Canadian Society for History and Philosophy of Science 2012/
Société canadienne d'histoire et de philosophie des sciences 2012

Abstracts/Résumés

Stillman Drake Lecture/Conférence Stillman Drake
Andrew Hodges, Wadham College, Oxford University

What does Alan Turing tell us about the history of science?

In 1936 Alan Turing published his famous paper 'On Computable Numbers....'. It created the concept of mathematical computability, defined the idea of a universal machine, and gave a new description of mental operations. Turing went on to play a leading part in wartime cryptography, and then to design an electronic computer, found the theory of Artificial Intelligence, and create a new branch of mathematical biology. While surveying the narrative of Turing's life, in this his centennial year, I will discuss his own observations about the nature of science and the place of the individual scientist. I will also highlight the difficulties of charting the progress of scientific ideas, illustrated by the question of how the Church-Turing thesis was originally formulated, and by the question of the origin of the digital computer.

Amoretti, Christina & Nicla Vassallo

Is Gender-Specific Medicine Epistemologically Well-Grounded? **(I.1B)**

This paper analyzes a recently developed branch of medicine, that is, gender-specific medicine, in order to assess whether it is epistemologically well-grounded. Even if gender-specific medicine has some important merits, we think that it still needs to be discussed in greater detail by examining the role of women as *subjects* of knowledge and as *objects* of knowledge in medicine. On the one hand, it is important to establish whether women physicians have contributed or may be able to contribute to ensure more rational, more objective medical hypotheses. On the other, we should clarify: (i) whether human beings really differ biologically and/or socially in relation to their sex and/or gender; (ii) whether these alleged differences positively influence the diagnosis, prognosis, and therapies of a disease; and (iii) whether these alleged differences affect how the patient is medically treated by his/her own physician. In discussing these cases, we would like to prove not only that the two concepts of sex and gender cannot be detached from others such as race, ethnos, social class, age, and religion, but also that further empirical research is still necessary to evaluate both women's role in medicine and the effectiveness of gender-specific medicine. As a conclusion, we shall prove that gender-specific medicine may be weakened by two flaws. First, it still embraces, explicitly or not, the stereotypical dichotomy between male/female, man/woman; second, it may support an intrinsic dichotomy between male/female, man/woman, and eventually strengthen old sex and/or gender based prejudices.

Anshan, Micah

A New Approach to Niels Bohr's Philosophy of Complementarity **(II.3A)**

Historians of quantum physics have generally treated Niels Bohr's philosophy of complementarity as pertaining only to physics. This account, which I call the singularist approach, fails to take Bohr's writings on complementarity in biology, psychology, epistemology and politics seriously. A thorough biographical understanding of Bohr supports the idea that he cared deeply about instances of complementarity beyond physics, problematizing claims by Beller that these examples were either pedagogical tools or propaganda meant to convince the world that his interpretation (the 'Copenhagen Interpretation) was correct. The myth of the Copenhagen Interpretation in the historiography of quantum physics reinforces the singularist approach as the academic focus has been on debating the nature of said interpretation rather than the specific principles that make it up. It is assumed that complementarity applies only to physics and this assumption is never challenged because the debate is centred on the Copenhagen dilemma—what does it mean and who is responsible for its creation? In the last decade commentators such as Howard have challenged the usual story of the Copenhagen Interpretation and shifted the 'blame' from Bohr to Heisenberg. Yet his criticisms continue to treat complementarity in an incomplete and thus unsatisfactory way. A full appreciation of Bohr's philosophy requires a new approach—the process approach—that takes Bohr's personal goals of cross-cultural and interdisciplinary co-operation seriously in order to understand complementarity in a way more amenable to his vision. Despite the value of previous Bohrian historiography in understanding complementarity as it applies to physics, this new approach will bring to light a greater understanding of Bohr's complex and far-reaching philosophy.

Aray, Basak

Logical Empiricism's Political Perspective on Language **(II.3C)**

From the political standpoint of the logical empiricist philosophy of language, absolutism and disconnection from the everyday world which characterize the metaphysics were intellectual instruments for maintaining social segregations. The Vienna Circle adopted verificationism, intersubjectivity and empirical reduction as cognitive values in favour of equality and accessibility, thus excluding academic verbalism and sectarianism associated to metaphysical speech. The empiricist criterion of verification was thought to provide a common basis for universal communication in order to overcome the splitting of the intellectual production in closed specialities and separate schools of thought with no possibility of cooperation. I will show how Neurath's consistent use of the visual education for the popularisation of social statistics via adult education illustrates the empowering motivation of logical empiricism. His picture language for the visualisation of statistics (ISOTYPE) shares the principles of consistency, standardization and empirical reduction with international auxiliary languages like Basic English and Interglossa, to the teaching of which Neurath contributed by providing visual material. A quick look at Ogden's reading of Bentham and Basic's structure will show the same empiricist tendency. Vertical translation from abstract language to the physicalist language of everyday experience facilitates the teaching of complex information (an especially useful task for socially disadvantaged people with a weak instructional background) while simultaneously helping the deconstruction of concepts. Therefore we should evaluate logical empiricism through its critical power and its pedagogical utility, rather than as a mere philosophical thesis on language.

Arthur, Richard T.W.

Leibniz on the Relativity of Motion **(III.1C)**

Commentators have noted several difficulties with Leibniz's position on the relativity of motion. First, his claim that the "equivalence of hypotheses" (about which of several moving bodies is actually moving) applies universally, is in conflict with the absoluteness of acceleration, especially rotational motion. Second, it is hard to reconcile his upholding of the universal relativity of motion with his concession to Newton that there is "a difference between an absolute true motion of a body, and a mere relative change of its situation with respect to another body". Third, it is alleged that in order to take inertial motion as unproblematic, he must be assuming the equivalent (in spacetime terms) of a good deal more structure than would follow from his relational theories of space, time and motion alone.

In this paper I attempt a reconstruction of Leibniz's position on its own terms, relating it to his characterization of motion as a change of situation, to his position on relations, to his doctrine that "there are no purely extrinsic denominations", and to his philosophy of force. My conclusion is that his position on the relativity of motion, although not free of difficulties, is a good deal more consistent than the usual criticisms suggest.

Barker, Gillian and Eric Desjardins

Thinking Outside the Mouse: Human Immunology, Animal Models and Organism-Environment Interactions. **(I.1A)**

Several recent opinion articles in *Nature Immunology* have expressed concern about the lack of clinical outcomes in human immunology. Human immunologists have come to realize that though we have an increasing body of knowledge about the immunology of the mouse, little of this knowledge has translated into clinical outcomes for humans. Some even suggest that we don't have a good understanding of what normal human immunity is. This lack of basic knowledge about human immunity and the poverty of clinical outcomes has triggered discussion of foundational issues in the scientific community, pointing at the inadequacy of the mouse as an animal model for studying human immunology. This raises the more general issue of what makes a good animal model. Typically, the adequacy of a given model organism is assessed in terms of similarity in molecular causal pathways between the model and the target organism. This approach is only partially correct, and the case of human immunology provides an excellent illustration some of its limitations. What this case shows is that the failure of inference in research employing animal models can result not only from differences in causal molecular pathways, but from differences in how the model and the target organism interact with (and adapt to) their respective natural environments. We argue that in order to assess the adequacy of a model organism for studying human immunity it is necessary to "think outside the mouse."

Bedford, Riiko

Dr. Emily Stowe and the founding of the Women's Medical College (1883): Negotiating a Place for Women in 19th-century Canadian Medicine **(III.3A)**

Dr. Emily Stowe (1831-1903) was one of Canada's first female physicians and an active suffragist; alongside her career as a physician, she was also engaged in the struggle for women's equality through her activities with the Toronto Women's Literary Club, a women's suffrage organized she founded in 1876. In 1883, Stowe's feminist activities coincided with her career as a physician with her role in the establishment of the Women's Medical College in Toronto (which would later become the Women's College Hospital). This paper examines the dialogue that takes place between Stowe and her detractors concerning, specifically, the importance of the inclusion of women in an emerging medical profession, and, more broadly, the role of women in society. Material will be drawn from meetings and correspondences concerning the establishment of the Medical College, from the extensive media coverage of an 1879 trial in which she was intimately involved in her role as a physician, and from her documented activities as the leader of the Toronto Women's Literary Club. Stowe's efforts to secure a place for women in medicine were carried out during a period when the medical profession was beginning to cohere, and when women were starting to gain certain grounds in their efforts to secure equal rights and opportunities. This particular episode is therefore revealing of the broader trend of the institutionalization and professionalization of medicine in Canada, and of the uncertain role of women in this process.

Belanger, Christopher

Must Lange's Laws of Nature Be Complete?' **(I.3C)**

It is commonly assumed that some regularities in nature are law-like, which usually means (at least) that they play special roles in our reasoning, explanations, and scientific theories. Marc Lange has recently proposed a novel account of the laws of nature, wherein to be a law is to be a member of a particular class of facts distinguished by their validity under certain counterfactual assumptions. Lange has argued that such laws must be "complete," which can be taken roughly to mean that the laws have no gaps in them, or leave no events uncovered.

In this talk I will consider what, if any, notion of nomic completeness is entailed by Lange's account. I will examine one of Lange's arguments for nomic completeness, and explain why I think it fails. My major positive claim is that Lange's laws of nature will be complete in a very restricted sense, which I have called "concrete completeness," but that there is no inconsistency in their being incomplete in other, possibly substantial, ways. I argue that whether Lange's laws are complete or not depends on certain facts about counterfactuals which are external to his theory.

Benétreau-Dupin, Yann

Methodological Implications of the Uniqueness of the Universe for Modern Cosmology **(I.3C)**

In his comprehensive examination of the issues in the philosophy of cosmology, George Ellis,1 echoing Milton Munitz,2 asserts that the concept of 'laws of physics' cannot apply to one object only, and therefore to the universe as an object of scientific study. I first examine responses to

this claim3 that articulate how this assertion rests on a problematic conception of physical laws. In particular, Munitz's and Ellis's requirement for laws to apply to a multiplicity of instances is questionable as it implies that the phenomena, if they are viewed as 'instances' of a law, can be completely captured by them. I then argue that it is the strict distinction between physical laws and physical facts that vanishes and that neither of them can pretend to completely express phenomena.

Furthermore, the traditional distinction between laws and initial conditions is problematic in cosmology. This distinction demarcates the necessary and the accidental, but it is questionable that these categories apply to the existence and properties of the universe.

I thus claim that the uniqueness of the universe does not in itself constitute an unsurmountable challenge to the possibility that it be the subject of laws. If it does represent an empirical and conceptual challenge to the scientific study of the universe - but not an impossibility in principle - it is in part because it forces us to recognize the conventional element that we otherwise overlook in the distinctions that usually frame our discourses on physical laws and phenomena.

Bland, Steven

Carnap, Quine, and the Dogma of Reductionism **(I.3B)**

From Quine's perspective, Carnap is engaged in an attempt to provide an empirical foundation for scientific theories by demonstrating that all theoretical terms and sentences can be reduced to observation terms and sentences. Furthermore, he claims that this philosophical program must fail, in large part because epistemological holism and the underdetermination of theory by observation have undermined the dogma of reductionism. In its place, Quine insists that we should substitute a thoroughly naturalized discipline which seeks to answer philosophical questions from within the empirical sciences. On the recently popular deflationary interpretation of Carnap, Quine has mistakenly attributed to Carnap the goal of advancing philosophical theses, including empirical foundationalism, when in fact Carnap is interested only in producing formal languages for possible use within the sciences. This project is left completely untouched by Quine's criticisms of reductionism. This paper will argue that Quine does indeed misunderstand Carnap's philosophical project, yet Carnap's overarching aim is the same as Quine's: to produce a scientific discipline within which philosophical questions can be properly answered. For Carnap, however, this must be a formal rather than an empirical discipline. To establish that this is the case, he must demonstrate that philosophical questions lack factual content, and it is for this reason that his empiricist criterion of meaning is essential to his philosophical project, contrary to the deflationary reading. Nevertheless, Quine's criticism is ultimately unsuccessful because Carnap's mature criterion of empirical significance is motivated in part by epistemological holism and the underdetermination of theory by observation.

Boese, Anthony

Putting Meat on Lockean Bones: An Argument for a Thick Lockean Thesis of Belief **(II.3B)**

The Lockean, or Threshold, Thesis of belief falls within an epistemic tradition that seeks to subsume the binary account of belief – one believes or does not—within the degrees of belief account—one holds a belief attitude that falls somewhere on a densely ordered spectrum. The Lockean Thesis maintains that if the attitude falls above the given threshold line then it is belief, and if it falls below then it is not. While appealing at first glance, the Lockean Thesis suffers from some crippling flaws. Principle among these is the issues that arise due to conflict with the conjunction rule as commonly illustrated in the Lottery and Preface Paradoxes. While abandoning the conjunction rule is an effective and tenable solvency, it falls short of fully saving the Lockean position. However, I will argue that appealing to thick confidence and forming a Thick Lockean Thesis is a simple and natural step that can finish the rescue of the Lockean Thesis.

First, I introduce the Lockean Thesis. Next, I introduce common complaints about it that attempt to prove it untenable. I will highlight the friction between the Lockean Thesis and the Conjunction Rule, and concerns stemming from the robustness of belief. Then, I will denfend the Lockean Thesis, showing how it can survive the Paradox criticisms by abandoning the conjunction rule. Moreover, I will present a second step in saving the Lockean Thesis—appealing to thick confidence and framing a Thick Lockean Thesis that avoids issues of robustness.

Bolinska, Agnes

Towards an account of scientific representation **(III.2C)**

A widespread view in the modeling literature holds that an account of mere scientific representation needs to be developed before we can determine what makes a representation true or accurate. In other words, an account of mere representation is conceptually prior to an account of true or accurate representation. I call this the *mere representation priority* (MRP) approach and argue that it is misguided. I show that this approach makes two critical assumptions: (i) that scientific representations should primarily be classified along a dimension of truth or accuracy and (ii) that this classification scheme should be adopted as a guiding principle for coming up with an account of scientific representation, the primary question being: "In virtue of what is *x* a scientific representation of *y, no matter how inaccurate*?" I challenge both of these assumptions. I argue that the crucial classification of scientific representations is in terms of how *successful* they are and that this does not amount to being more or less accurate. Moreover, it is more straightforward to begin by determining what it takes for something to qualify as a maximally successful representation and then use this to guide the development of a more general account covering the whole gamut of representation, not the other way around, as proponents of MRP suggest.

Bouchard, Frédéric

How Institutional Norms Offer a Rational Justification for the Deference to Scientific Experts **(II.2A)**

Recent public debates about climate change, genetically modified crops, science curriculum and other issues have both strengthened the necessity to appeal to some form of scientific expertise while at the same time relativizing the epistemic authority of its claims. Various social scientists, mostly in the field of STS (science and technology studies), have offered diagnoses of this phenomenon: their contribution has focused on describing how expertise is marshalled, not why it is legitimate to invoke it. Philosophy has offered rare but significant contributions to this debate. We could group these contributions in two broad families: some (e.g. Hardwig) offer moral underpinnings for expertise while others (e.g. Goldman) offer a detailed epistemic analysis of the types of epistemic asymmetry in expertise relationships. They are both right in significant respects but neither show how the heterogeneity of the types of objects for which expertise is invoked affects the epistemic authority of expertise. Using Merton's work as an inspiration, I will show how the types of values involved in scientific practice both buttress its authority concerning certain objects and weaken it for others. I will explain why the authority of scientific expertise has to be contextualized relative to the risk involved in the process it examines. My approach differs from that of other philosophers who have looked at risk in that I will argue that the means of strengthening the authority of scientific experts on many topics also inadvertently weaken that same authority on risk-related issues. I conclude by a call to arms for philosophers of science to take interest in the integrity of the scientific values and the institutions that support them.

Brigandt, Ingo

Evolutionary Developmental Biology and the Limits of Philosophical Accounts of Mechanistic Explanation **(I.3A)**

Evolutionary developmental biology (evo-devo) is an integrative approach which develops explanations of morphological evolution that combine resources from different fields and appeal to entities on several levels of organismal organization. Philosophical accounts of mechanistic explanation have been advocated as an alternative to law and equation-based explanations; and a clear virtue of this philosophical approach is that it can cover multilevel and multifield explanations. Even though explanations in evo-devo involve developmental mechanisms, my talk discusses several ways in which current philosophical accounts of mechanistic explanation are ill-suited to capture aspects of explanations important to evo-devo.

Evolutionary developmental biology explains (and explains in terms of) the generation of completely novel morphological features both during developmental time and especially in the course of evolution, a phenomenon on which philosophical accounts of mechanisms have been silent. Mechanistic explanations as construed by philosophers are qualitative (in terms of the interactions of parts), while for some evo-devo accounts quantitative changes of molecular entities (modelled by mathematical equations) are to be taken into account. Philosophers' conception of mechanisms as linear cannot accommodate the importance of feedback loops for some developmental processes. Developmental robustness, phenotypic plasticity, and modularity are related features of organismal systems that are of utmost importance to evo-devo, as they account for what makes organisms evolvable and capable of producing novel morphological

structures. Robustness is an organism's ability to develop a viable and functional phenotype in the face of environmental disturbances and genetic modification, a property which philosophical accounts of mechanistic explanation cannot capture.

Brown, James Robert

***A Muse of Fire* (I.1C)**

Dennett and others have rejected the use of thought experiments. This talk will be an account of Dennett's reasons followed by a rejection of them. In short, he has no case against the use of thought experiments. But the reasons for his failure are quite interesting in their own right.

Corriveau-Dussault, Antoine

***On the Normativity and Holism of Ecosystem Health* (I.2A)**

As a norm for environmental conservation, the concept of ecosystem health offers resources to account for two important facts about the ecological world: 1) that it is changing, and 2) that human beings are part of (and not isolated from) it. By accounting for these facts, it goes beyond both the balance of nature paradigm in ecological science and the wilderness approach to environmental policy. Unfortunately, however, the concept has not yet been articulated in a satisfactory way, leading many ecological scientists and philosophers to be skeptical about its scientific appropriateness. Because the concept is normative, indicating the good state(s) of ecosystems, it has been thought to inescapably involve ethical values and so to lie beyond the scope of scientific assessment. Moreover, because ecosystem health is a property claimed to exist at the ecosystem level, it has been said to presuppose a scientifically discredited organismic ontology of ecosystems.

My presentation is aimed at rehabilitating the concept of ecosystem health by clarifying the type of normativity and holism that it involves. I will attempt to do this, first, by integrating conceptual work by bioethicists on the concept of health and by neo-Aristotelian ethicists on the concept of goodness for, in order to show that the normativity involved by the concept need not engage ethical values. Secondly, I will examine whether and under what conditions the concepts of health and goodness for can be extended to non-organismic entities such as ecosystems without requiring dubious ontological commitments.

Champagne, Marc

***Psillos Thinks We Ought to be Realists. But Why?* (III.1B)**

Although Stathis Psillos is clearly a scientific realist, it is much harder to pin down why exactly he endorses this view. The most plausible gloss consists in a double-negation: Psillos is a realist because he is against anti-realism. Although logically this ought to count as an affirmation, from a dialectic standpoint a defense of scientific realism which has no direct story to tell in its favour is far less attractive than it could be. Alas, the only positive reason Psillos gives in this regard seems to be a form of voluntarism. Yet, when Psillos (1999, p. 222) reproaches constructive empiricists for not taking the "extra epistemic risk" implicit in attributions of a mind-

independent basis, he essentially turns the commitment into a matter of personal preference. I therefore want to revisit Psillos' arguments and see whether they can be recast in a stronger, more assertive, form. Specifically, I want to see whether his reliance on inference to the best explanation can be upgraded to a historical induction helping itself to "the fact that there has been a lot of structural continuity in theory-change" (Psillos 2009, p. 135). Since such a revamped defence would make realism more compelling, I differ from Psillos in my appraisal of the amount of epistemic risk involved. I will nevertheless point out the necessity of retaining some form of voluntarism in the argumentative mix, and so will conclude by dispelling the mistaken fear that countenancing a subjective contribution invariably leads to relativistic consequences.

Charenko, Melissa

Illuminating Turn of the Century Physics at the University of Toronto: The Case of the Crookes Tube **(III.2A)**

In 1887, the physics department at the University of Toronto was established and James Loudon became its first Professor. In spite of vitriolic opposition, Loudon believed that it was important for students to carry out demonstrations of natural phenomena and test natural laws, so he hoped to bring practical, hands-on science to the university. After much negotiation with those who believed that theory had priority and that technical training had no place within a university, Loudon established the first physical laboratory in Canada and began to stock it with the necessary scientific equipment. As had happened elsewhere, these instruments soon came to play integral roles in the lives of students and professors, altering the way that physics was taught and the types of research questions that could be explored.

In order to illuminate the shifts taking place in turn-of-the-century physics, I look at the role that scientific apparatus played in these changes by tracing the origins and uses of one of the most versatile instruments purchased by the University: the Crookes tube. Crookes tubes could be used in fields as diverse as phosphorescence, magnetism and x-ray photography, allowing me to illustrate the types of research questions undertaken and the state knowledge of turn-of-the-century physics using this instrument as my vehicle. My study of Crookes tubes also reveals that their material culture influenced the change in pedagogical approach that occurred in North American universities at this time: with increasing instrumentation came an emphasis on empirical research.

Chow, Sheldon

Abduction in Science and Cognition **(III.1A)**

Charles Sanders Peirce coined the term "abduction" to refer to a type of inference in science which is non-deductive and importantly different from inductive inference. Over his career, Peirce continually modified his account of abduction, and consequently no complete or coherent picture of abduction is found in his writings. Since Peirce's time, however, philosophers have

been working toward developing accounts and logics of abduction which attempt to capture various aspects of Peirce's ideas.

In spite of burgeoning, but disparate, research, much work remains to clarify the nature of abduction, both in science and in cognition. The central task for the present paper is to connect several strands of research in order to make some progress on this matter. The paper first provides the historical context within which the problem of understanding abduction will be addressed. Some recent accounts of abduction are then surveyed, and the connection between abduction in science and abduction in cognition is considered. What follows is the beginnings of a characterization of abduction, through which the relation between cognitive abduction and scientific abduction is understood.

Close-Koenig, Tricia

Blurring Boundaries by Focusing Images. Medical Science and Medical Practice in Interwar Strasbourg, France. **(I.2C)**

Histo-pathologists had been interested in cancer since the coining of the cellular theory. Rudolf Virchow and Julius Cohnheim defined cancer as specific changes in tissues; cancer was then considered a "pathologist's disease." Virchow had emphasized the principles of biopsy and its value in the diagnosis of malignant tumours, but he himself did not promote it for diagnosis of patients. In the nineteenth century, it was a pathologists' disease in research only. By the mid-twentieth century, pathologists figured in medical practice as mediators between alternative therapeutic solutions. Histopathology now inhabited a new arena, medical practice.

In this paper, the process through which the microscope slid from research to practice materialised will be explored and detailed in the aims of understanding how medical research integrates medical practices. The quasi-inherent character of scientific knowledge to be relevant to medical practice is not taken at face value here. An exploration of how Strasbourg radiotherapists used pathology laboratory reports in the early years of the nationally organised Centres Anti-Cancéreux in France demonstrates one manner that knowledge came to speak to practitioners. I argue that pathologists could direct patients to specific forms of therapy on the basis of microscopic images of cancer cells because they had defined (and re-defined) cancers and co-constructed radiation therapy practices.

Conolly, Brian Francis

Early Fourtheenth-Century Theories of Motion: Thomas de Bailly and Hervaeus Natalis on Continuity and Actual and Potential Infinities **(III.1C)**

The question of whether a continuum is comprised of an actual or potential infinity turns up in the early fourteenth century discussion of the intensification and remission of forms, e.g., as when water changes from being less hot to more hot. Thomas de Bailly maintains that such a process involves the continuous succession of numerically distinct forms, so that when comparing any two stages in the process, the less hot is numerically distinct from the more hot.

One of the most effective objections against this theory argues that it entails passage through an actual infinity of numerically distinct forms corresponding to the "nows" of continuous time. Thomas argues in response that there is not an actual, but merely a potential infinity of forms that the process must pass through, maintaining that only the form at the terminus of motion is in act. Hervaeus Natalis, on the contrary, argues that if the forms through which the process passes through are not in act but in potency, then it is necessary to posit between these forms an absolute continuity, which involves not only infinite divisibility but also some measure of overlap between any contiguous parts of the motion. He then argues that to be continuous in this manner precludes these same forms from being numerically distinct from each other. I evaluate the respective positions and arguments and consider an alternative proposal that maintains the succession of numerically distinct forms but avoids the actual infinity problem by dispensing with the continuity of motion.

Crawford, Sean

On the "Logical Behaviourism" of Logical Positivism **(II.3C)**

The received view of the history of analytic philosophy is that the logical positivists—Carnap and Hempel in particular—held the view commonly known as "logical" or "analytical" behaviourism, according to which the relations between psychological statements and the physical-behavioural statements intended to give their meaning are analytic and knowable a priori. I argue that this is a complete myth: the majority of such relations were viewed by the logical positivists as synthetic and knowable only a posteriori. Misleading comparisons between logical behaviourism, on the one hand, and genuine analytic-meaning theses, such phenomenalism and logical constructionism, on the other, probably played a role in creating and propagating the ubiquitous myth. More important, however, was the logical positivists' idiosyncratic extensional use of what are now considered crucially intensional semantic notions, such as "translation," "meaning" and their cognates. I focus on a particular instance of this latter phenomenon, arguing that a conflation of explicit definition and analyticity is the primary source of the myth.

Crippen, Matthew

Dewey on Science, Arts and Greek Philosophy **(II.3B)**

John Dewey claimed that modern science has entailed "a generalized . . . adoption of the point of view of the useful arts"—combined, however, with a confused dissociation of science from art. He associated this with a latent allegiance to classical traditions that emphasize fixity, and this, in turn, with the persistence of certain sceptical trends. Some, for example, designate the everyday world of change as merely apparent, equating reality to fixed laws and mathematical certainties. That we can know reality by discovering laws and suchlike is meant to bring consolation, but it does not lessen the invitation to mistrust firsthand experience. Others grant that reality is in transition. They add, however, that we cannot think coherently and build bodies of knowledge without fixing concepts to some extent, making knowledge a perversion of reality. According to Dewey, the first of these outlooks fails to recognize that laws do not express "any matter of fact

existence." Rather, "their ultimate implication is application; they are methods and when applied as methods they regulate the precarious flow of unique situations." The second view similarly fails to recognize that concepts are not principally representations of reality, but instruments through which we cognitively and physically interact with reality, rearranging it in such ways that it becomes more intelligible.

The proposed paper will consider Dewey's efforts to understand science in terms of both the practical and fine arts, and his attempts to alleviate sceptical trends by doing so. Dewey drew liberally from ancient Greek philosophy, and I will also spend some time discussing how his philosophy of art and science suggest that Plato and Aristotle correctly identified conditions under which things become knowable, even though they did not satisfactorily answer how these conditions can be met.

Cuffaro, Michael

Reflections on the Role of Entanglement in the Explanation of Quantum Computational Speedup **(III.3B)**

Of the many and varied applications of quantum information theory, perhaps the most fascinating is quantum computation. In this sub-field, computational algorithms are designed which utilise the resources available in quantum systems to compute solutions to computational problems with, in some cases, exponentially fewer resources than any known classical algorithm. But while the fact of quantum computational speedup is almost beyond doubt, the source of quantum speedup is still a matter of debate.

In this talk I argue that entanglement is a necessary component for any explanation of quantum speedup and I address purported counter-examples that some claim show that the contrary is true. In particular, I address Biham et al.'s mixed-state version of the Deutsch-Jozsa algorithm and Knill & Laflamme's 'deterministic quantum computation with one qubit' model of quantum computation. In the first case I show that pseudo-entangled states are always required to achieve speedup and argue that while these states are separable by definition, there is nevertheless a clear sense in which entanglement plays a role in the computational work done by them. In the second case, many have concluded that an alternative form of quantum correlation, quantum discord, is responsible for the speedup of such states. I argue that this conclusion is erroneous: that one should rather view both discord and entanglement as manifestations of the same underlying physical resource, and that entanglement is the more fundamental of these.

Curiel, Erik

Animadversions on the Semantic View of Theories **(III.3C)**

The semantic view of theories, the dominant view in the literature for the past few decades, holds roughly that a theory is (or is fully characterized by) its set of models, in the Tarskian sense. I offer three arguments why such an account cannot provide a sufficiently rich foundation for a full semantics of physical theories. First, on such a view, theories tell us what the world would be like if the theory were true of it, nothing more and nothing less. But that cannot be correct, as theories are often used with great success to characterize and shed light on physical systems they do not provide sound or accurate models of. The classical theory of fluid mechanics provides a host of powerful examples of this practice. Second, in Tarskian semantics no semantic content can reside in or accrue to relations among a theory's models. But relations among the individual models of a physical theory in fact express much that the theory tells us about the world. Whether or not the equations of motion of a theory, for example, have a well set initial-value formulation in the sense of Hadamard surely is a component of the semantic content of a theory, but it is essentially a property of the relations among the theory's models. Third, the semantic view cannot distinguish between true predictions of a theory and propositions the theory requires as preconditions for its application in practice.

Dacome, Lucia

Wax, Models, and Anatomy: The Material Culture of the Body in Eighteenth-Century Italy **(III.2A)**

This paper will focus on the collections of anatomical wax-models that were created in mideighteenth-century Italy. Coloured, three-dimensional, and soft/moist-looking, mid-eighteenth century anatomical models instantiated an impressive means to overcome the shortcomings related to the messy setting of physical deterioration, bodily fluids, bad smells and fear of contamination that characterised anatomical dissection. In doing so, they defined a less threatening, more controllable, orderly, accurate, and to some even agreeable, medium of anatomical knowledge and were thus regarded as potential replacements of the natural body itself. At a time in which calls for the integration of anatomical knowledge into the education of the 'enlightened individual' were growing increasingly loud, anatomical models afforded a more intimate and 'polite' ambiance to the encounter with human anatomy and allowed for physical proximity with the details of the inner body. Praised for their 'beauty', they were regarded as capable of producing immediate consensus and promised to foster anatomical knowledge by communicating it in a pleasurable way. This paper will explore the social life of eighteenth century anatomical models by focusing on the role of anatomical waxworks in the context of the re-assessment of the notions of evidence and credibility supporting claims about the body.

Djedovic, Alex

Downward Determination and Downward Explanation **(II.2B)**

The reductionism/emergentism debate has generated a variety of positions. Arguably the most popular stand on the topic is to reject ontological emergentism while still holding some variety of

methodological or epistemic emergentism. Such a position is compelling, but is often polysemous and unclear. This talk will aim at a more precise characterization of methodological/epistemic emergentism by drawing on resources from the philosophical accounts of scientific explanation. To that end, I argue for the plausibility of explanatory emergentism. This means that, given a suitable characterization of levels of organization, there is only upward causal determination, but explanatory relations can operate either in an upward or downward direction. Distinguishing between downward causal determination and downward explanation can contribute to clarifying emergentist claims, especially in fields dealing with complex systems such as systems or evolutionary biology.

Holding the above position requires developing an account of downward explanation that respects the rejection of ontological emergence. To do this, I develop philosophical machinery based on a combination of Woodward's (2002, 2007) causal-mechanical and Strevens' (2003, 2007) kairetic accounts of explanation. The combination of these two accounts allows for a level-neutral theory of explanation that places central importance on invariance relations under intervention. I argue, using the example of a living cell, that the explanation of the behaviour of any complex system is better captured by such explanatory machinery than by rival conceptions of explanation.

Dumsday, Travis

***Laws of Nature Don't Have* Ceteris Paribus *Clauses, They Are* Ceteris Paribus *Clauses* (I.3C)**

Laws of nature are properly (if somewhat controversially) conceived as abstract entities playing a governing role in the physical universe. Dispositionalists typically hold that laws are reducible or eliminable in favour of causal powers. That is, what we call 'laws of nature' are just descriptive generalizations of regularities whose ontological grounds consist in the dispositional properties associated with natural kinds. Certain dispositionalists have adopted more generous views of laws, arguing that dispositionalism allows for a certain kind of nomic realism (for example Bird (2006;2007)) or even that there are good explanatory reasons for positing both dispositions and laws (Hughes & Adams (1992) and Dumsday (2012)). However, to date no one has tried to argue that dispositionalism strictly implies nomic realism. This is what I attempt here. Basically, I argue that since at least some (probably all) dispositions have ceteris paribus clauses incorporating uninstantiated universals, and these ceteris paribus clauses help to determine their dispositions' ranges of manifestation, there are abstracta (in this case uninstantiated universals) which play a governing role in the physical universe. Hence laws of nature are real, irreducible factors in ontology, even on dispositionalism. I then consider some broader implications of this conclusion for other debates in metaphysics and the philosophy of science.

Gault, Dylan

***The Phillips Relation as a Newtonian Phenomenon* (II.1C)**

In a 1993 paper, Mark Phillips established a relationship between the light curve of members of a certain type of supernova and the peak brightness of these events. This relation, and ones similar to it, became the foundation of the work awarded The Nobel Prize in Physics in 2011. From a philosophical perspective, what is interesting about this paper is not the conclusion of the paper but the methodology used to justify these conclusions, of which establishing the relationship was a key part. Phillips' paper shows us a particular case of how one can, without producing a specific model or mechanism for a known class of phenomena, make a potentially robust generalization about that class that, on the strength of the empirical evidence, is valuable for further investigation to the extent that the generalization allows one to reclassify observations formerly identified as outside of the class as properly within it.

I propose that we understand Phillips' work as an example of the discovery of a phenomenon in the sense used by Isaac Newton. Accordingly, the Phillips Relation is an inductively justified theoretical abstraction from observations. In the Newtonian fashion, this relation is something that we expect to observe complicated by other phenomena that interfere with our observation of the phenomenon, but in systematic ways that provide us with information and is itself justified in the same way.

Gawne, Richard

***Logical Empiricist Philosophy of Biology Reconsidered: An Examination of Felix Mainx's* Foundations of Biology (II.3C)**

Works such as Kenneth Schaffner's "Antireductionism and Molecular Biology" (1967), William Wimsatt's "Teleology and the Logical Structure of Function Statements" (1972), Michael Ruse's The Philosophy of Biology (1973), and The Philosophy of Biological Science by David Hull (1974) are generally regarded as the first significant works on the philosophy of biology published in the twentieth century. Many contemporary philosophers of biology have suggested that the discipline's belated development is attributable to the erstwhile prominence of logical empiricism, which is said to have: (a) discouraged philosophers from studying the life sciences, and (b) corrupted the few scattered attempts at the philosophy of biology that were attempted. Recently, however, there has been a renewed interest in the history of twentieth-century philosophy of biology, and the legitimacy of the 'received view' outlined above has been called into question. Close historical research reveals that a number of philosophers influenced by logical empiricism did, in fact, have strong interests in the philosophy of biology, and that many of the topics being explored by these individuals would be familiar to contemporary practitioners. The aim of this essay is to contribute to the ongoing efforts to reevaluate the history of twentieth-century philosophy of biology by exploring the positions taken by Felix Mainx in his Foundations of Biology, which was originally published as a monograph in the International Encyclopedia of Unified Science. A close reading of this long-neglected work reveals that proponents of the received view have mischaracterized the nature of logical empiricist philosophy of biology. This being so, I argue that a new narrative history of twentieth-century philosophy of biology should be constructed.

Hall, Brian K.

From Homology/Analogy to Homology/Novelty: 170 years of the Concept of Homology **(I.3A)**

A central issue in homology since Richard Owen coined the modern definition in 1843 has been whether homology of features depends upon those features sharing common developmental pathways. Owen did not require this criterion, although he observed that homologues often do share developmental and (as we now know) genetic information. A similar situation has been explored to a limited extent in the origin of behaviors, especially, whether homologous behaviors (must) share a common structural basis. However, development evolves, and we now know that there can be several ways to produce an individual homologous feature. I discuss how the homology concept has been translated across the various levels of the biological hierarchy. A further conceptual issue is "with what do we contrast homology?" Owen said analogy, which is not incorrect but is a pre-evolutionary contrast. In 1870, E. Ray Lankester proposed homoplasy (independent evolution) as a second class of homology, alongside homogeny (shared evolution). Homoplasy has been equated with convergence (convergent evolution) and contrasted with homology (evolution from a common ancestor, including parallelism). But homology and homoplasy both reflect a single evolutionary tree of life. Novelties are features lacking a homologue in ancestors of the lineage. New genetic information arises either by gene duplication followed by one copy of the gene acquiring a new function, or by gene co-option following lateral (horizontal) gene transfer when novel genes can be transferred between distantly related organisms. Novelty may be the appropriate aspect of the genotype and phenotype to contrast with homology.

Hardy, Kristen A.

The Vital Force of the Nation: Homeopathy and Biopolitics in Victorian Canada **(III.3A)**

During the nineteenth-century, medicine was a contested terrain throughout much of the world, with adherents and practitioners from a variety of healthcare philosophies jostling for theoretical dominance, professional recognition, the allegiance of patients, and the right to practice in accordance with their own doctrines of illness and cure. Drawing on the work of Michel Foucault, I investigate the influence of anatomo-clinical and biopolitical rationalities on one such system of medical thought and practice, homeopathy, during the Victorian era, specifically within the Canadian context. This paper considers how specific 'ways of seeing' are inextricably linked with particular understandings of bodies, with biomedically rooted conceptions of health and illness, and with relations of power mobilized to justify professionalized medicine's authority over subjects. I argue that, while the dominant epistemic regime of the period impacted the evolving character of Victorian-era homeopathy, it also likely contributed to the decline of this system of alternative medicine and its failure to become a major player in efforts to bring the social body of the burgeoning Canadian nation under biopolitical management.

Harper, Bill

Newton's Scientific Method in Cosmology Today: The Transformation of Dark Energy from Wild Hypothesis to Accepted Background for Large Scale Cosmology. **(II.1C)**

On the basic Hypothetico-deductive model hypothesized principles are tested by experimental verification of observable consequences drawn from them. Empirical success is limited to accurate prediction.

Newton's inferences from phenomena realize an ideal of empirical success that is richer than prediction. To realize Newton's richer conception of empirical success a theory needs to do more than to accurately predict the phenomena it purports to explain; in addition, it needs to have the phenomena accurately measure parameters of the theory. Newton's method aims to turn theoretical questions into ones which can be empirically answered by measurement from phenomena. Propositions inferred from phenomena are provisionally accepted as guides to further research. Newton employs theory-mediated measurements to turn data into far more informative evidence than can be achieved by hypothetico-deductive confirmation alone. On his method deviations from the model developed so far count as new theory-mediated phenomena to be exploited as carrying information to aid in developing a more accurate successor.

All of these enrichments are exemplified in the classical response to Mercury's perihelion problem. Contrary to Kuhn, Newton's method endorses the radical transition from his theory to Einstein's. These richer themes of Newton's method are, also, strikingly realized in the response to a challenge to general relativity from a later problem posed by Mercury's perihelion.

We can also see Newton's method at work in cosmology today in the support afforded to the (dark energy) cosmic expansion from the agreeing measurements from supernovae and cosmic microwave background radiation.

Hochstein, Eric

A Patchwork View of Mechanistic Explanations **(II.1B)**

Over the past few years, it has been argued that the ideal models for providing explanations in the life sciences are mechanistic models (e.g. Machamer et al. 2000; Bunge 2003; Glennan 2005; Darden 2006; Craver 2006; Bechtel 2008; Wimsatt 2007). Mechanistic models provide explanations by identifying the structured components of systems, and describing how they interact to produce some phenomenon. These models are often contrasted with mere phenomenological models, which characterize and predict the phenomena produced by systems without explaining them (Craver 2006).

In this paper, I argue that the relationship between mechanistic and phenomenological models in the life sciences is more complicated and interconnected than philosophers have presupposed. The idea that mechanistic models explain while phenomenological models do not is overly simplistic. Both types of models can often be necessary, yet distinct, components of an explanation, with neither model in isolation being sufficient to provide an explanation.

In addition to characterizing physical parts and operations, an explanation of a mechanistic system must also provide a detailed account of the phenomenon under investigation. However, the model that is ideal for identifying the mechanisms of a system is not necessarily the best model for characterizing the phenomenon produced by that system. Instead, it is often a phenomenological model that is ideal for such tasks. This means that each type of model provides different information about the system needed for generating explanations, even though they may characterize systems in radically different ways that do not cohere neatly into a single type of model.

Hughes, William

Hans Jonas and Dialectical Opposition in Biology **(I.3A)**

Most philosophers of science approach biology from an analytical background; few contemporary philosophers incorporate dialectical logic into their understanding of biology. Perhaps due to the lack of mutual intelligibility, philosophers who do so - such as Hans Jonas - remain obscure. I believe that: (1) there is a strong case to be made for the applicability of dialectical principles to biology, both as a rational-scientific enterprise and an existential, humanistic endeavor; and (2) that Hans Jonas, in works such as The Phenomenon of Life, comprehensively outlines one such approach. Biology, which characterizes the living world as a dense, contingent and interconnected weave of causal processes, is an area of natural science in which freedom and contingency, the Animate and the Inanimate, potentiality and actuality, remain important oppositions.

In this presentation, I will: (1) introduce the dialectical principles which characterize Jonas' philosophy of biology (with a particular focus on his evolutionary teleology); (2) briefly explain the historical reception of Jonas' biological work; and (3) identify areas in the contemporary biological literature where Jonas' perspective could be salutary. I will also, as a practicing biologist, reflect on Jonas' influence on contemporary works that specifically refer to dialectical inquiry in biology, such as Levins and Lewontin's The Dialectical Biologist (1987). I will conclude that dialectical opposition is a useful, yet neglected tool in the philosophy of biology.

Hull, James

Town and Lab Coat: The University of Toronto and Role of Expertise in City Governance **(II.1A)**

The importance of universities to the economic development of the cities in which they are located and the position of scientific and technical experts in municipal governance have separately been explored. But these have rarely been brought together in a consideration of the roles played by university science and engineering faculty in addressing the new technical problems of growing and changing urban areas of the later 19th and early 20th century which confronted and sometimes confounded city councils. This paper looks at this topic in the case of the city of Toronto and the University of Toronto. In some cases, faculty simply acted as in effect consultants in specific instances where the city lacked sufficient or appropriate expertise

among its own staff. But in other cases faculty were drawn into political disputes which illustrate the problematic status of "expertise" in democratic governance. The chance to play the role of municipal experts however offered tempting possibilities for personal gain and professional advancement in an era of broad cultural acceptance of the supposed neutrality of science but less well developed recognition of conflict of interest.

Jansson, Lina

Does Newton's Theory of Gravity Postulate Causal Action at a Distance? **(I.2B)**

On face value it is tempting to take Newton's law of gravity as postulating causal action at a distance with, for example, the sun causally influencing the movement of the distant planets. Moreover, in both published work and in correspondence Newton seems happy to talk of universal gravity as being the cause of the motion of the heavens and the sea. However, in his famous 1693 letter to Bentley Newton seems to deny that such action is possible. Moreover, in the General Scholium itself he claims not to have discovered the cause of gravity. Janiak (2008) calls this Newton's dilemma and provides a solution to the dilemma that relies on Newton's distinction between a mathematical and a physical treatment of forces such that gravity is taken to be (among other things) a physical quantity that causes the motion of the planets around the sun as well as the motion of objects on earth.

I argue that we can reconcile the seeming tension in Newton's view in a way that stays closer to closer to Clarke's understanding of Newton. This suggestion involves thinking of his introduction of a mathematical treatment of force as merely involving the promissory note that there is a cause (so that different types of phenomena can be assigned to the same cause), while giving a law-based (and mathematical) explanation for the motion in question that nonetheless goes beyond being merely a tool for calculation.

Jinn, Nicole

A New Measure of Correlation to Infer Causation (**III.1A)**

The most widely accepted conception of correlation in science is one articulated primarily by Karl Pearson [12]. In this paper, I introduce an alternative measure of correlation called distance correlation, denoted dCor, that addresses two major deficiencies of the widely used Pearson correlation coefficient: 1) it can be zero for dependent random variables and 2) it requires the dimension of any two random variables to be identical. I show that the two deficiencies are closely related and argue that distinguishing independent variables is as important as recognizing dependent ones and that this hasn't been as closely looked at. The importance of properly establishing independence between random variables arises in numerous applications, especially in applying the causal Markov condition (CMC). The CMC is among the most frequently used assumptions in modern development of causal inference from statistical data.

Considering the lively debate, surrounding acceptance of the CMC, the contribution of this paper is that the dCor test of independence can be used to accurately detect all types of causal structure,

making causal inference more feasible. Accordingly, my response to Cartwright's opposition to using the CMC is that using the dCor to test the CMC enables researchers to relax the assumption of the CMC. This leads to more careful analysis of applying the CMC, which satisfies the intuitions that motivate Cartwright, and other philosophers of science, to argue for testing the CMC at all.

Jordan, Shelly L.

The Roles of History of Science and Philosophy of Science in T. S. Kuhn's Understanding of the Nature of Theory Choice in Science **(III.1B)**

One of the common charges laid against T.S. Kuhn's notion of the nature of theory-choice is that it denies the objectivity and rationality of science. In this paper I will argue, along with Kuhn, that such charges reflect a misunderstanding of both Kuhn's project and his conclusions. I shall also argue that at least some of this misunderstanding tends to stem from critics not clearly distinguishing between Kuhn qua historian of science and Kuhn qua philosopher of science. Making this distinction requires that one be at least somewhat familiar with the broader scope of Kuhn's writings and in particular with those of his writings that deal with the nature of what it means, in his view, to do the history of science as compared to what it means, in his view, to do the philosophy of science. Admittedly, this necessity to be well-read in Kuhn in order to properly understand him, arises at least in part because Kuhn himself was not always careful to maintain this distinction -- a distinction which he himself insisted was indispensable in the type of interdisciplinary work that he advocated and that he himself did.

Justus, James

The Methodological Individualism of Individual-Based Ecological Modeling **(I.2A)**

Cross-pollination between evolutionary and economic theorizing has a long and fruitful history. Evolutionary economics and evolutionary game theory are prominent examples. Recent philosophical work targets the same confluences (e.g. Okasha 2009). But evolution is but part of biology. And although "nothing in biology makes sense except in the light of evolution," ecology casts the same indispensable light. Fruitful cross-fertilization of ecological and economic theorizing should be expected even if it remains largely unexplored. Connections between methodological individualism (MI) and individual-based models (IBMs) is part of that untold story. MI is multifaceted, but a common denominator is privileging the individual level in explanations of higher level social phenomena. IBMs embodies the same perspective in ecological science. Just as actions of individual (rational) agents constitute the preferred level of analysis according to MI in economics, individual organisms function similarly for IBMs in ecology. Evaluating this analogy is the first task of this paper. Interestingly, recent work connecting rational choice theory and evolutionary theory provides support (e.g. Okasha 2011). The second task is showing that debates about MI in social science reveal insights about how IBMs should be understood. For example, the explanatory priority of the individual MI requires does not require ontological reductionism of population-level properties to the individual-level. Another example is that different methods for scaling actions of individual agents to higher-level

social patterns suggest tantalizing directions for how the connections between rational choice theory and evolutionary theory mentioned above might also be scaled to the population and community level.

Lamoureux, Lucien

Where Is There? Self-Location in Structural Representation **(III.2C)**

In his latest book, Scientific Representation, van Fraassen presents an empiricist view of structuralism and accounts for the empirical content of our theoretical terms on the basis of self-location. His idea is that in observing or measuring real entities our perceptual judgement is guided by classificatory logical spaces common to a theory's models. Implicit in perceptual judgement is a linguistic act of self-location in a logical space expressible by the indexical judgement "this is there in logical space". When information content from perceptual judgements is worked-up into a data model, self location becomes a condition of use of the data model as well as that of any embedding theoretical model. Pragmatic incoherence arises if I present a model as representing real entities then deny or doubt that it represents them, for I undo the significance of the act of self-location in a logical space.

But what, exactly, is a logical space? Van Fraassen describes it differently as an "ideal entity", an "artifact" and an "abstract entity". Yet, as a nominalist he denies its existence. In this paper I trace the development of this idea from van Fraassen's causal theory of time to his empiricist structuralism. I claim that as an entity logical space has become reified over time. I argue further that van Fraassen's appeal to pragmatic incoherence commits him to its existence

Lanao, Javier

Thought Experiments and Computer Simulations: A Paradox for Empiricism? **(I.1C)**

Empiricist philosophers of science are confronted with a genuine epistemological challenge generated by two intuitive, widespread beliefs: on the one hand, empiricists defend the epistemic priority of experimentation arguing that knowledge about the natural world is essentially obtained by direct experimental interaction. On the other hand, thought experiments and computer simulations seem to produce knowledge about the natural world without directly interacting with it. Most of the proposals in the literature escape this paradox by rejecting one of the conflicting claims. In contrast, I present a solution that reconciles both claims. My solution is based on an ontological distinction between the natural world and its representations, which implies a fundamental epistemic difference: while computer simulations and thought experiments are formal systems ontologically constrained by the theoretical knowledge that has been used in their design, experimental settings ontologically emulate their target systems keeping a certain degree of autonomy from the theoretical model. This difference has two further non-conflicting epistemological consequences which match the underlying intuitions that generated the paradox: first, only results from laboratory experiments can increase or decrease the justification of the model, accounting for the epistemic priority of experimental data; second, the results from simulations and thought experiments have the status of logical consequences and

are, therefore, unable to further justify the theoretical model. However, their formal nature grants that their results will be epistemically on par with experimental results for all other epistemic purposes allowing us to reliably explore the modal consequences of theories and models.

Lavers, Gregory

The Prospects for a Neo-Carnapian Philosophy of Mathematics **(I.3B)**

This paper seeks to reevaluate Carnap's mature philosophy of mathematics. To do so, it is important to identify the central features of Carnap's post-semantics views on mathematics. None of Carnap's post-semantics works can be seen as a definitive statement of his mature position on the philosophy of mathematics. As a result, his later view must be assembled from several different sources. I emphasize the notion of explication as central to Carnap's mature philosophy, and in particular his philosophy of mathematics. I then identify what I take to be the main advantages and disadvantages of Carnap's view. I argue that, despite their centrality in Carnap's mature thought, Carnap's views on explication are responsible for the main disadvantages that I identify. I further argue that Carnap's official account of explication is more liberal than is required for his own purposes. By accepting an, at least slightly, more constrained account of explication, one can avoid the disadvantages of the Carnapian position while preserving an essentially Carnapian position.

Leuridan, Bert

Evidence Amalgamation and the Principle of Total Evidence **(I.1B)**

In his paper on meta-analysis and evidence amalgamation, Stegenga (2011) devotes much attention to the Cochrane Collaboration (an institution striving for evidence-based health care). The Cochrane Collaboration has a strong preference for primary studies based on randomized controlled trials, at the expense of other types of studies (Higgins and Green 2009). This, Stegenga argues, violates the 'principle of total evidence' (Carnap 1947).

I am sympathatic to the view, defended by Stegenga, that evidence amalgamation (in casu the works of the Cochrane Collaboration) would benefit from covering a range of diverse kinds of primary studies.

I doubt however that the 'principle of total evidence' can straightforwardly be invoked to ground this claim. There exist many versions of the principle, and it is not clear which version best fits the practice of evidence amalgamation. I will focus on (a) the users of this evidence (for whom should the evidence be available), (b) the relevance of the evidence (what criteria should be used for the in-/exclusion of studies?) and (c) the reliability of the evidence (Carnap assumed that all evidence is equally and fully reliable; this is not the case in evidence amalgamation).

In short, the 'principle of total evidence' can shed useful light on interesting issues concerning evidence amalgamation, but it cannot straightforwardly be used to blame existing practices. There is much room (and need) for interpretation and for uncovering its underlying assumptions.

Louson, Eleanor

***Objective lenses?* Planet Earth*'s HD Cameras as Political and Situated.* (I.2A)**

I explore the politics inhabiting the high-definition cameras employed in the BBC Natural History Unit's documentary series Planet Earth (2006), as well as the contribution of these cameras to the program's purported objectivity. Filmmakers call on the technological sophistication of Planet Earth's HD cameras, as well as the use of novel filming techniques, to promote the documentary providing an unmediated and nondisruptive view of nature. However, as in most "blue-chip" wildlife programming, Planet Earth provides only one view of nature among many possible views, in this case one bereft of human beings and traces of civilization, devoid of animal behavior with no easy sociobiological explanation, and lacking any departure from heteronormative sexual behavior in the animal kingdom. I argue that Planet Earth's cameras offer a spectacular helicopter's-(and even satellite's-)eye view of nature, which is easily conflated with an objective view from nowhere; their hidden politics are reactionary, and promote an essentially conservative reading of nature and a naturalized account of human beings. These cameras can only offer partial perspectives, both literally and in terms of the stories about nature they have been enlisted to tell. As such, these documentary cameras can be fruitfully described within Haraway's account of situated knowledges. Our access to the natural world is mediated by these cameras, and their imperceptibility is part of that mediation.

McArthur, Dan

***Reconsidering Bohr's Neo Kantianism* (II.3A)**

Many philosophers of science in the mid twentieth century, such as Popper and Bunge, characterised Bohr as a crude verifictionist. However more recent scholarship on Bohr has revealed this, and many philosophical readings of the Copenhagen interpretation, to be a caricature of his actual views. In fact in some substantive respects Bohr's philosophy of science shares at least some features that are amenable to realists. As a number of scholars have noted, Bohr's philosophy was influenced heavily by Kant's philosophical framework for classical physics. This illuminates many key features of Bohr's thinking such as the correspondence rule and in his views on the centrality of classical concepts. In this paper I argue that understanding Bohr's Kantianism lets us re-evaluate the relation of Bohr's thought to the logical empiricism that was influential in his day. Recent scholarship by Richardson, Friedman and others has revealed that logical empiricism, like Bohr, has also been falsely caricatured as crudely varificationist. Friedman has also explored in some detail the neo-Kantian legacy in logical empiricism. Looking at a re-evaluated Bohr alongside a re-evaluated logical empiricism not only lets us get a clearer picture of Bohr's relationship with the philosophy of his day, it sheds light on the philosophical aspects of his arguments with Einstein and others. Most importantly for this paper, disabusing both Bohr and logical empiricism of crude verificationism and examining

them with a view to exploring their shared Kantian heritage lets us get a clearer look at some affinities but also some significant but under-emphasised differences between his thought and logical empiricism.

Meynell, Letitia

A Waltonian Model of Thought Experiments **(I.1C)**

Thought experiments have enjoyed intense critical scrutiny from philosophers and historians of science for more than twenty years. While various views can be found in this literature, it has been shaped by the on-going dialogue between John Norton and Jim Brown. Norton maintains that thought experiments are effectively colourful arguments; they can always be reduced to arguments and their contents are empirical (and thus *a posteriori*). In contrast, Brown argues that thought experiments are in a sense experiential and that their contents, at least sometimes, are *a priori* – they are internal experiments revealing Platonic truths perceived by the mind's eye. Other accounts, and there are many, are often thought to sit on a spectrum somewhere in between these poles.

In this paper I offer an account that strikes out on a new. Taking my starting point from Kendall Walton's account of representation (first devised for works of art), I argue that thought experiments are props for imagining. Contra Norton, I maintain that thought experiments share no more with arguments than a capacity to persuade, and that they defy reduction to any recognizable formal or informal argument form. Contra Brown, I argue that thought experiments need not be quasi-perceptual, though they do make use of our perceptual and imaginary capacities and habits in conveying their content and proving their points. As to the question of their a priori/ a posteriori nature, I show that a Waltonian approach effectively deflates its significance. Ultimately, a Waltonian analysis reveals that thought experiments share more with pictorial representation in the sciences than with either argumentation or observation (albeit of the Platonic realm).

Mills, Susan

"A Fever Caused by Sadness": Descartes' Psychosomatic Medicine. **(I.2C)**

Descartes has drawn great attention from medical histories for his mechanical philosophy and its medical implications. As these histories of medicine rightly acknowledge, Descartes's anatomical writings are replete with comparisons of the human body to machines, and—more exceptionally—he takes this comparison beyond mere analogy with his soul-body dualism and explanations of the organization and operations of the 'body machine' strictly as matter in lawful motion. However, with its emphasis on Descartes's machine model of the human body and the mechanical philosophy underlying it, the history of medicine has overlooked the role of the soul in Descartes's own medicine. Despite a legacy that would suggest otherwise, Descartes in fact advances a psychosomatic medicine that concerns the human being as a combination of soul and body. Descartes's psychosomatic medicine is particularly evident in his correspondence with Princess Elisabeth of Bohemia. In those letters, Descartes dispenses diagnoses and remedies that

treat the state of Elisabeth's soul as both the cause of and the cure for her maladies. Indeed, he deems the passions of the soul to be of the utmost importance in matters of the body's health. In sharp contrast, pure Cartesian mechanistic medicine of the body would preclude consideration of the passions. In my evaluation of this disparity, I look to Descartes's stated ambition to improve medicine, assess where his psychosomatic medicine stands in that regard compared to the strict mechanism for which the history of medicine has remembered him, and argue that it stands much closer.

Moore, Rebecca

Human Manufacture or Product of Nature? The first life patent in Canada, 1976-1982 **(I.1A)**

The understanding of the organism and its constituent parts has been fundamental in the legal decision to grant so-called genetically modified organisms entry into the Canadian patent system. In this paper I discuss the first Canadian patent granted for a whole organism—a colony of yeast species useful in the remediation of wastewater from pulp and paper mills. The 1982 decision by the Canadian Commissioner of Patents to grant patent protection over the described colony of yeast and 'yeast-like' microorganisms is historically significant for two reasons: first, it set the precedent allowing for certain living modified organisms to be regarded as statutory subject matter of the Patent Act; second, the decision set initial parameters for how to assess whether a living modified organism met the requirements of patentability as set out by the Patent Act. Both of these legal achievements required an understanding of the organism and the role of the scientist qua inventor. In this paper I examine the assumptions made about the nature and composition of the colony of microorganisms that allowed it to be granted patent protection under the jurisdiction of the Patent Act. I argue that the decision of the Canadian Commissioner of Patents was largely based on the precedent set in the United States in the Ananda Chakrabarty case, and that both of these decisions relied on a reductive analogy comparing microorganisms to chemicals.

Murphy, Taylor

On the evolution of an interpretive-access process of mind reading: the case of the default-mode network **(III.2B)**

Ever since early false-belief tests and theories of mindreading, philosophers and psychologists have been interested in the evolution and development of mindreading capacities (Baron-Cohen et al. 1985; Leslie, 1987). Peter Carruthers' (2011) "interpretive sensory access" (ISA) theory challenges traditional "inner-sense" accounts of mind-reading that assume direct access to one's own intentions and beliefs (e.g., Goldman 2006). Decisive in assessing competing accounts are general evolutionary and developmental considerations and behavioural studies in psychology, yet the evolution of cognitive processes themselves receive comparatively little attention. The notion of 'homology' from evolutionary-developmental biology—wherein evolutionary origins determine trait identity—is germane to theorizing evolutionary and developmental properties of evolutionary traits. This notion has recently been applied to biological functioning and the

prospect of its relevance for relating cognitive activities within a single individual has been raised, viz. serial or cognitive homology (Love 2007, M.L. Anderson 2010, Bergeron 2010).

I aim to show how evolutionary-developmental cognitive neuroscience can be brought to bear theories of mindreading. Particularly, I assess the role of the "default-mode network" (DMN) in the emergence of ISA. While consistent with ISA theories, cognitive homologies with DMN challenges certain evolutionary claims in Carruthers' account. Even if mindreading of others is evolutionary prior to reading one's own mind in task-related thought and cognition, mindreading of others may have originated through redeployment of an activity assessing one's own beliefs and intentions in task-unrelated thought, i.e., when "default moding." I evaluate the evidence for this and its consequences the evolution of mindreading in ISA accounts.

Myrvold, Wayne C.

***On the Social Responsibility of Philosophers of Science* (II.2A)**

As evidence mounts that most of the recent increase in global temperatures is attributable to anthropogenic greenhouse gases, public opinion has trended towards increasing skepticism. At least part of this trend can be attributed to an industry-funded public relations campaign aimed at promoting skepticism about climate science, documented in numerous publications (see e.g. the Union of Concerned Scientists' Smoke, Mirrors, and Hot Air, Hoggan and Littlemore's Climate Cover-Up, and Oreskes and Conway's Merchants of Doubt).

One tactic of the campaign has been to make much of some flaws in individual scientific publications. The attention paid to these would be appropriate if the conclusions of climate science were based on a delicate chain of reasoning no stronger than its weakest link. However, as scientists and philosophers of science have long recognized, we have reliable evidence for our conclusions primarily when multiple lines of evidence converge to support a common conclusion. Attribution of global warming is one example of this. Arguably, then, at least part of the efficacy of the disinformation campaign is due to misconceptions about how scientific evidence bears on theory. If so, then public understanding of science could be benefited by improved communication, not only of the results of science, but also its methods. The study of scientific methodology is the province of philosophy of science. I will argue that those of us whose field of research is articulating how science works have a role responsibility to endeavour to make such a contribution, and will discuss how we can.

Okruhlik, Kathleen

***Voluntarism, Values, and van Fraassen* (III.1B)**

Different forms of epistemic voluntarism are characterized by quite diverse stances toward the so-called "non-epistemic" values that affect the practice and content of science. In previous work, I have explored the values-and-voluntarism nexus in the work of Otto Neurath, highlighting both the striking similarities and the important differences between his approach and those that characterize some feminist accounts of science. In this paper, I turn my attention to Bas van Fraassen's epistemology and his philosophy of science, with an eye to understanding the relationship between s.

Various statements of van Fraassen's voluntarism will be canvassed, with emphasis on the characterizations developed in "Belief and the Will" (1984), Laws and Symmetry (1989), and The Empirical Stance (2002). Special attention will be paid to the historical antecedents van Fraassen cites at various times. These range from St. Augustine, Pascal, and William James to Jean-Paul Sartre. I shall explore not only the arguments but also prima facie tensions among some of these sources, especially tensions between the salient elements of American pragmatism and French existentialism. Attempting to resolve those tensions will serve to highlight some of the striking differences between van Fraassen's epistemic voluntarism and the distinct voluntarisms espoused by Neurath and by some twenty-first century feminist philosophers of science.

Olley, Allan

***The Hollow Moon: Inference in Celestial Mechanics* (I.2B)**

In 1966 Wallace J. Eckert, IBM researchers and Columbia University professor, put forward a suggestion that an anomalous lunar motion could be accounted for by the Moon's mass being concentrated near its surface, a hollow Moon. This anomaly and attendant conjecture received limited notice at the time and further measurements of the Moon's mass density along with improvements in the measurement and theory of lunar motion laid the anomaly to rest without recourse to the Hollow Moon. Eckert was one of the foremost experts on the lunar orbit and had dedicated his career to the innovative use of computers in refining the predictions of celestial mechanics. Most of Eckert's career was given over to the refinement of numerical prediction, but this incident affirms Eckert's conviction that improved prediction was an avenue of investigating physical hypotheses. In this talk I will outline the history of the Hollow Moon conjecture and then tease out the methodology that underlay it and relate it to long standing traditions in astronomy. My sources include the published literature of the time and archival sources that give some more insight into Eckert's attitude and the response his work received.

Olson, Dustin

***The Status of Quantum Mechanics in Russell's Mature Philosophy* (II.3A)**

Well known within Bertrand Russell's later theory of knowledge and perception are the postulates of non-demonstrative inference (hereafter, "postulates"). Less known is the seriousness with which Russell grappled with the contemporary science of his time—he worked assiduously to inform his philosophy with an understanding of how the world functions according to physics. Given his scientific diligence, it is interesting to find that Russell's postulates appear almost naively inconsistent with our current understanding of quantum mechanics (QM). But could Russell, who so thoroughly engaged with and incorporated physics into his philosophy, make such a crass mistake? I argue that due to recently discovered correspondences and a specific interpretation of quantum *reality*, Russell's development of the postulates, which ground and enable his entire theory of knowledge, is neither ignorant of nor

naïve towards QM. Even more, under the Copenhagen Interpretation of QM, I argue that Russell's system reveals a highly sophisticated grasp and integration of QM into his philosophical system. In subsequent developments within QM, Russell's theory may not stand the test of time; we cannot conclude, however, that his system failed to embrace the difficult scientific conclusions of his day. This argument provides strong evidence for why Russell's mature work remains of interest for metaphysicians, epistemologists, and philosophers of science alike.

Peacock, Kent

***Einstein on Separability and the Very Possibility of Science* (II.3A)**

In 1948 Albert Einstein published a short paper in which he summed up why he thought that quantum mechanics (QM) was incomplete. His argument for incompleteness is essentially the same as that of EPR 1935. In my talk I will explore why the peculiar sort of incompleteness he identified troubled him so much.

Einstein argued that it is essential to physical science that it treats the world as made up of objects arrayed in spacetime, that these objects have a reality independent of the perceiving subject," and that they obey what Don Howard has called the Separation Principle (SP): spacelike separation at time t guarantees physical independence at time t. Without SP, Einstein argued, \physical thinking in the familiar sense would not be possible" because one could not isolate an object so that it could reveal its properties independently of how it was studied.

Einstein's view remains, so to speak, the sentimental favourite even among those who know that it is almost certainly untenable. I will argue that his expectations for physics are unreasonable in more than one respect. First, experimental work on entangled states continues to supply more and more evidence for the kind of non-separability that Einstein thought was absurd. Second, Einstein's demand for the complete isolability of objects of study is too much to ask of any scientific theory. Third, it is decidedly inconsistent with the trend of recent research in quantum gravity to suppose that physics necessarily restricts itself to the study of discrete objects in a spacetime continuum.

Pearlberg, Danny

Mechanistic Explanations and the Classicism-Connectionism Debate

I argue that there are prima facie advantages to understanding the relationship between classical and connectionist approaches to cognitive architecture within the context of the mechanistic approach to explanation. These advantages are best seen in light of the current trend in cognitive modeling towards a hybrid approach- one that uses both classical and connectionist approaches. While the debate originally arose within the context of Marr's levels of analysis account of explanation in cognitive science and Nagel's account of intertheoretic reduction, neither account is well-suited to make sense of a hybrid approach to cognitive modeling.

In contrast with Marr's levels of realization, levels of mechanisms stand in a part-whole relation to one another, with the parts of a mechanism being on a lower level than the mechanism as a whole. Importantly, mechanistic explanations are inherently interlevel, in that showing how the phenomenon in question is produced by a mechanism requires an explanation that incorporates each of the levels of the mechanism.

Using the mechanistic approach we can allow for the possibility that some connectionist mechanisms stand in part-whole relation with some classical mechanisms. In which case, the explanation of a given cognitive phenomenon may require an explanation that incorporates both a connectionist mechanism and a classical mechanism. Thus, psychological explanations may require explanations that incorporate different-though related- ontological levels. This is exactly the type of explanation provided by hybrid approaches to cognitive modeling, and it is a type of explanation that the mechanistic approach is best able to make sense of.

Ray, Nicholas

The Story Has No Villains: Mach's Critique of Newton **(I.2B)**

Howard Stein famously remarked that Mach's critique of Newtonian spacetime exposed Mach's "abusive empiricism" and "hypercritical" philosophy. This interpretation of Mach's critique still holds much sway. Received wisdom has it that Mach applied an overly reductive form of phenomenalism to Newton's conceptions of absolute space and time, and that, apart from the inspirational influence his severe relationalism may have had on Einstein and the logical empiricists, there is little of inherent philosophical significance in the critique. However, recent interpretations have highlighted the careful ways in which Mach's critique characterises space and time relative to Newton's own view, not phenomenal reductionism. This interpretation calls into question the classical reading of the critique, one that focusses too much on maintaining continuity with Mach's phenomenalism—as developed mainly in his Contributions to the Analysis of Sensations—and not enough on his actual remarks in the Science of Mechanics. We first provide a brief account of the supposed Cartesian and classical empiricist roots of Mach's phenomenal reductionism, and an overview of his critique of absolute space and time. We then argue for two theses: Firstly, that Mach's rejection of Newtonian space and time relies on sophisticated dialectical considerations based on Newton's empiricism (and is therefore not the result of an application of Mach's reductionism); and, secondly, that Mach's phenomenalism should be reconsidered as part of his descriptionism—a reasonable and naturalistic reaction to Planck's characterisation of science as an uncovering of the "blueprint of reality", not a coarse, abusive, or essentially un-scientific philosophy.

Record, Isaac

Who is a suitably prepared model user? **(III.3C)**

Philosophers have recently given much attention to the question of what makes a good scientific model. Leading accounts seek to identify the kind of relationship that holds between elements of the model and those of a target system, for example "structural similarity" or "behavioral similarity." These accounts typically assume that good models unproblematically bear information about the target and that "suitably prepared users" will have no trouble deriving valid inferences about the target system on the basis of this information. Such accounts overlook the distinction between the information contained in the model and that conveyed to the model user. Modelers choose the models they do in part because of the aptness of the selected models to convey appropriate information to model users. Put another way, models function as efficient messengers when model users are suitably prepared to correctly interpret the information encoded in them. What determines the aptness of a given scheme is dependent on the purpose of the scheme and the training, socialization, and native abilities of the users. One useful way to think about user engagement with models is in terms of affordances provided to the user by the model. For example, models afford true and false information, and they afford task-oriented and non-task oriented information. I argue that suitably prepared users are those for whom task-oriented affordances are perceptible and non-task oriented affordances are hidden. Similarly, a good model is one in which true information is perceptible and false information is hidden.

Reynolds, Andrew S.

From Pathways to Networks: Developments in the Science of Intracellular Signalling. **(I.1A)**

In the 1970s scientists began to uncover details of the molecular events involved in the transmission, reception, and transduction of signals which make up the vital business of cell-cell communication. Drawing upon analogies from electronic engineering the sequence of intracellular events ultimately resulting in various changes in cellular behaviour or morphology was described as a 'signal pathway'. In this context of cybernetics and information theory the cell was regarded metaphorically as a little computer with circuits and programs, and scientists were busy trying to map various pathways of importance for understanding development, health and disease in humans and other organisms. Through the 80s and 90s it became clear just how complex the inner workings of a cell are and the widespread occurrence of 'cross-talk' between signal pathways was recognized. As a result the preferred metaphor became one of signaling 'networks'. In the post-genomics era of functional genomics and systems biology this network metaphor has obvious advantages; but it is just as metaphorical as the talk of pathways, or is it? This talk describes the history of the experiments, techniques, and evidence that motivated this shift in language and perspective, and considers the questions: 'When is a scientific metaphor no longer a metaphor?', and 'What are the potential health consequences for humans when biomedical research and drug development rely on inadequate metaphors?'

Richard, Hélène

Innovation et Diffusion des Techniques de Diagnostic Prénatal en France de 1821 à 1970. **(I.2C)**

Le diagnostic prénatal s'est développé au début des années 70 pour répondre à la détresse des couples qui avaient mis au monde un enfant atteint d'une affection grave et qui apprenaient que le risque d'avoir un autre enfant atteint était élevé. »

Cette présentation du diagnostic prénatal est très largement diffusée dans la littérature secondaire. Cette dernière associe plus spécifiquement le diagnostic prénatal au diagnostic des maladies génétiques et chromosomiques du foetus et affirme classiquement que sa naissance correspond au développement des techniques d'imagerie et de biologie moléculaire dans les années 1970.

Néanmoins, nous souhaiterions montrer que ces présupposés sont trompeurs, pour des raisons historiques et épistémologiques. Les examens cliniques, tels le palper abdominal ou l'auscultation des bruits du coeur foetal par le stéthoscope, réalisée dès 1821 par Lejumeau de Kergaradec, se sont diffusés à partir du milieu du XIXe siècle, suivis de la radiographie ou de l'endocrinologie au début du XXe siècle. La présentation classique ignore donc fortement un usage déjà bien établi des techniques de diagnostic prénatal depuis près de 150 ans et en offre une vision très réductrice, le limitant à la recherche de maladies génétiques. L'histoire du diagnostic prénatal en France de 1821 à 1970 nous permettra de revaloriser une riche origine du diagnostic prénatal, offrant une analyse plus complète des techniques comme des indications. Nous montrerons alors que l'essor du diagnostic prénatal est allé de pair avec la constitution de la médecine foetale dès le milieu du XIXe siècle, bien avant les années 1970.

Richter, Adam

The Trinity and the Cube: Nescience in the Epistemology of John Wallis **(II.3B)**

John Wallis (1616-1703) is well known as one of the leading mathematicians and physicists of the Royal Society of London in the seventeenth century. However, few historians have examined Wallis's theology, despite indications of its connections to his mathematics and natural philosophy. For example, he compares the Holy Trinity to the three dimensions of a cube: each dimension is a necessary part of the cube, but none constitutes a cube on its own. Such, in Wallis's conception, is the nature of the Father, Son and Holy Spirit: they are equal and equally necessary parts of God.

Such metaphors reflect the epistemology that these areas of Wallis's thought share: in all fields, he acknowledges the limits of human reason and, accordingly, tolerates a degree of nescience. In his theology, he admits that his metaphors will never correspond exactly to the nature of divine mysteries like the Trinity, since they are beyond human understanding. In his natural philosophy, Wallis takes a similar approach to the ultimate causes of natural phenomena. For instance, he argues that the earth and the moon have a common centre of gravity, but he is not concerned with what exactly connects the two bodies. According to Wallis, the task as a physicist is to recognize natural phenomena, not to explain their ultimate causes which, like the nature of the Trinity, he considers beyond human understanding. Examining this principle may shed light on the epistemological links between theology and the empiricist natural philosophy in which Wallis took part.

Samaroo, Ryan

On Friedman's Characterisation of the Equivalence Principle **(III.1C)**

I will address one aspect of Michael Friedman's recent contribution to the theory of theories. The heart of this contribution, found in a number of works but especially in *Dynamics of Reason* (2001), is Friedman's account of the distinguished epistemological status of certain mathematical and physical presuppositions—what he calls 'constitutive presuppositions'—within a conceptual framework adequate for pursuing the exact sciences.

I will clarify the idea of a constitutive presupposition and the scope of its applicability in philosophical analysis. Then I will turn my attention to Friedman's account of the constitutive presuppositions that form the basis of general relativity, an account on which Riemann's theory of manifolds and the equivalence principle are constitutive presuppositions of Einstein's field equations. In contrast with Friedman's account, I will characterise the role of the equivalence principle, understood in the light of Riemann's theory, as the pivotal step in the reinterpretation of free fall as geodesic motion, and a first step towards treating gravitation as space-time curvature. On my account therefore, the equivalence principle is not a constitutive presupposition but an empirical hypothesis—an inductive generalisation from a set of empirical facts—that motivates a new constitutive presupposition, namely, a new and purely local definition of a geodesic.

Smeenk, Chris

Measurement and Underdetermination **(III.1A)**

The underdetermination argument claims that for any given theory T there is a rival theory T0 such that (i) T and T0 are empirically equivalent — they save the same phenomena; yet (ii) T and T0 are not theoretically equivalent — they represent the world in substantially different ways. The argument turns on conflicting verdicts delivered from two different senses of "equivalence" applied to the rival theories. These conflicting verdicts pose a problem for scientific realists who take empirical success as sufficient grounds for taking a theory to give us a true description of the world. I will reconsider whether we have a drawn a sufficiently clear contrast between the two notions of equivalence. I argue that the empirical content of a theory should be characterized in terms of measurement, in the following sense. Schematically, a "measuring device" M is a physical system whose "meter reading" is correlated with a property of a target system P within a specified domain of applicability. The correlations between M and P, and the situations under which M can serve as a useful measuring device for P's properties, follow from the description of the two systems and their interaction within a given theory. Taking the empirical content of a theory to consist of a set of possible measurements in this sense is substantially richer than, e.g., requiring merely isomorphism between a model of the theory and a data model. As a result there is not a sufficiently sharp contrast between empirical and theoretical equivalence to support an interesting version of underdetermination.

Stegenga, Jacob

Assessing Medical Evidence **(I.1B)**

‘Quality assessment tools’ (QATs) are used to measure the quality of evidence from medical studies. The first QAT was developed in the 1980s, and now there are dozens available. QATs are designed to take into account various methodological details of medical studies, including randomization, blinding, and other features of studies typically deemed relevant to minimizing bias and error. Although the various QATs on offer differ widely from each other, most represent attempts to satisfy what I call the principal of total complexity of evidence (modifying Carnap’s principle), which holds that when assessing any individual piece of evidence one ought to take into account the relevant features of evidence which bear on how compelling the evidence is. I offer a defense of this principle. Unfortunately, empirical studies suggest that QATs have low inter-rater reliability and low inter-tool reliability. This is an instance of a more general problem I call the underdetermination of evidence by theory (reversing the Duhem-Quine locution).

Disagreements about the strength of a particular piece of evidence can be due to different– but in principle equally good – weightings of the fine-grained methodological featureswhich constitute QATs.

Stuart, Mike

The Cognitive Science of Thought Experiments: Paul Thagard's Skepticism **(I.1C)**

Paul Thagard argues for a skeptical position on the role and epistemological merit of thought experiments. He claims that “the made-up thought experiments favoured by many philosophers are not evidence at all.” This is a serious challenge to the burgeoning field that studies thought experiments, which takes it for granted that thought experiments can provide evidence for claims in both philosophy and science. I dispute his criticisms by first presenting counterarguments and counterexamples to his characterization of thought experiments. Then I directly address the claim that thought experiments are dangerous and misleading, by showing that they actually fit Thagard’s own conception of good science. While I think Thagard is mistaken in rejecting the method of thought experimentation, there is still much in his own work and the work of his colleagues that should guide future research. For example, cognitive science might hold the key to explaining the epistemological role of narrative and emotion in thought experiments. I conclude by discussing these ideas, and examining what consequences they have for the various accounts of thought experiments that we see today.

Tait, Morgan Christopher

On the Philosophical Significance of the Pusey-Barrett-Rudolph Theorem **(III.3B)**

In their important recent paper ‘The quantum state cannot be interpreted statistically’, Mathew Pusey, Jonathan Barrett, and Terry Rudolph show that a certain class of epistemic interpretations of the quantum state is incompatible with the empirical predictions of quantum mechanics. In this talk I examine the philosophical significance of the PBR theorem. I begin by reviewing a useful taxonomy of interpretations of the quantum state introduced by Nicholas Harrigan and Rob Spekkens. I then review the PBR argument and examine the main physical assumptions employed in the theorem. I also examine the implications of this theorem for existing epistemic

interpretations of the quantum state. I show that the PBR theorem does not rule out any of these existing interpretations, all of which reject at least one of the assumptions used to generate the result. Nevertheless, I argue that the PBR result is important in that it forces any epistemic interpretation of the quantum state to clarify which, if any, of the physical assumptions employed in the theorem must be rejected, thereby bringing the space of possible epistemic interpretations into sharper relief.

Taylor, James

***Dark Energy and Empirical Cosmology* (II.1C)**

Thornburg, M. Hayden

***Emergence and Levels in Early System's Thinking* (II.2B)**

I construct a historical narrative tracing the roots of the interdisciplinary field of Complexity Science in order to elaborate the development of the concept of "emergence" in the field. The period that I deal with begins with the formation of the Society of General System Theory in 1954 and extends to the introduction of the "soft systems approach" by Checkland in 1980. Complexity scientists develop quantitative models of self-organizing dynamical processes that have interdisciplinary applications. The notion of "emergence" figures prominently in discussions of complexity in the literature. Broadly speaking, the presence of emergence signifies that a whole system possesses properties that are different than the sum of the properties of its parts. I divide theories of emergence into three distinct traditions of thought, those that construe it as conceptual, inferential, and ontological. I show that the work of early systems theorists, such as Bertalanffy, Boulding, Simon, and Forrester, suggests that emergence is ontological, but with the introduction of the "soft systems" approach by von Foerster and Checkland, emergence begins to be construed as conceptual. Historically, both ontological and conceptual theories of emergence tend to incorporate the view that there are natural levels or organization, and that level relations distinguish types of emergent properties. I argue that this "layered" picture of nature is not essential to the ontological theories of emergence, while it is required by the conceptual theories of emergence. I conclude by presenting reasons to dispense with the layered model and adopt a minimal ontological emergentism.

Torfehnezhad, Parzhad

***Understanding Carnap's Language* (I.3B)**

This article has two goals: first, to illustrate to what exactly the term "linguistic framework" refers; second is to prove that analytic-synthetic distinction, contrary to Quine, is not a fundamental distinction for Carnap. In the course of achieving these goals, I will defend Carnap's position on the concept of linguistic framework as the result of adopting the linguistic doctrine of logical truths and show that the analytic-synthetic distinction is only a relative

distinction which can be considered only inside an accepted framework. I will also show how a linguistic framework receives all of its support from empirical observations and how revision at any level of abstraction is possible accordingly. At the end we will see how Quine's objection cannot be viewed as an objection against Carnap's main points. Quine holds a rather conservative stance against ontology compared to that of Carnap. Quine insists only on one of the two possible ways of constructing a language whereas both ways are equally legitimate for Carnap.

Verreault-Julien, Phillipe

'Models-as-mediators': is there a missing link? **(III.3C)**

Recent analyses of economic models tend to conceive them as mediators between abstract theories and the concrete world (see Knuuttila 2009 and Morrison and Morgan 1999). Models are seen as being independent entities which their function is to serve as tools or instruments of investigation. This conception, quite tied to the actual practice of economists, focuses on modeling as an activity instead of on the properties of models themselves. This account thus analyses models in relation to the cognitive activities scientists are engaged in while constructing and using models. If the insights gained by this conception are certainly valuable, I argue in this presentation that the 'models-as-mediators' view overemphasizes the functions models have for scientists over their informational relations as the primary unit of analysis. Drawing on recent philosophical literature (e.g. Chakravartty 2010, Grüne-Yanoff 2009), I aim to show that informational relations and functions must go hand in hand in order to not miss the mediating link between the world and the models. If models are to be used to learn or make inferences about target systems, we cannot escape from clarifying in virtue of what models can serve those cognitive activities. It is an issue that the 'models-as-mediators' view does, but cannot set aside.

Wiener, Leah

Representing the Health of Aboriginal children at the Spanish River Residential School, Ontario **(III.3A)**

This paper, derived from my previous research on the health of Aboriginal children in mid-northern Ontario during the early twentieth century, analyzes how Department of Indian Affairs officials and contracted teachers reported on and portrayed the health of students at the Spanish River Industrial School, north of Lake Huron. Centered on an analysis of student heights and weights, the paper will show how such measurements may create a distorted view of student health, as perceived by historians or by health inspectors and school officials of that era. I will link these findings to broader trends in representations of the health of Aboriginal peoples in vital statistics and medical journals between approximately 1910 and 1940 to argue that Euro-Canadian physicians and bureaucrats portrayed Aboriginal peoples as a "dying" race, yet put forth conflicting and potentially incriminating data which indicate, to a twenty-first century

observer, that these very doctors and officials were continuously exacerbating the factors that caused and perpetuated high mortality rates among Aboriginal Canadians.

Wilson, Alex

***Is Scientific Knowledge Possible?* (II.3B)**

Science is often considered to be the greatest cognitive achievement, and the greatest generator of knowledge, in the Western World. Moreover, in the current philosophy of science literature, there is much talk of "scientific knowledge". However, unlike the term "knowledge" in philosophy, there are few explicit analyses of scientific knowledge. I consider a sample of three accounts of scientific knowledge given by Stathis Psillos (1999), Alexander Bird (2007), and Bas van Fraassen (1999). Psillos implicitly follows the analytic tradition's justified true belief analysis of knowledge. Bird dramatically departs from this tradition and adopts Timothy Williamson's primitivist account. Bas van Fraassen is the foremost anti-realist philosopher of science, thus his probabilistic, voluntarist view demands attention. After examining and criticizing each position in turn, I argue that none of these philosophers has found a viable account of scientific knowledge. I then suggest that scientific understanding may be a better cognitive goal than knowledge for science. Understanding does not rely on true beliefs, and can better accommodate the non-propositional aspects of science, such as scientific modeling and *fingerspitzengefühl*. However, proponents of scientific understanding, such as Catherine Elgin (2006), claim that some forms of scientific understanding must rely on true beliefs. However, I maintain that scientific understanding *never* has to rely on true beliefs, but only on appearances. Moreover, I do not adopt van Fraassen's (2008) characterization of "apperances" as reality, rather I eschew his commonsense realism and take an agnostic stance as to whether or not we directly perceive reality.

Wong, James

***What is at Stake in Democratizing Knowledge? The Relation Between Knowledge and the Political.* (II.1A)**

In a recent paper Margaret Grebowicz (2007) takes the project of democratizing knowledge by standpoint theorists and feminist epistemologists, in particular Helen Longino (2002), to task because they do not take seriously enough that epistemology is both a rational pursuit as well as a political event. In taking Lyotard's critique of deliberative democracy as her starting point, she contends that democracy may not be the correct concept for thinking through issues of power differences, marginalized communities and resistance in the production of knowledge. The issue for her is justice for the marginalized. Consensus papers over differences and shuts out dissent, and would result in a "closed and totalizing" structure (19). Instead, her account provides not only a "robust notion of knowledge as political" (25) but also allows for the development of a critical consciousness that engages dissenting others in on-going negotiations. I argue that Grebowicz's account faces two sets of difficulties. First, the relation between knowledge and the political is underdeveloped in her account: How do the political interact with knowledge (the epistemological)? Second, Grebowicz's use of Lyotard's critique does not adequately address the

issue of inclusion of others from an epistemological point of view: Are there limits to difference in including dissent in order to make disagreement matter? Grebowicz underestimates the resources available to feminist and social epistemologists, like Longino and Kitcher (2002), to address questions about epistemological marginalization and to allow dissent a central role in a dynamic account of the continual transformation of knowledge.

Wuest, Amy

***Philipp Frank's* Science, Facts, and Values*: Refining the Role of Values in Logical Empiricism* (II.3C)**

It has long been known that the Logical Empiricists allowed extra-scientific and pragmatic values to play a limited role in scientific theorizing. However, in a previously lost manuscript---Science, Facts, and Values--- Philipp Frank presents an account of the interactions between science and values that significantly extends this earlier, more well known aspect of Logical Empiricist thought. I argue that Frank's manuscript offers a new way to understand the aim of logical reconstructions and that it serves to unify that project with the more overly political work of other members of the Vienna Circle, such as Otto Neurath. Using Frank's manuscript as my guide, I will explicate his take on the continual interplay between the content of scientific theories and extra-scientific values. I will pay particular attention to how nonscientific values may enter into scientific discourse. From this, I will argue that the method of logical reconstruction is not just a tool which can be used to explicate scientific practice, but that it also helps to make clear when and how political ideologies have sought influence the direction of scientific work. This not only recasts Logical Empiricism as a socially engaged philosophical programme, but will also demonstrate the relevance of Logical Empiricism to more contemporary discussions about science and values.