of the international forced-migration process – some new light shall be shed on the difficult re-integration of the German-speaking refugees in North-American neurophysiology. When taking microscopes, brain slides or staining technologies as essential utensils of modern neuroscientific research, the contingent scientific luggage of German-speaking émigré-neurophysiologists and neuropathologists shall historically be unpacked and scrutinized as to its role and influence in the process of re-integration of the exiled neuroscientists in North America. As a result of their holistic research and clinical approaches to neuroscientific laboratory research in the early 1930s and 1940s, important re-adaptations and modifications in neurophysiological research styles emerged, despite the fact that many of the fleeing émigrés arrived in the United States and Canada with not much more than suitcases filled with a few research instruments, specimens or histological slides as well as the addresses of relatives, friends or international colleagues in their pockets.

SPECIAL SESSION

Discussion Roundtable

Surveying the History of Science: Texts and Courses in the Modern Curriculum

Session Organiser: Gordon McOuat, University of King's College

Spurred by the latest contribution in this field, Lesley Cormack and Andrew Ede's A History of Science in Society: From Philosophy to Utility (Toronto: University of Toronto Press, 2012), and challenges and changes in the modern curriculum, this panel/symposium will examine the nature of survey courses and texts in the history of science: their place in the curriculum, strategies and tribulations in their construction, and criticisms of present offerings.

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