Anjan Chakravartty
SCIENCE, METAPHYSICS, AND THE PHILOSOPHY OF SCIENCE
June 4 14:00 – 14:30
MacLeod 214

Subsequent to the transition from the era of natural philosophy to what we now recognize as the era of the modern sciences, the latter have often been described as independent of the major philosophical preoccupations that previously informed theorizing about the natural world. The extent to which this is a naïve description is a matter of debate, and in particular, views of the relationship between the modern sciences and metaphysics have varied enormously. Logical positivism spawned a distaste for metaphysics within the philosophy of science which lasts to this day, but in recent years, a renaissance in analytic metaphysics has been embraced by a growing number of philosophers of science. Those moved by distaste commonly subscribe either to a minimalist Humean metaphysic, or to a quietism about metaphysical questions generally, and often maintain that such stances are operative in scientific practice itself. Those moved by attraction contend that metaphysical investigations into the natures of things like properties, causation, laws, and modality, are required in order to interpret descriptions of the world furnished by our best scientific theories, and often suggest that metaphysical commitments with respect to issues such as these likely play a significant role in scientific theorizing itself. In this paper, I will attempt to enumerate the philosophical presuppositions separating these approaches to scientific knowledge, and consider some prospects for their resolution.

Andrea Charise
Location, location, location: Cultural technologies of brain mapping
in Victorian science and modern neuroimaging
June 3, 15:45-16:15
MacLeod 254

In this paper, I discuss the conceptual linkages between recent trends in neuroimaging technologies of "brain mapping" and the enormously popular nineteenth-century pseudoscientific practice of phrenology. Although phrenology (which proposed that the strength of innate psychological faculties could be correlated to physical variations on the surface of the skull) is often dismissed as an embarrassing blunder in the history of neuroscience, scholars including Nicholas Dames and John Van Whye have recently argued that it provided a vital precursor to later nineteenth-century science, particularly the Darwinian model of evolution. By considering this simultaneously social and scientific nineteenth-century discourse alongside contemporary neuroimaging technologies such as magnetic resonance imaging (MRI), I seek to assess the extent to which modern neurological research may be said to inherit nineteenth-century and, more specifically, phrenological assumptions regarding the nature of personality, destiny, and the ethics of psychological treatment.

My central question asks: How has the phrenological theory of cerebral localization re-emerged in modern neurology to answer questions about the nature of the self, as revealed by the technologically-mediated body? I discuss the extent to which phrenological practices influenced nineteenth-century concepts of self by examining George Combe's highly influential *The*

Constitution of Man Considered in Relation to External Objects (1828) and Josef Spurzheim's *Physiognomical System* (1815). Moreover, in what ways do modern neurological impressions of personality, genetic predisposition, and physical manifestations of psychopathology retain –but also fundamentally depart from– those tenets of the "quack" science to which it owes the very notion of cerebral localization? I argue that modern neuroimaging technologies demonstrate a shift in the textual site(s) of brain pathology: namely, from phrenology's external, tactile "reading" of the mind as physical variations of the skull, to the internalized imaging of the brain as the site at which the very structure of self may be interpreted. Finally, I emphasize the significance of this transition from tactile to visual technologies, and investigate its implications for the privileging of certain models of medical evidence and "truth."

Alexandre Korolev

Indeterminism of the Norton-type Lipschitz-Indeterministic Systems as an Artefact of Infinite Idealizations

June 5, 9:00-9:30

MacLeod 254

I argue that the singularity arising from the violation of the Lipschitz condition in the simple Newtonian Lipschitz-indeterministic system recently proposed by John Norton (2003) is so fragile as to be completely destroyed by slightly relaxing certain (infinite) idealizations required by this model. In particular, I show that the idealization of an absolutely nondeformable, or infinitely rigid, dome is an essential assumption for anomalous motion to begin; any slightest finite elastic deformation of the dome due to finite rigidity of the dome irreparably destroys the shape of the dome required for indeterminism to obtain. I further demonstrate that this situation cannot be remedied by making the dome a little "pointier" at the apex, in the hope that the dome assumes just the right shape after it is "squished" down by the weight of the mass placed on top of the dome.

I also exhibit and examine several further situations – the rope-on-the-edge example and the rope-on-the-spherical-dome example – which, unlike the original Norton's example and its modifications, have no singularities in surface's curvatures, and show that indeterminism in these cases, too, critically depends on the nature of certain infinite idealizations pertaining to elastic properties of the bodies in these models.

As a result, I argue that indeterminism of these Norton-type Lipschitz-indeterministic systems should rather be viewed as an artefact of certain infinite idealizations essential for the models, depriving the examples of much of their intended metaphysical import, as, for example, in Norton's antifundamentalist programme.

Ben Almassi

Conflicting Expert Testimony and the Search for Gravitational Waves

June 4, 15:00-15:30

MacLeod 202

How can non-experts make informed decisions about whom to trust given conflicting expert testimony? A speaker's expertise provides *prima facie* justification for believing her testimony, but given seemingly equally authoritative claims to the contrary, what are third parties to do? Alvin Goldman considers this issue of expert disagreement at a general level. Here I weigh Goldman's account against a case from modern physics, a case which itself receives conflicting accounts from sociologist of science Harry Collins and philosopher/physicist Allan Franklin: the early search for gravitational waves, in which field pioneer and eventual outcast Joe Weber came to intractable dispute with the rest of his field over claim to have detected gravity waves. Against Collins and Franklin, I propose that we see the resolution of this dispute as social *and* evidential; although the experimental data alone did not force the resolution, nonetheless there were good *credibility*-based reasons to trust certain experts over others. Furthermore, I argue, this case calls into question Goldman's guidelines for third-party assessment of expert disputes. Specifically, the case reminds us that in the context of modern experimental physics dialectical superiority, track records, and interests are not always good indicators of expert reliability, whereas fellow- and meta-expert agreement prove to be crucial indicators even when Goldman's criterion of conditional expert independence is not satisifed.

Alexandru Manfu
ENANTIOMORPHY, SYMMETRY AND THE REALITY OF SPACE
JUNE 4, 11:45-12:15
MACLEOD 202

Kant's 1768 argument for the reality of space as something that is independent of the existence of all matter developed around the notion of incongruent counterparts (or *enantiomorphs*) – objects similar and equal to each other but which cannot be made to coincide through translations and rotations (e.g., a pair of hands). One of the premises of Kant's argument asserts that that there is a matter of fact about the handedness of a hand in an otherwise empty universe. As some authors have noticed (e.g., Earman, Nerlich), Kant's premise is defeated by the fact that space has features like *orientability* and *dimensionality* (for instance, a right hand may become a left if properly transported in a non-orientable space or if properly rotated in the fourth dimension). Kant's argument has been reconditioned by Nerlich, who interprets Kant as saying not that there is a matter of fact about the *handedness* of a lone hand, but that there is a matter of fact about the *enantiomorphy* of a lone hand. However, Nerlich does not present an explicit formulation of his reconstruction of Kant. I offer such a formulation and then evaluate it. I argue that Nerlich's reconstruction is hardly more plausible than the original – Nerlich's argument does fix a (nowadays) obvious problem with Kant's original argument, but in doing so, it collapses into circularity. This objection apart, I argue that Nerlich's argument is precarious because it rests on a very fragile foundation, namely the contingent fact of there being asymmetric objects.

Andrew Morgan
Constraining the Foils: The Similarity Condition
June 4, 17:00-17:30
MacLeod 254

Peter Lipton's 1991 paper *Contrastive Explanation and Causal Triangulation* illustrates how the explanations specified by contrastive questions are significantly different from their non-contrastive counterparts in scientific explanation. Since asking *'why P rather than Q'* specifies an explanation unique from the explanations of 'P', 'Q', and 'P and not Q' alone, a new and specifically contrastive account of what constitutes good scientific contrastive explanations is required. This paper proposes that Lipton's account, like many contemporary accounts of contrastive explanations, is incomplete insofar as it only specifies the appropriate *difference* between the fact of P and its foil Q as constituting a contrastive explanation. This allows for causally unrelated contrasts to be explained when there ought not to be an explanation. A good account of scientific contrastive explanation requires a further necessary condition: that the contrasts are appropriately *similar* in order to qualify as a candidate for finding the appropriate causal difference. My similarity condition limits what qualifies as a 'contrast class' for any given fact P, thereby eliminating problematic cases where contrastive explanations are given for facts P and foils Q that ought not be explained. This condition is detailed by way of the counterfactual causal history of Q. The facts and foils of contrastive questions must first be shown to be related in the appropriate way before a contrastive explanation can specify an appropriate explanatory difference.

Alan Richardson
Logical Empiricism without Empiricism and without Analytic Philosophy
June 5, 11:45-12:15
MacLeod 202

The standard histories of logical empiricism use one or both of two narrative frames: logical empiricism as an episode in the history of empiricism or logical empiricism as an episode in the history of analytic philosophy. In the first case, the historical narrative presumes we know beforehand what empiricism is. In the second case, the narrative presumes the antecedent availability of a notion of analytic philosophy of which logical empiricism is one variety. Now, one can make either or both of these presumptions but, more remarkably, one can also *not* make them. An historian might take logical empiricists at their word in the 1920s that informal and traditional philosophical words like 'empiricism' have no empirical value and provide nothing in the way of historical understanding. Similarly, an historian might be interested in the fact that the logical empiricists in the 1920s and 1930s did not present their philosophy as an instance of 'analytic philosophy'; the term 'analytic philosophy' became standard only in the late 1940s and early 1950s—how philosophy becomes analytic philosophy is part of the history of logical empiricism (and related projects) and thus not a framework that ought to be presupposed when doing that history. (Assuming, that is, you do not wish to write teleological history.) The talk purposes to sketch a history of logical empiricism in which both 'empiricism' and 'analytic philosophy' are contested terms in that history and not historical frames upon which to hang that history. It does this through a modest bit of historicism by taking seriously that logical empiricism at its founding was presented as, in the first and most important instance, *scientific philosophy*. The talk will attempt to establish what was meant by that term, what science logical empiricism took philosophy to be, and why that term ultimately disappeared from philosophical parlance. Lingering consequences for today's analytic philosophy will be sketched.

Alirio Rosales & John Woods
Fruitful distortion: Idealization, analogy, and scientific understanding
June 4, 16:00-16:30
MacLeod 254

Idealization in science takes a number of forms, three of which are especially important. In one form idealization is abstraction (e.g. geometric models) (e.g. perfect information); in another it is perfection; and in a third it is analogy (e.g. Bohr's model of the atom, rational agents). No matter the form, idealizations are representations of things as they are not. They are distortions of empirical objects, *on purpose*. This gives rise to the quite general question of how the epistemic goals of science are advanced by these distortions. Part of the answer is that conditions exist under which thinking of things as they aren't captures essential features of what they are like; which in turn produces an improved understanding of how they are. This involves the analogical aspect of idealization in an irreducibly central way. Up to now, what is known of the connection between analogy and idealization has been implicit in the contributions of philosophers and methodologists of science. Our aim here is to bring the matter closer to the surface of explicit articulation, and to elaborate further the connection between idealization and scientific understanding.

Burcu Erciyes
Feminist Objectivity Versus Traditional Objectivity
June 4, 12:15-12:45
MacLeod 214

Feminism has long been under attack by philosophers who doubt its theoretical contribution. Specifically, the value of feminism's critique of science has been fiercely questioned for falling into various forms of relativism, which is argued to go against the very idea of a science based on universal laws and a unique method. In this traditional view of science, objectivity is preserved when judgment is free from subjective factors. Influenced by historicism, feminists claim that all knowers are necessarily located in space and time, thus the "view from nowhere" is impossible. Accordingly, science cannot be totally value-neutral. Although they share no consensus on "objectivity", feminists agree that it should be deconstructed and/or reconceptualized. One problem faced by feminists is that if the reconceptualization of "objectivity" leads to a different understanding of the term, then it would be impossible to compare the new sense of the term with the old one and claim that the former provides a better way of understanding the world than the latter. In order for two concepts to be compared and examined in terms of compatibility or incompatibility they should refer to the same thing or at least overlap significantly. Given that feminist and traditional objectivity rest on different assumptions, one *prima facie* reaction is that they are neither compatible nor incompatible, for they do not refer to the same thing. Nevertheless, if feminists want their objections to be taken seriously and avoid being charged with irrelevance, they should show that there is a common ground for comparison. Thus, there will be room to claim that feminism provides a better account of the world than mainstream philosophy of science. In the paper, I argue that there is common ground that enables us to compare these two notions and, hence, a feminist critique is relevant.

Bert Leuridan
Science without experiments. The case of classical genetics
June 3, 15:45-16:15
MacLeod 214

Experimentation is frequently considered a very useful way to discover causal relations. Moreover, experimental data are usually deemed far more reliable in this respect than merely observational data. I will side this view and explore its consequences regarding causal discovery in classical genetics. First I will examine the distinction between experimental designs and merely observational (e.g. prospective) studies as it is made in statistics and the methodology of the special sciences (Kutner et al., 2005) and in philosophy of science (Woodward, 2003; Giere,1997). I will emphasize the role of manipulation in this respect. Then I will examine the empirical basis of early classical genetics. Classical genetics was based on genetic crosses (e.g. crosses of tall pea plants and short pea plants). These were often called 'experiments', as is evident from the works of Mendel, Correns, de Vries, Bateson, Morgan, etc. (Today they are still labeled so, see Orel, 1996). I will show that genetic crosses do not qualify as experiments, but rather as observational designs (they closely resemble prospective designs).
Finally, I will explore the consequences of this finding by means of a case study. Before 1911, T.H. Morgan strongly opposed Mendelian genetics (cf. Morgan, 1909). His arguments were directed against the causal structure of this theory. I will show that they can be understood by taking into account the non-experimental nature of genetic crosses. Only by performing many such crosses, and more importantly, by incorporating knowledge from i.a. cytology, classical genetics eventually developed into a well-established theory.

Robyn Blum
Dynamic Causal Modeling: A Reply to Egan and Matthews
June 5, 9:00-9:30
MacLeod 214

In a recent paper, Egan and Matthews (2006) describe a new analytic technique for neuroimaging studies, dynamic causal modeling (DCM), and suggest that this technique represents a "third way" of doing cognitive neuroscience that offers an alternative to the rival "top down" (psychology-driven) and "bottom up" (neuroscience-driven) approaches to cognitive neuroscience. DCM allows researchers to test various causal models of the interactions between a small number of brain regions during a cognitive task. A closer look at this technique, however, shows that it is ultimately rooted in the top-down approach, as it includes an initial analysis (prior to DCM itself) that requires specification of a psychological model. Having shown that DCM does not really provide a third way, I next consider a plausible alternative third way provided by independent component analysis (ICA). Unlike DCM, ICA does not require a psychological model as part of the analysis. It does, however, require reference to psychological models for the results of the analysis to be interpreted. I conclude that the prospects for finding a third way are grim, but that such an approach is not needed to solve the apparent impasse between top-down and bottom-up approaches.

Boaz Miller
Popper, Models and the Rationality Principle
June 4, 10:45-11:45
MacLeod 202

Philosophy of science has undergone a transition in understanding scientific theories. In the first half of the twentieth century, the syntactic view of theories was dominant. It regards a theory as a set of statements closed under deduction. In contrast, the semantic view of theories, which has emerged in the 1960s, regards theories as families of models, which are abstract objects standing in a relation of similarity to the world.

I show that Popper presents an interesting intermediary view between the syntactic and the semantic approaches. The context of Popper's discussion is his philosophy of social science. Popper extensively discusses models when he proposes his notion of 'the rationality principle' (RP), which is the assumption that individuals act in accordance with the objective situation. Popper ascribes RP a privileged status in social science as the 'animating law' of all models.

I argue that the common statistical interpretation of RP is incorrect, and propose a new interpretation of RP as consisting of an idealization and two abstractions. I show that my interpretation helps to solve alleged problems in reconciling RP with the rest of Popper's philosophy of science. I critically discuss the privileged status Popper ascribes to RP, and drawing on Kahneman and Tversky's Prospect Theory I argue that RP, as I interpret it, plays an important role in social science. However, RP also has inherent limitations denying it the privileged status Popper ascribes to it.

Cecelia Watson
The Artist Versus the Associationists: William James's use of John La Farge's theories of art in *The Principles of Psychology*
June 3, 16:15-16:45
MacLeod 254

Before he enrolled in the Lawrence Scientific School, William James studied art under William Morris Hunt in Newport, RI. James intended to make a career of painting, and he spent many hours painting and discussing art theory with his friend and fellow art student John La Farge. After leaving Hunt's studio, La Farge became a well-known painter, stained-glass artist and art critic, and, although James decided to devote his energy to science, he and La Farge maintained a lifelong friendship. But their relationship was more than a simple social tie for James; it was a source of intellectual inspiration: James declared late in his life that ever since he and La Farge had studied art under Hunt, James had been contemplating La Farge's technique and his theories of art. James also told La Farge that Delacroix's artistic philosophy as La Farge described it mirrored James's own pragmatic philosophy. But despite these biographical and theoretical intersections, little attention has been given to La Farge's substantive influence on James's

psychology and philosophy; the few historians who have seriously considered the idea that James's art training influenced his psychology and philosophy do not fully explore the relationship between James and La Farge and their writings. This paper focuses on a small part of La Farge's influence on James, beginning by explaining what James learned from painting with La Farge in Newport, and then examining James's translation of those lessons into arguments against Associationist psychology.

Fan Chen & Dongming Cao
Japan's Colonial Scientific Research Institution in China:
The Shanghai Science Institute
June 3, 10:00-10:30
MacLeod 214

During the Japanese invasion and occupation of China, more than 100 Japanese colonial research organizations were in place. These were funded by the Japanese government, as part of a repayment of money acquired after the 1900 war. The Shanghai Science Institute (SSI), established in 1931, was a representative Japanese colonial scientific research organization. This paper investigates the process of setting up the SSI, and the roles of Chinese scientists in it. We describe the transformation of the SSI from a pure academic research institute into a tool of Japan's invasion of China. We provide information about the staff status, organizational system and chief research activities of the SSI, and briefly present a few indigenous Chinese scientists and explore the relationship of the SSI with academic and cultural circles then in China.

Corey Mulvihill
Fine Distinctions: Constructive Empiricism, Instrumentalism and Bell's Theorem
June 4, 15:00-15:30
MacLeod 254

Bas van Fraassen and Arthur Fine have both written about the relationship between their general philosophical orientations, constructive empiricism and the natural ontological attitude respectively, and instrumentalism VAN FRAASSEN (2001); FINE (2001). In this debate Fine has argued that van Fraassen's and his views should be properly understood to be instrumentalist.

Fine argues that if van Fraassen towed the 'party line' a bit closer, he would escape the epistemological problems inherent in having phenomena of different degrees of warrant. While van Fraassen admits that his position "could appear as a Corollary" to Dewey's instrumentalism, he insists that there is much about the instrumentalist position with which he would not want to be associated.

I will argue that van Fraassen overstates the distinctions made between his view and instrumentalism. However I will show, drawing from their treatments of Bell's inequality, that, in this case, Fine overstates the differences between his and van Fraassen's view VAN FRAASSEN (1989); FINE (1989).

This paper will not attempt to prove that van Fraassen and Fine have demonstrably equivalent views. However, if van Fraassen is right that his position differs from Fine's and

instrumentalism, and if Fine is right that the natural ontological attitude differs from constructive empiricism with respect to quantum mechanics, then it shall be argued that they cannot be right for reasons they have so far given.

Darren Abramsonn
Two Sources for Turing
June 3, 14:00-14:30
MacLeod 202

The Turing Test has generated vast amounts of discussion among computer scientists, psychologists, and philosophers. One question that has been occasionally posed, but answered only in the most cursory fashion, is that of the origin of the idea for the Test itself.

In this paper I show that, contrary to claims of the origin of the idea in behaviorist or operationalist inclinations, Turing took the idea for his test directly from Descartes. Although many other commentators have noted the similarity between the Turing Test and comments Descartes makes in the *Discourse*, none has noticed that Turing was aware of these comments. I show, through evidence from the Turing archive at King's College, Cambridge, that Turing was in fact aware of Descartes's views on the distinction between the behavior of machines and beings with reason. Then I briefly discuss whether, given Descartes's use of the technical term 'moral impossibility', Turing's understanding of the Cartesian view is accurate.

Next, I discuss the enormous influence on Turing of his correspondence with, and reading papers of, the British psychiatrist W. R. Ashby. This reading makes clear Turing's understanding of what is now called the 'dynamical hypothesis' in cognitive science, his critical response to it, and his fundamental reason for introducing the concept of 'learning machines'.

In short, I defend the view that through an understanding of the sources available to Turing, we can better understand his motivation for holding two of the most significant philosophical claims presented in his landmark 1950 paper.

Doreen Fraser
The Applicability of Mathematics: A Case Study from Quantum Field Theory
June 5, 9:30-10:00
MacLeod 254

Contemporary physical theories are formulated in mathematical terms. As physicists and philosophers have noted, this obvious fact is actually surprising and difficult to explain. It seems miraculous that, on the one hand, physicists successfully apply mathematics in their attempted theoretical descriptions of the world, but, on the other hand, the domains of mathematics that get applied were often developed years in advance by pure mathematicians unaware of potential applications. One approach to supplying a (non-miraculous) explanation of this phenomenon is to reject the assumption that mathematics performs a descriptive function in physical theories. The alternative that I will explore is that mathematics is less like the language of physics and more like the logical framework(s) for physics; that is, mathematics is used as a tool to facilitate reasoning. I will argue that quantum field theory furnishes a case study of how mathematics can make a non-trivial contribution to physical theorizing by playing a role similar to that of logic in everyday applications. The implications for scientific realism will be discussed. This case study is also interesting because mathematical rigor is essential, providing a counterbalance to recent attempts to explain the lack of rigor in physics (e.g., Kevin Davey, "Is Mathematical Rigor Necessary in Physics?" *British Journal for the Philosophy of Science* 54, pp. 439-463, Sept. 2003).

Dylan Gault
Why Consider Malaria to Be a Mosquito Disease?
June 3, 10:45-11:15
MacLeod 254

It is the position of this paper that malaria research is one area where value concepts associated with disease have played a significant role in research. Researchers tended to focus on mosquitoes as vectors of malaria, not as organisms that also suffer from infection. This encourages what I call a passive view of the mosquito, one where the dynamics of mosquito biology are downplayed save for where that biology is directly involved in the transmission of the malarial parasite to human beings. This approach to mosquito biology encourages certain solutions to the human problem of dealing with malaria and discourages other solutions. Such solutions may be frustrated by the activity of mosquito biology. This was the case with the predictable evolution of resistance within mosquito populations that lead to the eventual failure of anti-malaria programs that used insecticides as a means of reducing human exposure to malaria. On the other hand, viewing malaria as a disease of mosquitoes encourages researchers to investigate programs that make use of mosquito biology. Such a program is currently in development in the form of a fungus that preferentially kills mosquitoes that do not mount an immune response to malarial parasites. Thus, an examination of malaria and mosquitoes provides an opportunity to examine the influence of value concepts of disease on research with historical examples of the consequences of this influence.

Daniel McArthur
Is the String Theory Landscape Question Begging? Debating the Relevance of Testability in Fundamental Physical Theory
June 5, 10:00-10:30
MacLeod 254

Leonard Susskind argues that recent efforts to reconcile string theory with the observed values of physical constants produces not a small number of unique versions of the theory but an array of variations amounting to a number as high as 10^{100}. This implies that at least some versions of the theory will be consistent with any potential observation. Susskind contends that this "landscape" of variations implies that the value of fundamental constants can only be explained with the anthropic principle and not by any predictive version of string theory. The idea is that if things were otherwise, life in the universe would not be possible. The further conclusion is advanced that the search for a version of the theory that can predict the values of fundamental constants is fruitless. The idea has attracted criticism from physicists such as Woit, Gross and others but has attracted little comment from philosophers of science. I show why the debate ought to interest philosophers of science since Susskind purports to re-evaluate the role of testability as a theoretical virtue in fundamental physical theory. I also address Susskind's use of the anthropic principle. I show that his arguments are question begging since they state that the failure of string theory to make predictions about the value of fundamental constants creates no problem for the theory, stating instead that the value of the constants cannot be predicted. Thus, he assumes, without argument, the validity of the landscape approach and its immunity to any test.

Eric Weidenhammer
An Eighteenth-Century Approach to Chemistry and the Body
June 5, 10:45-11:15
MacLeod 214

In 1752, John Pringle (1707-1782), a successful London physician and future president of the Royal Society, published his *Observation on the Diseases of the Army* based on his experiences as a physician with the British army in Flanders. Appended to the book were a series of experiments into the related processes of fermentation and putrefaction that had won him the Royal Society's Copley medal. These accounts reveal a great deal about the relationship of chemistry to the eighteenth-century understanding of the body.

Born in Scotland to landed wealth, Pringle studied medicine at Leiden University under the tremendously influential Hermann Boerhaave (1668-1738). Pringle's medical ideas were based on the Hippocratean "environmentalist" belief that many illnesses were the result of external conditions (particularly heat and moisture) that disposed the body's fluids to putrid infection. According to a widely held theory, vapors emanating from sources of decay were thought to account for a number of contagious diseases.

Pringle's experimental observation of decomposing materials were meant to provide insight into various bodily process (digestion, health, and disease) along with a means of testing potential "antiseptic" remedies. They supported his suggested hygienic reforms to army conditions. Such experiments proved influential in the Enlightenment investigation of miasmatic illness. They can also be situated within an existing chemical tradition exploring the role of fermentation and putrefaction in relation to the body. This paper will locate Pringle's experimental work within this ongoing investigation.

Eric Desjardins
Historicity as Path Dependence in Biology
June 3, 9:30-10:00
MacLeod 254

Since the 1960s, there has been an increasing interest in emphasizing "historicity" in biological theories. Loosely, historicity means that history matters. However, the latter phrase has been interpreted in different ways, and no general account of biological historicity has been hitherto available. I show that historicity, understood as path dependence, can manifest itself at several levels of analysis. Path dependence occurs when a process branches into alternative outcomes, and when the outcome that obtains depends on the trajectory taken by the process. Using the work of Peter Price and his collaborators on sawflies, I will show that path dependence can happen at the developmental, evolutionary and ecological levels. This example shows how a feature of the developmental history (ovopositor) acts as a phylogenetic constraint (maintained ancestral characters around which adaptations are focused) responsible for different patterns in evolution and ecology. I will show how path dependence can be useful in interpreting this cascade of causal influence. Finally, I conclude that path dependence does not exhaust all the reasons why history matters to biology. For instances it applies only to stochastic processes. A more general account would have to encompass historicity in deterministic processes, too. This raises the question: "In what fundamental ways and to what extent deterministic and stochastic historicity differ?" I only point at some differences.

Emerson Doyle
Two Notions of Incommensurability
June 5, 9:00-9:30
MacLeod 202

Many commentators note that Kuhn's thesis of incommensurability can be taken as a reaction to the doctrine of scientific methodology espoused by the logical positivists. Indeed, The Structure of Scientific Revolutions can be read as an attempt at a wholesale rejection of that doctrine. It is clear however, that Kuhn's eschewal of positivist notions is only partial. It is this underlying retention of key positivist themes, coupled with the conscious attempt to distinguish his views from what came before, that gives the thesis of incommensurability its air of appeal. I argue that this interplay is responsible for the difficulties in elucidating what the thesis actually entails, and is ultimately why it fails. Offering a reconstruction of the thesis with consideration to its positivist influences, I suggest that Kuhn argues variously for two distinct versions of incommensurability. The first is a holistic concept greatly embedded in the component parts of a paradigm, while the second focuses on untranslatability. Presenting arguments against both formulations, I identify the foremost difficulty of the thesis to be the interplay between retained positivist notions and Kuhn's novel ideas. I conclude in each case that the thesis of incommensurability is either untenable, or philosophically uninteresting.

Eric Hochstein
A Worry for the Parity Principle
June 3, 9:00-9:30
MacLeod 202

In their 1998 paper "The Extended Mind", Andy Clark and David Chalmers offer what they call The Parity Principle. Roughly put, this principle states that if the performing of a certain task would be considered a cognitive process *had the brain alone performed it*, then the task is *ipso facto* a cognitive one. In this paper, I will show that this principle is too functionally broad as it currently stands. As a result of this, it commits us to granting cognitive agenthood to things we would not want to consider cognitive. First, I will show how the Parity Principle as stated would force us to attribute cognitive processes to such things as plants, vegetation and (possibly even) bacteria. Second, I will argue that the solution to this problem cannot be simply to abandon the Parity Principle, since this would pose far greater problems for cognitive science. Lastly, I will provide some possible suggestions for solving this problem.

Matthew Eisler

Technological Metaphor and Fuel Cell Research and Development

June 3, 15:00-15:30

MacLeod 254

Despite over 50 years of work since the Second World War, researchers have largely failed to develop durable and affordable commercial fuel cells. During this period, expectations tended to exceed the knowledge base, largely because definitions of "success" have varied according to context and application. I argue that we should understand fuel cell research communities as a central node of expectation generation. Historically, experiments on notional fuel cells in controlled conditions using chemically simple fuels fostered hopes for similar performance in practical applications using carbonaceous and hydrocarbon fuels. However, such laboratory fuel cells have not been a reliable gauge of how full-size carbonaceous fuel cells would function in real-world conditions. While historians and sociologists have laid to rest the linear model of technology development, in which "research" must precede "development," this approach has long dominated the conduct of fuel cell research communities. I demonstrate that efforts to apply basic knowledge in fuel cell technology have often become renewed searches for basic physical principles, accounting for the boom-bust character of post-Second World War fuel cell research.

Eric Oberheim

Feyerabend, Einstein and Incommensurability

June 5, 9:30-10:00

MacLeod 202

Paul Feyerabend and Thomas Kuhn are often accredited with independently introducing 'incommensurability' into the philosophy of science in 1962. They were both initially treated with much skepticism, as they appeared to be implying that science is irrational. This paper takes a closer look at Feyerabend's introduction of 'incommensurable' (1962). First, the basic idea of incommensurability is explained. Then, based on archive materials, Feyerabend's basic idea is traced back to his unpublished (1951) doctoral thesis, and his use of insights by Duhem (1906). Even so, it was none other than Albert Einstein who first used the term 'incommensurable' in 1946 to describe the relation between theories in physics. Einstein explicitly restricted his discussion of weighing the comparative merits of incommensurable theories to those that talk about the entire universe. This criteria also marks the main difference between Kuhn and Feyerabend's incommensurability theses. For Feyerabend, but not Kuhn, only such universal theories can be incommensurable. Lastly, it is argued that the metaphysical position Feyerabend explicitly delineated while introducing incommensurability was Kantian — but without necessary, unchanging categories. This is the same sort of metaphysical position to which Einstein explicitly subscribed when he used the term 'incommensurable'. Taken together, the

historical evidence indicates that although Feyerabend was interpreted by the community of philosophers of science to be promoting a radical, irrationalist thesis, in fact, his basic claims were merely repetitions of Einstein.

Eran Tal

Simulated Evidence: Signatures of a Quantum Phase Transition

June 4, 16:00-16:30

MacLeod 202

What makes empirical data serve as reliable evidence for phenomena has been the subject of a recent debate among philosophers of science. Jim Woodward (2000) has influentially argued against what he dubs “logical” accounts of confirmation, and in favour of viewing evidential relationships as based on an empirical investigation of techniques for data production. In this paper I draw attention to an increasingly common method for establishing standards of evidence in physics, a method which fits into neither of these competing accounts. When lacking a clear idea of which data would count as evidence for a certain phenomenon, physicists often run a computer simulation of their detection process. The simulation produces “signatures”, i.e. patterns of data that are expected to be observed if the phenomenon is present.

In this paper I focus on the role played by such simulated signatures in the study of the Mott-insulator-to-superfluid phase transition in ultracold gases. Signatures for this phase transition were produced by simulating an actual experiment and reproducing its results. The simulated signatures were then used to justify the experimenters’ claims to detect the phase transition. As I argue, neither “logical” nor empirical approaches towards scientific evidence of the kind described by Woodward can make sense of this evidential use of computer simulation. Instead, I propose to view signature-based detection as employing a certain kind of confirmational holism, a kind which nevertheless does not fall prey to the theoretical difficulties typically associated with Duhem-Quine varieties of holism.

Nicolas Fillion

Aristotle’s Logic and its Modern Reconstructions

June 3, 9:30-10:00

MacLeod 202

It is widely accepted, and quite rightly, that Frege's publication of the *Begriffsschrift* in 1879 begat the most prolific era of the history of logic. However, for philosophers such as Quine, Geach, *et al*, the obvious ideological correlate of this claim is, that traditional formal logic no longer retains any theoretical interest. The effect of this view is that contemporary logicians have looked upon Aristotle's logic with jaundiced eyes, identifying as many mistakes as possible and exclusively emphasizing discontinuities.

In this paper, I will argue that this view is wrong insofar as Aristotle's logic can be reformulated in a way that satisfies the standards of modern logic without compromising its underlying philosophy. The argument will consist of a critical examination of the two most famous attempts to produce such a model, namely Lukasiewicz' reconstruction of syllogistic as an extension of propositional calculus (1957) and Corcoran-Smiley's natural deduction system (1974). The former view will be shown to be inadequate, since it fails to do justice to Aristotle's methods of proof. Therefore, despite its authoritative position in the literature, Lukasiewicz's view does not render its *lettres de noblesse* to traditional logic. It will also be shown that the latter view makes sense of most of Aristotle's claims regarding the constitutive elements of logic and the methods of proof (conversion, ecthesis, per impossibile). As a reconstruction of Aristotle's syllogistic according to our current standards, this model *shows* that Aristotelian logic still *has* a theoretical interest.

Melanie Frappier
If 'Copenhagen' Is Leibzig's Code Name, What Does 'Interpretation' Mean? :A Re-Examination of the Origin of the Copenhagen Interpretation
June 4, 16:30-17:00
MacLeod 202

In his 2002 PSA paper, Howard (2004) argues that Bohr, Heisenberg and the other members of the "Copenhagen School" never shared a unique interpretation of quantum mechanics. According to Howard, the belief in the existence of an official "Copenhagen interpretation" find its origin in the 1955 paper where Heisenberg introduced the expression in order to describe not Bohr's, but *his* own interpretation of quantum mechanics. The myth of an orthodox interpretation, Howard continues, was then rapidly propagated by Heisenberg's most fervent admirer (Hanson), proponents of rival interpretations looking for a straw theory to attack (like Bohm), and an epistemological anarchist (you guessed it, Feyerabend).

The story is not so simple. As I show here, Heisenberg's 1955 paper did little more than christening what many (de Broglie, Born, Sommerfeld, etc.) already referred to as the "orthodox" or "usual" interpretation of quantum mechanics. Only a few people in the 1950s noticed the variety of views subsumed under the names "Copenhagen" and "orthodox" interpretations, but among them we find no other than Hanson, Feyerabend, and to some extent Bohm. This confusion as to the existence of a unitary interpretation of quantum mechanics and the contradictory claims made on the topic by many authors arises, I argue, from the slow distinction being made between the different meanings of the term "interpretation" (and their different philosophical motivations) in the 1950s, a reflection triggered by the development of "competitors" to the so-called "Copenhagen interpretation."

Guillaume Maranda
The Double Language Model and The Theory-Ladenness of Our Observation Reports
June 4, 16:30-17:00
MacLeod 254

The thesis of the theory-ladenness of observation is often explained by the fact that we use theoretical terms to express our observational judgments. As such, we really are confronted with the thesis of the theory-ladenness of our observation reports. The latter comes as a criticism of the double language model in philosophy of science and it infamously leads to the thesis of the incommensurability between theories, relativism, and scepticism. In this paper, I shall show how this is not necessarily the case. More precisely, I shall expound the main strategies used to reject the following argument (ARG):

1- Our observation reports are theory-laden
2- If our observation reports are theory-laden, then our empirical tests are circular or incomplete.

THEREFORE

3- Our empirical tests are circular or incomplete.

In fact, the claim that our observation reports are theory-laden has been interpreted in three different ways:

- **T-L1**: An observation sentence is theory-laden when an observational proposition is formulated with theoretical vocabulary (van Fraassen 1980, 1992).
- **T-L2**: An observation report is theory-laden if our perceptual judgments involve the application of concepts and that the meaning of these concepts is fixed by a theory (Feyerabend 1958) (Churchland 1979) (Hesse 1970).
- **T-L3**: An observation report is theory-laden when its intensional content was a theoretical statement (Maxwell 1962) (Shapere 1982) (Suppe 1972).

I shall argue that we can always reject the second premise of argument (ARG) regardless of which thesis (T-L1, T-L2 or T-L3) we rely on in order to interpret its first premise. In conclusion, I shall explain why current debates are (and should be) now focusing on T-L3.

G.G.R. Murray

An analysis of Einstein’s Theory of Special Relativity as a Principle Theory of Space-Time

June 4, 14:00-14:30

MacLeod 202

In an article he that he wrote for *The Times* in 1919, Einstein described a division he perceived between two different types of scientific theories, which he termed constructive and principle theories. Einstein thought that an understanding of this distinction would illuminate the nature of his theory of special relativity which he considered to be a principle theory. In this paper I examine this distinction and how it can shed light upon Einstein’s theory. I consider how and why special relativity came to be a principle theory of space-time rather than a constructive theory due to the its reconciling of the apparent contradiction between the principle of relativity and the law of the propagation of light and its necessity for an empirically verifiable definition of space-time. I look at the implications of special relativity being a principle theory – how it prevents us from creating a corresponding constructive theory and how the principle theory is explanatory. This analysis of special relativity as a principle theory of space-time will be valuable not only in enhancing our understanding of special relativity, but also in providing us with a greater insight into the significance of the distinction Einstein made between constructive and principle theories and a greater comprehension of the nature of scientific explanation in general.

Benny Goldberg

Leibniz, Mechanism, and Machines

June 3, 14:00-14:30

MacLeod 214

The goal of this paper is to understand Leibniz's conception of mechanism and the related notion of machines. The motivation for this goal is Leibniz's famous 'mill' argument, where Leibniz argues that there is no way to explain the mind mechanically—hence the need for simple substances, monads. I attempt to ferret out Leibniz's conception of mechanism in three ways: first, I look at the mill argument itself, and attempt to see how Leibniz uses mechanism in this argument; second, I look at Leibniz work more broadly, focusing on the late, mature period (roughly post-1680s); and third, I will examine an actual machine devised and constructed by Leibniz, his calculating machine, to determine what notions of mechanism are operative there. In the end, I specify three conceptions of mechanism, each a refined and more specific version of the last: general mechanism, formal mechanism, and functional mechanism. Along the way a number of other issues in Leibniz's philosophy will arise, including his philosophy of mind, the importance of unity, and the problem of animal generation.

Ernst Hamm
Exchange, Natural Knowledge and Dissent in the Dutch Enlightenment
June 3, 14:30-15:00
MacLeod 214

Exchange was crucial to the commercial life of the Dutch republic in the seventeenth and eighteenth centuries, integral to the more general character of the Dutch Enlightenment, and exemplified by religious toleration and the pursuit and promotion of natural knowledge. A fruitful way to examine the movement of ideas and things between different bodies of knowledge is by looking at Mennonites, a group of Dutch religious dissenters who were especially active in the promotion of science and Dutch urban life. The case of the Mennonite Galenus Abrahamsz. (1622-1706) – physician, preacher, alchemist, founder of a seminary, Spinozist, (failed) entrepreneur, and (successful) promoter of religious tolerance – offers an illuminating example of the ways in which lines of exchange cut across and informed distinct bodies of knowledge in the early Enlightenment. Throughout the eighteenth century Mennonites were active in the promotion of science, be it by sponsoring lectures by natural philosophers such as Daniel Fahrenheit, publishing, and participating in the scientific societies.A notable centre for experimental philosophy in Amsterdam was the Mennonite seminary, which housed one of the finest collections of scientific instruments in the Netherlands. This paper will argue that the crucial exchange for Mennonites was not, as one might expect, between theological doctrine and scientific ideas, but between science and the promotion of the common good as understood in the Dutch Enlightenment.

Martha Harris
Getting Engaged with Atoms: Chemical Perspectives on Molecules and Bonding
June 3, 9:00-9:30
MacLeod 212

Theories of chemical bonding in the mid-twentieth century relied on a model of the atom defined largely by quantum physics. This dependence was necessary in order to fully account for the behaviour of the electron that caused chemical bonds to form, according to both the valence bond and molecular orbital approaches, in which the molecule is treated as a collection of atom-to-atom bonds, or an entity built up from contributing electrons, respectively. However, the adoption of a physical atomic model by chemists marked a departure from previous modes of thought, in which atoms were viewed as objects that could not be engaged with on chemical terms. Chemists of the nineteenth century and turn of the twentieth were more likely to engage with the properties of atoms by studying molecules, rather than the constituents of molecules, the atoms themselves. In the 1910s and 1920s, chemists began to study the electron's role in bonding, but it was not until the adoption of wave mechanics into chemical bonding in the late 1920s that chemists were able to fully engage with the atom's physical properties. By studying the contributions of physical chemists Gilbert Lewis and Irving Langmuir in developing the cubic model of the atom within a chemical atomism, and contemporary attitudes of physicists and chemists about the value of physical and chemical perspectives on the atom, we will see that theories of chemical bonding encouraged chemists to engage with the atom as a chemical and physical object in the early twentieth century.

Matt Hettche
Kant and the Prohibition of Armchair Cosmology
June 4, 11:15-11:45
MacLeod 202

Do the Antinomies of the first *Critique* serve as an argument for the method and progress of modern cosmology? After addressing some of the standard objections to Kant's Antinomies, I outline an account of Kant's project in the 'Antinomy Chapter' that explains how an affirmative answer to this question just might be possible. I maintain, in particular, that Kant's focus in the Antinomies is not only consistent with the largely empirical direction of modern cosmological research but that the theory of scientific progress implicit to Kant's Antinomies may in fact also explain how modern cosmology has managed recently to secure its own autonomy as a science. Building upon the work of W. H. Walsh, Michael Friedman, and Michelle Grier, I argue that since Kant's general notion of an 'antinomy of pure reason' is not in-itself philosophically implausible, there is a reasonable sense in which Kant's refutation of rational cosmology in the 'Antinomy Chapter' supports the method and progress of modern cosmology.

Robert G. Hudson
Carnap's Empiricism, Lost and Found
June 5, 10:45-11:15
MacLeod 202

Recent scholarship (by mainly Michael Friedman, but also by Thomas Uebel) on the philosophy of Rudolf Carnap covering the period from the publication of Carnap's 1928 book *Der Logische Aufbau der Welt* through to the mid to late 1930's has tended to view Carnap as espousing a form of conventionalism (as epitomized by his adoption of the Principle of Tolerance), and not a form of empirical foundationalism. On this view, it follows that Carnap's 1934 *The Logical Syntax of Language* (abbreviated *LSL*) is the pinnacle of his work during this era, this book having developed in its most complete form the conventionalist approach to

dissolving the pseudoproblems that often attend philosophical investigation. In this paper, I seek to resuscitate the empiricist interpretation of Carnap's work during this time period. My argument in this paper is to note, first, the character of Carnap's empiricism in both the *Aufbau* and in his 1932 *Unity of Science*, and then to describe how Carnap in *LSL* eschews for the most part the empiricism he espouses in these earlier works and adopts alternatively a form of conventionalism succinctly embodied in his Principle of Tolerance. Unfortunately, the conventionalist approach Carnap sets forth in *LSL* faces the serious hazard of collapsing into epistemological relativism. My speculation is that Carnap came to recognize this deficiency in *LSL* and in subsequent work ("Testability and Meaning", published in 1936/37) felt the need to re-instate his empiricist agenda. This subsequent work provides a much improved empiricist epistemology compared to Carnap's previous efforts.

Ingo Brigandt

Continuity in Scientific Concept Use: Homology in the 19^{th} Century Before and After Darwin
June 4, 16:00-16:30
MacLeod 214

The conventional wisdom about the notion of homology in 19^{th} century biology is that evolutionary theory introduced a novel concept, a post-Darwinian 'phylogenetic' homology concept, distinct from the pre-Darwinian 'idealistic' homology concept. This idea has been supported philosophically by the tenet that the nature of scientific concepts resides in theoretical definitions, and historically by the (nowadays extensively criticized) assumption that pre-Darwinian biology was in the grip of essentialism. Against the conventional wisdom, the present paper points to important continuities in the use of the homology concept during the 19^{th} century, while at the same using some of these stable features of concept use as a basis for understanding the adoption of post-Darwinian accounts and definitions of homology. Homology was used in actual *research practice* in quite similar ways throughout the 19^{th} century, in terms of how homologies were established and how knowledge about homologies was used in morphological, embryological, and taxonomic work. Moreover, before and after Darwin the homology concept was used to pursue largely the same *scientific goals*, namely the morphological comparison of structures and the classification of species. This historical study of the homology concept motivates some philosophical ideas about concepts. The identity of a concept does not boil down to a theoretical definition, but consists in several aspects (some of which may change in history), including features of practical concept use and the scientific goal pursued by a concept's use. This latter notion accounts for why novel theoretical definitions are adopted (semantic change).

Isaac Record

Instruments of Explanation
June 4, 17:00-17:30
MacLeod 202

Technology shapes scientific explanation. Some technologies enable novel kinds of explanation, requiring novel justifications. There is no question that instruments of various sorts have made certain styles of explanation easier; digital computers 'extend' or 'extrapolate' the

capabilities of unaided humans to tabulate results or solve equations (Humphreys 2004). Instruments in these categories provide interesting puzzles and case studies for philosophers and historians as they negotiate between theory and the practice of scientists. But instruments in Humphreys' third category, comprising those that 'augment' human capacities, present a deeper challenge.

In their work on the atomic bomb, Ulam, von Neumann, and Metropolis used computer aided numerical methods (Monte Carlo simulations) where analytic methods proved intractable and real experiments too dangerous or expensive (Galison 1997). Mere danger or expense do not rise to the level of a 'problem' in philosophy; our worry is possibility. Computers made practical what without them is very difficult, but rarely impossible. Nevertheless, Monte Carlo simulations enable new kinds of scientific explanations, and these explanations cannot rest on extensions of old accounts.

Advances in technologies make new explanations first feasible, then acceptable, and finally standard. The standards of explanation change as scientists adopt methodologies crucially dependent on particular technologies. This raises the worry that as scientists adopt new computer methods, they will admit corresponding explanations without any external validation. In this paper, with the help of several case studies, I provide a general philosophical account of the complex relationship between instruments and explanation.

Jeremy Wideman
In Defence of Conservation: A Theoretically Grounded and Practically Applicable Approach to the Classification of Species
June 4, 16:30-17:00
MacLeod 214

The paper proposes that the current conception of species be split into two notions: a unit of evolution and a unit of conservation. A unit of evolution can be conceived of as a natural kind, which persists for a time accumulating variations and then evolves via natural selection or goes extinct. Parts of a unit of evolution can be related to one another by any of several current species concepts (biological, ecological, phylogenetic, etc.). A unit of conservation is a definable population or group of organisms that we have deemed as worth conserving. These will often consist of the variations found within a unit of evolution. For example, the brown bear and polar bear may form a single evolutionary unit but they are undeniably each a unit of conservation. Units of conservation may seem like arbitrary units but I argue that a unit of conservation can be thought of as a group of organisms with the propensity to sometime in the future become a unit of evolution. This is a theoretical unit that can be empirically tested. The present account enriches John Dupré's suggestion to distinguish between species as units of evolution and as units of classification (2001, "In defence of classification"), first by laying out practically applicable criteria of classifying species as units of conservation, and second by theoretically discussing aims of conservation and the relation of units of conservation to units of evolution.

Jeremy Howick
Double-Blinding: Benefits and Risks of Being in the Dark
June 5, 11:15-11:45

MacLeod 254

The feature of being ‘double blind’, where neither patients nor physicians are aware of who receives the experimental treatment, is universally trumpeted as being a virtue of clinical trials. Hence, trials that fail to remain successfully double blind are regarded as inferior. The rationale for this view is unobjectionable: double blinding rules out the potential confounding influences of patient and physician beliefs. Nonetheless, viewing double blind trials as necessarily superior leads to the paradox that very effective experimental treatments will not be supportable by best evidence. If a new drug were to make the most severe symptoms of, say, the common cold disappear within seconds, most participants and investigators would correctly identify it as the latest wonder drug and not the placebo. Any trial testing the effectiveness of this wonder drug will fail to remain double blind. It seems strange that an account of evidence should make *a priori* judgments that certain claims can never be supported by ‘best evidence’. It would be different if the claims at issue were pseudoscientific – untestable. But so far as treatments with large effects go, the claim that they are effective is highly testable and intuitively they should receive *greater* support from the evidence than do claims about treatments with moderate effects. In this paper I argue that the two *potential* confounders ruled out by double blinding are often not *actual* confounders outside placebo controlled trials of treatments with mild effects and that have subjective outcome measures. In short, the view that double blinding is an epistemic good requires qualification.

Jim Jordan
Feyerabend’s Pluralist Approach to Scientific Knowledge
June 5, 10:00-10:30
MacLeod 202

Paul Feyerabend was once called “the worst enemy of science.” I contend, on the basis of his earlier philosophical work, that this label is undeserved. Feyerabend’s criticisms of scientific practice arise out of an ethical concern, inspired by Mill, for scientists and non-scientists alike to express and research their theories about the world in whatever ways are appropriate to their practice. However, in granting this freedom, he maintains, following Popper, that there must be a way to validate these competing claims as knowledge about the world. The theories arising from methodological pluralism would continue to remain subject to independent empirical verification, without rejecting the theories out of hand on the basis of their origin. Moreover, in his view, even untestable theories have some value in the search for knowledge, for they can contribute to a theoretical dialogue that may produce a verifiable theory. I give a brief critique of his conception of this dialectic. I conclude that the model of knowledge acquisition Feyerabend holds forth is not robust on its own, but that a restricted application of it may help expand the body of scientific knowledge.

James A. Overton
Building Better Bridges Laws with Category Theory
June 4, 10:00-10:30

MacLeod 214

Much of the debate over theory reduction focuses on bridge laws, which express the identities of theoretical terms of the primary theory with those of the secondary theory (Nagel 1961, Schaffner 1967, Fodor 1974). The strength of the identity relation is problematic, because many times it appears that translation between the concepts of the two theories is only partial and intransitive (Wimsatt 1976).

In this paper I propose a weaker bridge principle: a morphism between the "core model" of the primary theory and that of the secondary theory. By "core model" of a theory I mean a general model which is capable of describing the range of systems to which the theory applies. I propose that these models and the morphisms between them can be understood in terms of category theory: a category is a mathematical structure which captures both states and the processes that transform one state into another. A functor is a morphism between categories which preserves the structure of states and the processes connecting them.

To demonstrate my proposal I apply category theory to classical thermodynamics and statistical mechanics. I then establish a functor which bridges the category of classical thermodynamics and the category of statistical mechanics. Using two different formulations of these categories I illustrate both the strengths and limitations of the functor approach -- a functor cannot bridge categories if their structures are incompatible. I conclude that the bridge functor account does a better job in this example of theory reduction than bridge laws expressing identities.

Jacob Stegenga
Robustness and Contradictory Evidence
June 3, 16:15-16:45
MacLeod 214

A novel technique works well, according to the “experimenter’s regress” argument, only when the technique gives acceptable results (Collins 1985). Robustness, a recent notion in philosophy of science, has been used to counter such relativist arguments: multiple theoretically independent techniques giving coherent results provide greater epistemic support to those results than would a single technique. For example, Hacking argued that when a cellular structure is observed with different types of microscopes, we have more reason to believe that the structure is real (1983). Robustness can “break the data circle” of the experimenter’s regress (Culp 1994).

Rasmussen (1993) used an example from cell biology to dispute the epistemic value of robustness: multiple methods in electron microscopy suggested the existence in cells of what is now considered an artifact. Three further arguments can be made against robustness: scientists don’t always have multiple techniques, and even if they do, the techniques may not be theoretically independent. Third, epistemic guidance is needed most in difficult cases, when multiple techniques produce incongruent data. A current controversy regarding the mode of influenza transmission demonstrates this problem. Occupational health experts argue (using mathematical models and animal experiments) that influenza could be transmitted through the air, whereas infectious disease physicians argue (based on clinical experience and observational studies) that influenza is transmitted only by contact. This controversy demonstrates the poverty of robustness: multiple techniques and reasoning strategies are used by different scientists (representing different disciplines), but the results remain inconclusive.

I conclude, with Rasmussen, that a plurality of considerations are used in evidential assessment, including one's theories of techniques, the value of precedent data, weighting of different methods, the risks and benefits of given conclusions, aesthetics judgments, and inferences regarding the function of hypothesized entities and mechanisms. Given disparate data from a plurality of techniques, and given a lack of methodological meta-standards, the outcome of scientific controversies depends on social forces affecting the choice of methods and standards.

Jonathan Tsou

Psychiatric Kinds, Looping Effects, and Stable Targets: Are Any Mental Disorders Natural Kinds?

June 3, 10:45-11:15

MacLeod 254

In this paper, I argue that some mental disorders are natural kinds insofar as members of these kinds share the same causal structure. To adopt a phrase from Hilary Putnam (1975), natural kinds share the same "general hidden structure." Following Richard Boyd's (1988, 1989, 1991) homeostatic cluster property theory of natural kinds, I present the causes of mental disorders (e.g., a chromosomal or neurobiological anomaly) as homeostatic mechanisms and their effects (i.e., a characteristic group of co-occurring symptoms) as clusters of properties. Down syndrome, PKU, and paranoid schizophrenia are presented as paradigm cases of such kinds. I present my argument against Ian Hacking's (1999, ch. 4, 2007) analysis that suggests that the objects classified by human science classifications—because of the looping effects of such classifications—are inherently unstable entities (what Hacking calls "moving targets"). In contrast to Hacking's analysis, I present mental disorders that are natural kinds as objects of classification that remain stable in spite of looping effects.

Kareem Khalifa

Inference to the Best Explanation: Virtues, Causes, and Contrasts

June 4, 14:00-14:30

MacLeod 254

I present a new account of Inference to the Best Explanation (IBE), which following Peter Lipton, consists of causal triangulation via contrastive inference. In distinction from Lipton, I hold that IBE is not only an eliminative method, but also consists of comparative criteria. Additionally, I deny Lipton's claim that an eliminative method requires an overarching explanation to determine if a hypothesis is incomplete or incorrect. I then argue that many of the theoretical virtues—the cornerstones of more traditional accounts of IBE (Harman, Thagard)—should be understood as heuristics for realizing this form of causal triangulation. Specifically, a hypothesis exhibiting simplicity, consilience (scope), mechanism, and precision will also be favored by my account of IBE. This permits this account of IBE to blunt a variety of frequently-raised charges that the theoretical virtues lack epistemic value.

Kirsten MacDonald
Evolutionizing Culture: Can It Be Done?
June 4, 17:00-17:30
MacLeod 214

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

Kalil T. Swain Oldham
"The Purpose of All of Science:" Gustav Kirchhoff's Contemporaries Respond to the Doctrine of Description
June 3, 9:30-10:00
MacLeod 214

In his Berlin lectures of 1875 Gustav Kirchhoff argued for a severe limitation of the scope of "pure mechanics." He asserted that physicists ought not to concern themselves with investigating causes, which only leads to confusion. They should, on the other hand, focus only on developing the most complete and simplest description of natural phenomena. This was his doctrine of description. In this paper I examine three different responses to Kirchhoff's new program for physics – by Hermann von Helmholtz, Ernst Mach, and Eduard Zeller. Their interpretations, appearing in the thirty years after Kirchhoff's lectures, demonstrate his importance for *fin-de-siècle* debates in the philosophy of physics. Kirchhoff was more than an important touchstone for leading scientists and philosophers; in forming their philosophical positions, each appropriated Kirchhoff's legacy for his own purposes. Helmholtz regularly defended the notion of cause against the doctrine of description. Mach, on the other hand, embraced Kirchhoff's remarks and he hoped Kirchhoff's fame and notoriety would cast a favorable light on his own "principle of the economy of physical inquiry." And Zeller, in suggesting his "metaphysics as an empirical science," argued, against Kirchhoff and Mach, that description and explanation were in fact inseparable. Kirchhoff's comments amounted to the outlines of an epistemological position, and his doctrine of description portended a fundamental transformation in physicists' thinking about their science. Historically Kirchhoff has represented the rising tide of widespread ambivalence towards complete, causal, or true explanations in the natural sciences. We have yet to grasp precisely how he fits into this story.

Lisa Mullins

Making Science History: Fontenelle and the *Histoire et Mémoires* of the Académie Royale des Sciences

June 3, 15:00-15:30

MacLeod 214

This paper explores one of the most significant roles Bernard le Bovier de Fontenelle played as secretary of the French Academy of Sciences in the first four decades of the eighteenth-century. There is a volume of the *Histoire de l'Académie royale des sciences* for each year of its existence in the old regime. Each volume has two distinct separately paginated sections: the *Histoire*, written by the secretary, self-consciously aimed at a non-specialist audience; and the *Mémoires,* a collection of essays by academicians. The *Histoire* is a summary and simplification of many of the accompanying *mémoires*, and some of the natural philosophy done in the Académie during the year. Most historians use the *Histoire* unproblematically – it is simply the secretary's summary of the year's activities. In this paper, I disregard that assumption and focus on some of the many stories that the *Histoire* tells, not only about the development of individual scientific disciplines, but also about the emergence of history of science, and Fontenelle's own intellectual convictions and prejudices. I compare the *Histoire*'s record of the Academy's work to that of the *procès-verbaux*, the manuscript minutes of every biweekly meeting, in order to reveal what disciplines and what academicians were under- and over-represented in this 'impartial' annual account. I weigh Fontenelle's *Histoire* articles against their accompanying *Mémoires* articles to discover how Fontenelle transformed original natural philosophic findings into historical events. By examining Fontenelle's literary strategies and techniques, and specifically the new narratives for natural knowledge, I argue that Fontenelle changed the meaning of and value ascribed to that knowledge, thereby creating new texts of natural philosophy.

Letitia Meynell

The Depiction of Causes

June 4, 15:00-15:30

MacLeod 214

There are a number of different epistemic functions that scientific images can play, but in this paper I will focus on the capacity of images to depict causes. I will argue that, despite the limitations of two-dimensional static media, a number of scientific representations successfully depict causes and are, because of this, epistemically significant. I begin with a brief explanation of the theory of representation that seems best able to explain the distinctive capacity of pictures to represent causes?Kendall Walton?s account, defended in Mimesis as Make-Believe. One advantage of this theory is that it does not imply or assume any specific metaphysics of causation, but instead explains how viewers with different understandings of causation can see a particular representation as causal and gain insight into a particular phenomenon through the representation.

I take mechanical drawings, particularly assembly drawings, to be paradigm cases of depictions of causes. My analysis of this type of technical drawing will form the basic framework against which I will consider examples of depicted causes from various sciences.

Leslie Tomory

The origins of the manufactured gas industry in the late 18th and early 19th century: a case study in the interaction between science and technology in the Industrial Revolution

June 5, 11:15-11:45

MacLoed 214

Historians have long debated the link between science and technology in the Industrial Revolution. A direct link has proved elusive, with few clear cases of scientific theory being the direct foundation of a particular technology. This paper will examine a specific case where the link between the two is clear: the manufactured gas industry. The manufactured gas industry, or gaslight, emerged in the period 1790-1820, with roots in France, Germany and England. The pneumatic chemistry of the 18th century provided the concepts, techniques, and instruments which were appropriated by the founders of the manufactured gas industry.

The retort, the pneumatic trough, lime purification, and the gasometer were used for research purposes long before they became the essential components of the gas plant. In addition, knowledge about inflammable gases was developed in pneumatic chemistry before it was applied in industry. The first researchers trying to develop the technology of the manufactured gas industry, such as Philippe Lebon, Zaccheus Winzler, or William Murdoch at Boulton and Watt, were conversant with the pneumatic chemistry of the period and used this knowledge in their development work.

Mathieu Côté-Charbonneau

Synthetic psychology: an analysis of Valentino Braitenberg's *Vehicles* approach to the scientific study of the mind

June 5, 9:30-10:00

MacLeod 214

As many historians and philosophers of psychology have argued, cognitive psychology and neuropsychology are two sciences mainly driven by an analytical methodology, that is, the scientist first begins his researches by postulating the existence of mental states (e.g. attention, understanding, etc.) and then enquires about the possible mechanisms realizing these mental functions (an analytical strategy know as reverse-engineering). The analytical strategy presupposes a semantic interpretation of mental states, that is, mental states are always about something that exists outside the mind (e.g. object recognition). Such an approach has been the target of multiple of criticisms: their main point is that the conceptual framework used by the analytical strategy is not a product of empirical enquiry but is an a priori remnant of our common-sense understanding of mental life.

Valentino Braitenberg's synthetic psychology proposes a new way of approaching the mind by relieving psychology of semantically loaded mental states and by insisting on pattern recognition and transformation as the primal function of the brain. Braitenberg's synthetic psychology proposes an understanding of the mind as a closed system in which what matters are the neuronal activation patterns being processed by the brain and not the real external objects affecting the sensorial apparatus.

The purpose of this talk is to present Braitenberg's alternative theory and methodology for cognitive psychology. I will discuss the theoretical foundations of this approach and will sort out some philosophical implications of his original research program for our understanding of the nature of explanations in psychology.

Moira Howes

Epistemic Emotions, Salience and Ignorance in Scientific Reasoning

June 4, 11:45-12:15

MacLeod 214

In this paper, I examine a striking case in biology wherein relevant evidence and valuable lines of inquiry are ignored. While gender values almost certainly play a role in this case concerning immune functions of the human female reproductive tract, explanations of scientific ignorance and evidence mismanagement in terms of values is sometimes too abstract to convince skeptical audiences. Moreover, such value-based explanations are themselves only partial. Instead of relying solely on values to explain certain weaknesses of reasoning present in this and similar cases, I argue that we should draw upon work in the philosophy of emotion and the epistemology of ignorance. Because emotions have a primary role in establishing salience (De Sousa 1987), the analysis of emotions is particularly relevant to work on the epistemology of ignorance, wherein it is claimed that ignorance is not merely an uninterested lack of knowledge, but is in some cases actively produced and maintained. (Harding 2006; Tuana 2006, 2004) I address how epistemic feelings such as interest, curiosity, anxiety, and feelings of doubt and certainty are involved in decisions to ignore relevant evidence, actively produce and maintain ignorance, and avoid clearly significant lines of inquiry. And, because epistemic feelings say as much about investigators as the investigated, they provide clearer means by which to connect personal, social, ethical and other values to epistemic evaluation in science. The objective is to understand better scientific irrationality and failures of intellectual virtue in this and relevantly similar cases.

Matthew D. Lund

N.R. Hanson on the Relation Between Philosophy and History of Science

June 3, 14:30-15:00

MacLeod 254

This paper explores and defends Hanson's contention that it is the supposition and testing of normative criteria for science that allows analysis of case-studies to go beyond the cases themselves. Hanson uses Galileo's discovery of the law for free-falling bodies to show that while facts inexpressible in a given notation are not impossible to grasp, the practical obstacle such a process involves is very conceptually important for understanding the growth of science. Hanson's emphasis is on how the successful conceptual framework for free-fall was rationally constructed – Hanson's disbelief in flashes of inspiration separates him from Kuhn and it follows directly from his commitment to a normative framework. Normative criteria are the philosophical elements that allow us to learn from, and abstract away from, case-studies.

Mike Maleki
Why Human Kinds Are Different
In Defence of Hacking
June 3, 11:15-11:45
MacLeod 254

Ian Hacking's distinction between 'natural' and 'human' kinds has recently been criticized on some grounds. In this paper, I defend it specifically against a critique levied by Rachel Cooper. Hacking has identified several characteristics of human kinds, and concludes that these make human kinds radically different from natural kinds. I will argue that Cooper's critique fails to show that Hacking's account is wrong. Ultimately, the arguments in this paper would entail that there is little unity among the natural and the social sciences, yet it is premature to find this worrisome.

Michael McEwan
The Semantic View of Scientific Theories: Whats Right About it?
June 4, 9:30-10:00
MacLeod 214

According to the *semantic view*, scientific theories are characterized as (or by) a collection of models. Though popular for more than thirty years, this view has increasingly fallen into disrepute. Some complain that the notion of a 'model' is left too vague, or that it is sometimes equivocated on. Still others think the notion is construed too narrowly. In addition, most of the purported virtues of the sematic view have come under attack. Critics argue that the view is not 'language independent' in any special way, that it does a poor job of reflecting scientific practice, and, moreover, that it is not a particularly apt vehicle for scientific realism. Assuming these criticisms are well founded—and most are—we are left wondering what, if anything, is actually right about this view. This paper is, in part, an answer to this question. I start with a diagnosis of the problem and identify a number of problematic doctrines typically associated with the semantic view. I then revisit Patrick Suppes' original characterization and show that much of it is unproblematic. I propose a "modest semantic view" which retains many of Suppes' key insights, but removes its problematic baggage. Though it has many limitations, it is shown that the modest semantic view is still the ideal tool for at least two tasks: (i) investigations into scientific methodology (like those undertaken by Suppes); and (ii) investigations concerning the structural continuity between theories.

Matteo Mossio et. al.
Self-maintaining organization and biological functions
June 3, 9:00-9:30
MacLeod 254

One of the main philosophical issues raised by the concept of biological function relies on a tension between two apparently conflicting exigencies. On the one side, functional attributions seem to have a genuine explanatory role in accounting for the nature of living systems. On the other side, the concept of function contains a *normative* dimension, since it refers to some effect that the entity is *supposed* to produce, and that would *explain* the existence and the structure of the considered entity. In this respect, the concept of function generates an epistemological problem, to the extent that it calls for a naturalized account of its normative nature, which would make it compatible with the accepted structure of scientific explanation.

In this paper, we articulate and defend an original account of functional attributions to biological systems, which shares with more classical systemic approaches the basic intuition according to which functional attributions provide information about properties of the *current organization* of a biological system. In particular, we will suggest that the concept of function is inherently related to the idea of *self-maintaining organization*, to the extent that functions can be defined as contributions to the maintenance of an organizationally *closed* and *differentiated* system.

We will argue that our account offers a solution of the aforementioned tension, since it provides a framework in which functional attributions are at the same time truly explanatory and informational (in organizational terms), and fully compatible with a naturalized account of normativity.

Neelam Sethi
Rethinking Normativity
June 4, 11:15-11:45
MacLeod 214

In this paper, I explore the recent turn in philosophy of science to what I term hard normativity (hard choices and hard issues are central to this notion of normativity). Within traditional philosophy of science it is labeled external rather than internal normativity, but the sense of outside and inside is both fluid and negotiable as I attempt to argue in the paper. By examining this new turn in normativity (which best describes recent work by several philosophers) I aim to shed some light on the connection between the recent development in philosophy of science and feminist philosophy of science. In particular, I look at the recent writings of Philip Kitcher, Nancy Cartwright and others; I then relocate this new work alongside the works of several feminist philosophers of science with the aim of exploring if there is any convergence (or lack thereof) in how each views normativity and if so what its consequences might be for philosophy of science.

Kathleen Okruhlik
Putnam, Proctor, and Political Economy
June 4, 11:45-11:15
MacLeod 214

This paper uses as its jumping-off point a comparison of the works of an historian and a philosopher on the question of value-free science. The historian is Robert Proctor, author of Value-Free Science? Purity and Power in Modern Knowledge. The philosopher is Hilary Putnam, author of Collapse of the Fact/Value Dichotomy and Other Essays. In each case, a major part of the argument focuses on developments within political economy, a similarity that makes the differences between the two discussions all the more interesting. Putnam's hero in Collapse of the Fact/Value Distinction is Amartya Sen, winner of a Nobel Prize for Economics. Putnam admires Sen's "capabilities" approach to welfare economics because it insists that issues of development economics and ethical concerns cannot be kept apart. Sen's refusal to separate questions of fact and value marks him as a sort of progressive revolutionary in political economy. He provides the chief case study in a book that deals largely with more abstract philosophical arguments.

The central section of Proctor's Value-Free Science? deals with the politics of neutrality in late 19th-century German social theory. Here again political economy is a chief focus of attention, but the historical perspective creates interesting contrasts with the Putnam's story. Here it is the "conservatives" who insist that ethical concerns are not separable from political economy, arguing (for example) that one must count in the costs of production not just those incurred by the entrepreneur but also the unhappiness of workers.

The contrasts between the two approaches provide a useful perspective on current debates about the possibility and desirability of value-free science.

Paul Simard Smith

Revisiting van Fraassen on Inference to the Best Explanation

June 4, 14:30-15:00

MacLeod 254

A normative study of inference determines which inferences are warranted, which are middling, and which are unwarranted. Logicians have traditionally included some ampliative inferences such as inference to the best explanation (IBE) in the class of warranted inferences. Scientific Realists have taken this generosity on the part of logicians as justification for adopting the practice of inferring that the entities posited in the best explanation are real. Van Fraassen (1980) criticizes this practice. Psillos (1996) has characterized van Fraassen's criticism as charging realists with making an unwarranted *leap of faith* when they infer that certain entities exist based on the entities involvement in the best explanation. Upon closer examination, however, van Fraassen's criticisms of IBE are more troubling for the inferential practice of realists than Psillos' account would lead us to believe. Not only is van Fraassen suggesting that the realist's use of IBE makes an unwarranted leap of faith from explanation to fact, but also that the realist's understanding of IBE is arbitrary. There are other ways to construe IBE and realists have yet to give sufficient reason to construe IBE in a way that justifies their use of that inference. First, I review Psillos' account of van Fraassen's criticisms of IBE. Second, I clarify van Fraassen's criticisms arguing that they are more forceful than Psillos' account indicates. Finally, I suggest some strategies for how realists might respond to van Fraassen's criticisms.

Patrick Slaney
Conant on Laboratory Life -or- Institutions of Bi-Polarity in Post-War Science
June 3, 14:00-14:30
MacLeod 254

Ever since Bruno Latour proclaimed, "Give me a laboratory and I will move the world" historians have drawn attention to the importance of particular kinds of spaces for understanding how science lives in culture. This paper examines how James B. Conant, President of Harvard, attempted to situate the space of the laboratory within the cold war world. During the early cold war norms for democratic life were under debate within America. Conant, an influential educator and scientist, sought to undermine the argument that politics would best be served by citizens becoming more like scientists, dispassionate and objective. At the same time, Conant wanted to legitimate the post war political order where, as a matter of fact, the fate of the body politic seemed to rest in the hands of a technocratic elite. In widely read works, Conant attacked the idea of a scientific character, understood as a normative ideal, and argued that science was credible and scientific authority legitimate because of the idealized space of the laboratory rather than because of the particular virtues of scientists. Conant described the lab as a panopticon-like space, where the collective eyes of the profession monitor all action. The normative emotional regime he built into the space of the lab allowed Conant to contest claims that scientists were good models of democratic virtue, as he complemented his account of the rigor of the lab with an account of the frequent emotional instability and excesses of lives of scientists outside of the lab.

Rachel Bryant
What if ecological communities are not individuals?
June 3, 10:00-10:30
MacLeod 254

An assumption that ecological communities are ontologically robust units underlies much of ecological theory and pervades conservation biology. We commonly understand communities as delimited by clearly defined boundaries and/or as self-regulating causal systems (Schrader Frechette and McCoy 1993, Cooper 2003, Sterelny 2006). In other words, we take ecological communities to resemble our paradigmatic biological individuals – organisms.
Symptoms of this assumption abound. Among the symptoms are ecologists' search for mechanisms of density-dependent regulation, and the view that communities are vulnerable to invasion by "alien" species. Yet recently, philosophers of biology have begun to question the assumption that communities are like individuals (Schrader Frechette and McCoy 1993, Cooper 2003, Sterelny 2006). If ecological communities are not such ontologically robust units, what might be the repercussions? Kim Sterelny (2006) argues that community ecology as we know it could not survive.
Contra Sterelny, I argue that community ecology could subsist without the assumption that ecological communities are ontologically robust units. Conservation biology, however, would be forced to transform radically.

Andrea Reichenberger

Reichenbach’s Causal Theory of Time: A Critique
June 4, 14:30-15:00
MacLeod 202

Causality is a key concept in Reichenbach’s philosophy of physics. According to Reichenbach, time order is definable by means of causality. This thesis rest upon Reichenbach’s idea of the so-called coordinate definition. To be more precise, Reichenbach uses two topological coordinative definitions for deriving time order from causality. Firstly, he defines time order in terms of possible cause: event A happens before event B if A could have caused B but B couldn’t have caused A. Secondly, he defines the concept of simultaneity on the basis of Einstein’s theory of relativity. In an Einsteinian universe, no causal influence can travel faster than the speed of light in vacuum, thus any event at A whose time of occurrence is in the open interval between t_1 and t_2 could be defined to be simultaneous with event E. Hence, it is clear that Reichenbach is more or less forced to maintain that „the causal theory of time could not be definitively established before Einstein had completed his theory of relativity“ (*The Direction of Time* 1956, p. 25). I will argue that general theory of relativity does not provide a motivation for the causal theory. On the contrary, general relativity promotes the view of space-time as a primitive entity, i.e. space-time is taken as part of the basic ontological furniture, e.g., one quantifies over space-time points, and the notion of space-time point is used to explain other notions like spatiotemporal coincidence. From this point of view the concept of causation and the concept of a localized particle, concepts which the causal theorist takes as basic, are in need of analysis.

Robert Moir
Theories, Models and Representation: Lessons from Solid State Physics
June 4, 9:00-9:30
MacLeod 214

A clear distinction between mathematical theories and models will help us understand how such models relate to the phenomena being modeled. This is so, I argue, because a deeper understanding of the phenomena is obtained not by single models on their own, but by an elucidation of the detailed interrelations between models of different levels of complexity or detail, which can simultaneously involve intertheoretic relations. There are many different accounts of the structure and role of models in the application of theory in recent literature, which characterize important features of models and how they relate to theories and the world. My considerations in this paper make distinctions that will add to these accounts and, in some cases, suggest refinements to them. I will examine models of crystalline solids, specifically metals. This is a sufficiently rich example since it involves a network of interrelated models and the simultaneous application of many physical theories. Since these models involve many kinds of idealizations, the standard view is that they do not provide explanations since the explanans are, strictly speaking, false of the phenomena under scrutiny. I will argue, however, that these models do give us physical understanding of the phenomena being investigated and, in so far as they do, they should be considered to be explanatory. Nevertheless, the way in which the meaning of various theoretical terms shifts between models places strong restrictions on how the models can be considered to be representative of the systems being modeled.

Ryan Samaroo
Can Carnap’s conventionalism be attributed to Poincaré and, if so, how?

June 5, 10:45-11:15
MacLeod 202

As a doctoral student, Carnap read Poincaré's work on the interpretation of geometric axioms as well as his work on the relationship between geometry and classical mechanics. The inspiration he took from Poincaré came to bear on the general syntactic ascent in his work of the late 1920s and early 1930s. Elements of this inspiration are also manifest in his view that questions about scientific truth and the meanings of basic theoretical terms are meaningful only insofar as they are internal to a linguistic framework that we accept conventionally.

Philosophers of physics are widely familiar with the broad strokes of Carnap's and Poincaré's conventionalist views. And it is often suggested that Carnap's stance on the free or conventional choice of a linguistic framework can be attributed to Poincaré's views on the conventional choice of a metrical framework for mechanics. It is unclear, however, precisely which elements of Carnap's conventionalism can be attributed to Poincaré. My purpose here is to clarify this.

I proceed in three stages. First, I distinguish between two sorts of conventionalism in Poincaré's work. Second, I show in detail how Carnap captures aspects of each in his syntactical reconstruction of the language of physics. Third, I cast doubt on the idea that Carnap's conventionalism can be strictly or straightforwardly attributed to Poincaré. I conclude, nonetheless, that Carnap's conventionalism embodies a central epistemological point of Poincaré's: the way we describe the world in a given language *bears* on our interpretation of the questions and observations that are consequent on that description.

Saray Ayala-Lopez
Biased Cognition and minimal cognition
June 5, 10:00-10:30
MacLeod 214

The notion of cognition within cognitive sciences is biased. Cognition is ordinarily identified with the kind of abilities typically exhibited by human beings, and its study has been done from an anthropomorphic perspective. In order to measure the cognitive status of an organism behavior, we look for the presence of these human-like processes. Recent findings in plant neurobiology (Trewavas, 2003; Baluška et al. 2006), on the one hand, and within the research on prokaryote organisms (di Primio et al. 2000; Greenspan & van Swinderen, 2004), on the other, suggest, however, that cognition can be fairly predicated of non-human (simple) organisms (Calvo, 2007; van Duijin et al., 2006). In order to develop a notion of cognition suitable for simple and not only 'elite' organisms, we need to look for the basic requirements for cognition. The field of embodied and situated cognition provides an adequate framework for this project. Orthodoxy in cognitive science assigns cognition to the internal workings of the brain. Under this new increasing view, however, embodiment and environment are constitutive of cognition (e.g. Clark, 1997; Brook, 1999; O'Regan & Noë, 2001). Here I consider two examples of complex (cognitive) behavior in prokaryotes and plants, and address the question whether the embodied and situated framework provides the necessary tools to claim that these behaviors are examples of cognition.

Sheldon Chow
Newton's Theory of Universal Gravitation, the Cause of Gravity, and the General Scholium
June 3, 16:15-16 :45
MacLeod 202

In the penultimate paragraph of the General Scholium, we get a glimpse of Newton's own reflection on the cause of gravity. But much of what Newton wrote there is cryptic, and many scholars have laboured to decipher its meaning. My work contributes to this ongoing project. In this paper, I take up the penultimate paragraph of the General Scholium with a view to illuminating (a) Newton's attitude towards his theory of universal gravitation, and (b) his attitude towards the problem of attributing a cause to gravity. I do so in two ways. First, I situate the paragraph in the context of the General Scholium as a whole. Second, I interpret the General Scholium with respect to some of Newton's other works and letters, as well as to some modern scholarly commentaries and writings. I reject the possibility that Newton believed that a "subtle spirit"—which is introduced in the final paragraph of the General Scholium and discussed in the Queries to the Opticks —is the cause of gravity; I also reject the possibility that he believed that God is the cause of gravity. I argue instead that Newton, on his own methodology, was not required to seek out its cause, and deliberately withheld from making claims about it. I conclude that the gravity's cause, for Newton, is an issue that does not bear on the truth of his theory of universal gravitation.

Susan Liepert
The problem of fish ears: Investigating animal subjectivity in late-eighteenth-century Europe
June 5, 11 :45-12 :15
MacLeod 214

Can fish hear? This question was hotly debated throughout the eighteenth century. Classical authorities had argued that fish responded to noise, and could, therefore, hear. Early eighteenth-century anatomists, however, could find no evidence that fish had organs of hearing; according to the mechanistic models of science in ascendance at this time, this meant that fish were deaf. As the tenets of vitalism gained ground in the 1730s and 40s, this opinion was attacked in its turn, and a wide variety of procedures and apparata (involving anything from home-made hand-grenades, to naked underwater bell-ringing) attempted to replicate the actual experiences of fish.

The sometimes comic history of the Enlightenment's engagement with fish ears demonstrates a more serious underlying struggle to conceptualize animal subjectivity. The lynchpin of the controversy was conflict over what role a fish's own experience of sound played in resolving the problem, and what constituted adequate evidence of this experience. My paper will contrast the epistemological focus on animal subjectivity which allowed anatomist John Hunter to resolve the issue of fish hearing, and the strategies of popular natural history, which centre on the ontological status of a particular category of lived animal experience to the practical exclusion of epistemological concerns.

Stephen Snobelen
"Alone in a garden": Newton's apple revisited and the poetics of natural philosophical inspiration
June 3, 16:45-17:15
MacLeod 202

A strong historiographical tradition exists that expresses scepticism about Isaac Newton's account of the fall of the apple and the inspirational role it played in his early thinking on gravitational physics. Late in life, Newton told a handful of friends that an early insight into universal gravitation had come to him as a young man while home from Cambridge at Woolsthorpe, when, during a reflective moment, he saw an apple fall. The earth, he concluded, drew the apple toward it just as it held the moon in orbit. Dating back to David Brewster's magisterial biography in the Victorian period, many historians have questioned the validity of Newton's recollections, suggesting that the story was either the de novo fabrication of an old man or that the incident, if true, was much less important to his physics than he later claimed. To assess whether or not the common scepticism is justified, this paper strips away the editorialising about the apple and its Edenic connotations that flourished after Newton's death and examines a wide range of evidence, including all the surviving first-hand accounts of the apple story, evidence for the existence of apple trees at Woolsthorpe Manor during Newton's youth and analogous examples of Newton resorting to gardens for inspiration. Although the apple story will likely never be proven, this paper contends that when all the available evidence is assembled, the story is more plausible than often claimed and, what is more, fits in well with what we know about Newton's personality.

Roger Stanev
HIV/AIDS Activism and the Challenges In Designing and Monitoring Clinically Relevant Trials
June 5, 11:45-12:15
MacLeod 254

The inability of expert communities to respond quickly enough to the AIDS epidemic in the 1980's spawned a credibility crisis surrounding the biomedical sciences, resulting in grassroots movements like the HIV/AIDS treatment activism (Epstein 1996). My paper focuses on changes in clinical trials brought about by this activism; most specifically, changes in pre-designated rules, such as stopping rules—when to stop a clinical trial.

Historically, antiviral drugs have required scientific testing prior to general usage. The standard method for testing has been clinical experimentation—a.k.a. clinical trials—attempts to test as scientifically rigorously as possible, the hypothesis of whether or not the new agent is more effective—as well as safer—than the placebo or standard treatment, often by randomizing subjects between test vs. control groups. Traditionally, clinical trials have been designed to restrict entry to a homogeneous group of patients, so that treatment effects could be measured more precisely; and pre-designated rules, such as stopping rules have always been planned as rigid and formal, so as to produce results as clean and complete as possible, in order to assess the evidential import of the trial outcome.

My talk looks at a case where the U.S.AIDS Clinical Trial Group decided to accept a data monitoring committee recommendation to stop a placebo-controlled trial of Zidovudine earlier than its pre-designated rules, on the grounds that it was thought unethical to continue the original trial, given the statistical significance of the observed differences, despite the challenges and problems of evidential interpretation that can arise with such change.

Travis Dumsday

Natural Kinds, Complex Essences, and the Real Difficulty Facing Scientific Essentialism

June 4, 14:30-15:00

MacLeod 214

In the ongoing debate concerning the ontology of laws, the advocates of two major theories – nomological necessity (sometimes called the Dretske-Tooley-Armstrong theory, or DTA) and scientific essentialism - seem to have reached a stalemate. Neither side is able to gain a consistent upper hand over the other in the literature. My aim is to diagnose the root of this deadlock and break it in favor of a somewhat modified essentialism. I argue that the reason why essentialism has not emerged victorious lies in a persistent ambiguity, found in both its proponents and critics, concerning the use of 'essence.' Briefly put, they speak alternatively of 'essence' as something unitary, and of 'essential characteristics,' or 'essential properties,' which latter usages imply that an essence is a complex thing made up of multiple features. This ambiguity, scarcely noticed in the literature, is more important than it might appear; for as soon as essences are conceived as compounds of distinct features, features which have no clear unifying element, essentialism loses its explanatory power over and against the conception of laws propounded by the DTA school. In order to rectify this problem, one must clarify how an essence can be conceived as complex yet unified; that is, one must specify the source of unity of an essence. This requires stepping back from the laws debate in philosophy of science and into the ontology of essence, and I briefly outline how this issue can be handled in a way favourable to essentialism.

Tracy Finn

Autism Spectrum Disorders and Theory of Mind: Developmental Evidence and Philosophical Implications

June 3, 14:30-15:00

MacLeod 202

I discuss the current debate concerning theory of mind, how this problem relates to autism research, and what this research reveals about how the prediction and explanation of behaviour is accomplished. In the philosophical theory of mind literature, there are two major views about how this capacity works: theory-theory and simulation theory. Theory-theory holds that we exploit an internally stored set of generalizations and principles, which we use to theorize about the mental states of others, and then generate predictions and explanations of behaviour. Simulation theory states that we explain and predict behaviour by simulating the mental states of others, and use our own decision-making systems to model potential action scenarios that cause the observed behaviour.

I argue that simulation theory is the more tenable position, based on evidence gleaned from autism research. Cognitive and developmental autism research contrasts the development of social cognition in both normally developing children, and children with autism spectrum disorders. This evidence is considered by theorists on both sides of this debate, but often in a piecemeal manner, which results in a stalemate between these two views of theory of mind. However, when all the relevant developmental evidence is compiled and analyzed together, it strongly supports simulation theory. I will discuss this evidence, why simulation theory is the more tenable view, and the implications of this for the longstanding philosophical problem concerning folk psychological explanations of behaviour.

Trevor Pearce
From the Police to the Population – Hacking, Foucault, and Ecology
June 3, 11:45-12:15
MacLeod 254

In the preface to *Historical Ontology* (2002), Ian Hacking mentions two ongoing projects that he hopes to develop in future research: ‘making up people’ and ‘styles of reasoning’. These influential projects both began in the early 1980s, but their roots stretch back to the 1970s and his first encounter with the work of Michel Foucault.

This paper has two parts. In the first, I will demonstrate that Hacking’s ‘styles of reasoning’ project can be traced to Foucault’s *savoir*-based writings of the 1960s, and that his ‘making up people’ project can be traced to Foucault’s *pouvoir*-based writings of the 1970s. I will also argue that Hacking’s distinction between indifferent and interactive kinds, which corresponds to a distinction between the natural and human sciences, implies that *pouvoir*, in Foucault’s sense, is not relevant to the practice of natural science.

In the second part of the paper, I will investigate case studies from the history of ecology, showing that Foucault’s combined *pouvoir-savoir* methodology can also be applied to the natural sciences. This is not an account of how political power corrupts natural science; I am merely suggesting, *contra* Hacking’s implication, that the study of changing modes of organization – changing technologies of power in Foucault’s sense – is also relevant to natural sciences like ecology.

HYLARIE KOCHIRAS
NEWTON'S SUBSTANCE COUNTING PROBLEM
JUNE 3, 15:45-16:15
MACLEOD 202

Can two things be in the same place at the same time? Newton thinks they might if they are substances of different kinds, and I argue that this is the reason that Newton cannot solve his problem about gravity's physical cause. His gravitational theory raises the spectre of matter acting at a distance, with sun and planets attracting one another across empty space. Since he considers unmediated action between material bodies absurd, he hopes to discover some immaterial substance, such as an aether, that might fill space and possess active powers to produce gravitational attraction. Yet any effort to locate such a medium or to associate active properties with it rather than with matter founders upon what I call the 'Substance Counting Problem'. If an immaterial substance could co-occupy the places occupied by material bodies, then we cannot use material bodies to try to confine the substance. Thus we cannot determine how many substances are present in a given location, or even if we knew, we would have no basis for associating active powers of attraction with an immaterial medium rather than with matter.

David Meshoulam

Men in the Middle: Biographies in Early 20th-century American Science Textbooks

June 3, 10:45-11:15
MacLeod 214

As attendance to American high schools surged in the late 19th century, educators implemented new approaches to teaching science that they hoped would engage a diverse population of students and help stem declining enrollments in science courses. Among the many reforms, which included increased attention to student interest and hands-on laboratory activities, were efforts to teach students about the lives of scientists. This biographical and historical method, backed by such prominent educators as John Dewey and G. Stanley Hall, would teach students about the cultural and social import of science.

By the early years of the 1900s, secondary science school textbooks heeded the suggestions of reformers and printed short biographies as a solution to the perceived shortcomings of high-school pedagogy. Physics was the first discipline to incorporate this method, but its popularity quickly spread to chemistry and biology. Vignettes of scientists, often short and unconnected to the material found in the text, included little more than an illustration of the scientist and a list of his major contributions. Science professors and leaders in the field of education saw these stories as a way to engage students in the scientific material, shape proper thinking, infuse cultural values, and teach the "scientific method." Though it remains difficult to measure the success of their efforts, examining biographies and the rhetoric surrounding their implementation demonstrates the flexibility of science and how its image was negotiated between scientists, educators, and the public.

Michelle Hoffman

“Scientific Stimulus of a National Character”: Ontario High School Science in International Context, 1890-1910

June 3, 11:15-11:45
MacLeod 214

This paper explores the international influences on Ontario’s secondary science curriculum at the turn of the twentieth century. Ontario’s high school science curriculum underwent significant changes during the period from 1880-1910: empirical teaching methods were gradually adopted; most high schools acquired at least rudimentary laboratory apparatus for chemistry and physics; and educators increasingly emphasized teaching science by reference to applications that were interesting and relevant. The move toward more practical high school curricula became an international trend in 1890s. The Imperial Conference in Berlin, the Ribot Commission in

France, the Bryce Commission in England, and the Committee of Ten in the United States all took place during this decade, and unanimously called for high school curricula more relevant to students' everyday experiences.

From 1889-1909, Ontario educators embarked on no fewer than six foreign tours to study technical schools in the United States and Europe. Yet not all outside developments were welcomed with enthusiasm. As the New Education movement gained momentum in the U.S., Ontario school officials were often wary of the "liberal" ideas floating around American curricular reforms, but remained nonetheless in the thrall of thinkers like John Dewey and G. Stanley Hall – both of whom eventually visited Toronto. Efforts to reform the school science curriculum in Ontario, I argue, must be understood in the context of local tensions between imperialism and continentalism. These cultural impulses competed for currency throughout Canada at the turn of the twentieth century, an era when schools were looked to as instruments of national identity-building and economic change.

Adam R. Shapiro

What did Scopes really do to high school biology?: Textbook politics and the evolution of science education in the early twentieth century

June 3, 11:45-12:15

MacLeod 214

Scholars have long debated the extent to which the 1925 Scopes antievolution trial influenced biology textbooks. Most agree that, in the short term, evolution was removed from the books in direct response to the trial. While this conclusion is valid, is it incomplete for several reasons.

Most studies have been restricted to examining only the content of the published textbooks. This approach treats the textbook as the physical trace of some generalized opinion of "publishers" or textbook "authors," ignoring the fact that the production of a textbook is a process that requires the negotiation of several parties: authors, editors, sales representatives, sources of external feedback, and textbook adopters and consumers. Examining only textbook content obscures the internal tensions and negotiations that defined "response" to the trial.

Textbooks do not emerge instantaneously, and many books published in the aftermath of Scopes began development prior to the trial. This limits the use of the trial as a sharply defined turning point in biology textbook history, and reveals that many publishers were already responding to the antievolution movement prior to the trial.

Accounts of changes to biology textbooks need to take account of new sales and adoption strategies that affected textbook consumption. The Scopes trial proved to many the insufficiency of some marketing practices in parts of the country in which textbooks were regulated at state level.

This paper examines these issues, arguing for deeper understanding of Scopes' impact on science education, and reassessing the uses of textbooks in science studies.

Robert Smith

W.H. McCrea and the Remaking of Cosmology in the 1930s

June 4, 9:00-9:30

MacLeod 202

In the late 1920s and early 1930s, various investigators established the concept of the expanding universe. One result was a burst of interest in developing new cosmological models and theories, including ones that were not based on Einstein's theory of general relativity. Here I will examine the researches of one of those to enter this novel area, William Hunter McCrea (1904-1999). McCrea was one of the leading figures in British astronomy and cosmology in the twentieth century. As we will see, he made major contributions in the 1930s to what has been described as `cosmic physics,' that is, the integration of cosmology and physics. McCrea was one of a relatively small number of active British cosmologists in the 1930s, but like some others in this group he had very close ties to both the University of Edinburgh and the University of Cambridge. I will argue that these institutional links very strongly influenced McCrea's career as a cosmologist and the directions of his research as well as the methods and techniques he applied. I will also discuss how his philosophical and religious convictions helped shape his cosmological researches.

George Gale
"I told them 'show me a fossil, and I'll give it up' " — Bondi, 1988
June 4, 9:30-10:00
MacLeod 202

What made its adherents give up the Steady State Cosmological Theory? According to the received view, the 1965 discovery by Penzias and Wilson of the cosmic 3°K background radiation, interpreted by Dicke et al to be a relic of the Big Bang, provided literal smoking gun evidence against the Steady State. Yet, how can this view be correct, when three of the main proponents of the theory, including one of its founders, gave up the theory for reasons completely unconnected to the 3°K evidence? Herman Bondi, William McCrea and Dennis Sciama all gave up the Steady State theory, but each for his own reasons, not including the relic radiation evidence. In the paper which follows I will describe and discuss the reasons proposed by each of these scientists, attempting to reveal why it was to them more compelling than the 3°K evidence. Finally, I will make some suggestions why the received view has settled on the relic radiation discovery as the death knell of the Steady State theory.

Dylan Gault
20th Century Cosmology Is Not Over
June 4, 10:00-10:30
MacLeod 202

At the end of the 20th century, astronomers recorded observations of distant supernovae that provided evidence for a positive value of the relativistic parameter known as the cosmological constant. Prior to the discovery, the prevailing opinion within the cosmological community, including those who performed the supernovae observations, seemed to be that the value of the constant was zero; essentially, it was believed that the cosmological constant was not really a parameter of standard cosmological theory. Following this discovery, many in the cosmological community viewed the discovery as the impetus of a revolution in cosmology; yet this revolution did little, if anything, to harm the pre-existing scientific framework of cosmological theory. The addition of the cosmological constant does not change the core claims of the standard cosmological model of the 20th century as identified by cosmologists since the discovery of the cosmic background radiation. Indeed, I argue, in establishing the non-zero value of the parameter, cosmologists provided more evidence for the dominant theory of cosmology. The supernovae observations, along with other methods that measure the cosmological constant, grant us better, and agreeing, measurements of the parameters of the standard cosmological model. In this sense, though the discovery marks a significant change in the practice of cosmology between the 20th and 21st centuries, the dominant cosmological theory of the 20th century has not been abandoned.

Leon Antonio Rocha

The Many Faces of 'Mr. Science' in China : The 'Science and Philosophy of Life Debate', 1923-24

June 4, 10:00-10:30

MacLeod 254

Just what is this thing called science? What sort of questions can it answer? Are there limits to what it can accomplish? Most importantly, can "Mr Science" supply us with a universal, ethical outlook of life that brings power and prosperity, and simultaneously peace and harmony? Such were the issues central to the sprawling "Science and Philosophy of Life" debate, which appeared in the supplements and magazines in China between March 1923 and December 1924.

On one side of this battle, there was Zhang Junmai (Carsun Chang, 1887-1969), a prominent philosopher. Having witnessed the "spiritual bankruptcy" of Europe after the first World War, and inspired by the Lebensphilosophie of Rudolf Eucken, Bergson and Hans Driesch, Zhang Junmai believed that science, though a useful instrument for solving practical problems, could not tell us how to live a good life. On the other side was Ding Wenjiang (1888-1936), a geologist trained at Glasgow, who embraced the thought of Ernst Mach, and branded Zhang Junmai and his supporters as "metaphysical demons". Others participating in the debate contributed perspectives influenced by Dewey or Marxism of various flavours. In the end, the self-proclaimed "defenders of science" declared victory – though they seemed to have won by vitriol, not reason.

My paper will shed light on this complex debate that continued to cause ripples in the intellectual field in the 1920s. It also discusses the networks and connections of the protagonists, as well as the rhetoric and dynamics of these intellectual skirmishes in Republican China.

Jeff Kochan

Heidegger and the Historiography of the Experiment

June 4, 10:45-11:15

MacLeod 254

A key concept in Heidegger's philosophy of science is *mathesis*, or 'the mathematical.' Heidegger developed this concept through a discussion of 17th-century experimentation. *Mathesis* names the *a priori* in experimental practice. Heidegger argued that early-modern experiments were constrained by an emergent human disposition to construe the world *a priori* in mechanico-mathematical terms.

Heidegger's analysis is potentially challenged by Steven Shapin, who argues for a consequential distinction between early-modern mathematical and experimental cultures. In contrast to mathematicians, experimentalists eschewed values of exactitude and certainty. Using Robert Boyle as his exemplar, Shapin contends that early-modern experimental practice included the deliberate repudiation of key features of mathematical culture.
Yet Shapin also points to a tension in Boyle's attitude towards mathematics. Although he rejected mathematical certainty, Boyle nevertheless endorsed the discipline of mathematics. This tension can be resolved by drawing on Heidegger's contrast between *mathesis* and mathematics. The latter is simply a special case of the former. Heidegger argued that although numerical practice is the clearest expression of *mathesis*, it is inessential to the latter's a priorism. While Boyle eschewed numerical exactitude, his mechanical philosophy, or 'corpuscularism,' placed an inflexible *a priori* constraint on, and thereby limited the investigative range open to, early-modern experimentalists. Boyle's corpuscularism was thus a localised instance of Heideggerian *mathesis*. Understanding it in this way helps to explain how Boyle's experimental practice fit hand in glove with the Royal Society's mission to promote 'Physico-Mathematicall-Experimentall Learning.'

Mielle Chandler
Beneath the Politics of Nature: Conceptualizing Gestationality
June 4, 11:15-11:45
MacLeod 254

This paper reformulates Emmanuel Lévinas' elaboration of ethics through the theme of gestation in order to illustrate how Bruno Latour's politicization of nature suppresses the ethical dimensions of the lifeworld. Lévinas understands ethics as facilitating the potentials of 'others' through material provision. I argue not only that such 'socio-material' facilitation arises from and is predicated on both the biological gestation entailed in reproduction and the developmental systems within which entities are constituted, but also that the significance of ethics must be conceptualized within the context of the gestational aspects of the lifeworld. This paper thus seeks to make evident the effectuations that underlie the very possibility of politicizing nature.

While Latour relies on a particular ontological model, the political subject, in order to fulfil his project of inviting a provisional selection of non-human entities into the realm of existence *qua* political recognizability, Lévinas' ethics remain ontologically open. But if Latour's foreclosure is ontological, Lévinas' is biological. A Lévinasian critique of Latour thus requires a Latourian critique of Lévinas, one that counters Lévinas' circumscription of ethics to human entities and his abandoning of all non-human matter to a political state of nature devoid of ethical significance. A retheorization of ethics as starting from the gestational mediums of the lifeworld entails an unseating of ontology, of presence, existence, and political voice as the sites of significance. What this retheorization calls for, rather, is a shift of our attention to the capacities for infinite potential held by the mediums in which life is steeped.

Patrick McGivern
Canguilhem on Health, Disease, and Medical Science
June 4, 11:45-12:15
MacLeod 254

In *The Normal and the Pathological*, Georges Canguilhem argues against any account of health based on statistical normalcy. Rather than being understood as a state that is typical for an organism of a particular kind, he argues that health should be understood in terms of an 'individual norm' that closely depends on an organism's environmental context or *milieu*. Canguilhem's account of the concept of health is also essentially *value* based: an individual organism is healthy if it can maintain its 'vital values' within its environment. Canguilhem argues that these two features make medical science – i.e., a science of health and diseases – problematic. If science is concerned with discovering general laws, and if health can only be understood with respect to individuals in particular contexts, then there will be no useful generalizations for medical science to be about. And if science is concerned with giving objective descriptions of the world, and if health is essentially value based, then again the idea of a science of health seems to contradict the concept of health itself. In this paper, I examine Canguilhem's attempt to develop an objective account of vital values to address these two worries. I then argue that a solution to both worries can be found by examining how the concepts of health and disease can apply to individuals at different levels of organization, so that we can distinguish between healthy (or diseased) organisms, organs, cells, or even genes.

Howard H. Chiang

"Rethinking 'Style' for Historians and Philosophers of Science: A Converging Perspective from Sexuality, Translation, and East Asian Studies"

June 4, 9:00-9:30

MacLeod 254

Historians and philosophers of science have furnished an array of theoretical-historiographical terms to describe different systems of knowledge and emphasize the discontinuities among them. Some of the most famous include Thomas Kuhn's "paradigm," Michel Foucault's "episteme," and the notion of "scientific style of reasoning" recently developed by Ian Hacking and refined by Arnold Davidson. This paper takes up this theoretical-historiographical thread, and assesses the values and limitations of the notion of "style" for the historical and philosophical study of science. Specifically, offering a converging perspective from sexuality, translation, and East Asian studies, this paper argues that the heretofore ways in which historians and philosophers of science have used the notion of "style" are severely restricted in terms of its mere applicability to the intellectual history of Western science. The particular example of the translation of "homosexuality" into Chinese during the May Fourth era reveals that when scholars broaden their geo-political horizon to appreciate the historical developments (of modes of thought about sexuality) in non-Western parts of the world, the notion of "style" has the potential of carrying a much more dynamic conceptual weight, and the specific idea of a "scientific style of reasoning" can be much more limited than has been typically assumed.

This paper will first and foremost show how the Western psychiatric style of reasoning about homosexuality was transformed into a Chinese nationalistic style of argumentation about same-sex desire, as Chinese public intellectuals introduced and translated the European discourse of sexology during the early Republican period and beyond. The paper then engages briefly with the historiography of "national styles" of science and comments on the successes and weaknesses of how "style" operates there in light of the rapidly evolving historiography of East Asian nationalism. Learning from the kind of historical investigation based on which these theoretical-historiographical insights could be drawn, the paper ends with some concluding remarks on the limitations of "social histories from below" and the under-appreciated importance of "epistemological histories of possibilities and comprehensibility."

David Luesink

Medicine and philology leads Science: The case of the standardization of scientific terminology in early twentieth century China

June 4, 9:30-10:00

MacLeod 254

This paper will explore the relationship between the leading science organization of the Republican period, the Science Society of China (SSC) and the *Yixue mingci shencha hui* (Medical Terms Investigation Committee) in the work of standardizing scientific terminology. It was missionary and Chinese overseas-trained physicians, educators, social reformers and philologists who first seriously took up the task of standardization, while members of the Science Society of China finished their degrees at elite American universities and published their journal remotely. In 1918, the founding members of the SSC returned to their Shanghai publishing base and the standardization committee opened up from a focus on medical and chemical standardization to embrace Chinese terminology for all fields of science and technology. Yet medicine and its "cognate sciences" still predominated. While scholars in China and the U.S. have argued for the centrality of the SSC in establishing the authority of science in China, my evidence demonstrates that the SSC "translated" the work of the MTIC to their own larger goals, thus eliding the important cooperative work of missionaries, physicians and philologists in establishing a central discursive space for science in twentieth century China.
