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Abstracts / Résumés

Mathieu Albert (University of Toronto) and Anne-Julie Houle (Université de Montréal)

The development of an interdisciplinary research and clinical practice: the case of oncology

Oncology is a medical specialty which is characterised by a strong interdisciplinary dimension. Indeed, oncology is composed of clinicians trained in different specialties – mainly surgery, haematology, internal medicine and radio-oncology – and it is subdivided in three subspecialties – surgical oncology, medical oncology and radio-oncology. Our study aims at shedding light on the factors that have contributed to build oncology as an interdisciplinary specialty.

This preliminary study is based on 9 semi-structured interviews with physicians and clinical scientists working in oncology at the Health Centre of the University of Montreal (CHUM) and the McGill University Health Center. Two main themes were discussed during the interviews: 1) the development of medical technologies, of clinical services and of clinical research in oncology, and 2) the evolution of training programs in oncology.

Analysis of the interviews suggests that the development of interdisciplinarity within oncology lies on the following three factors: 1) the importance given to interdisciplinarity within training programs; 2) the creation of interdisciplinary teams to deliver clinical services; and 3) the growing research collaborations between clinical and fundamental scientists.

The importance given to interdisciplinarity within oncology is a characteristic that not only distinguish it from other medical specialties, but also from different scientific disciplines. Moreover, this characteristic raises some questions about Bourdieu's concept of "field" and Abbott's concept of "jurisdiction". Whereas both these concepts focus on the competition between actors to exercise "power", oncology appears to be a counterexample since its development seems to follow a model more focused on collaboration than competition between actors.

Sean Armstrong (Independent Scholar)

Freud and Hannibal: A contribution to the debate on Freud's vision of psychoanalysis as a reformed version of Judaism

However debatable the current status of psychoanalysis, Freud himself has lost none of his importance for the history of science. Here the question of his relationship to Judaism is an important one, most mysteriously posed in his last major work, *Moses and Monotheism*. Recent commentators have written that, at the end of his life, Freud saw himself as leading psychoanalysis out of the Nazi darkness into a new land in the English-speaking world, and hypothesized that he may have believed that psychoanalysis represented the refined essence of Judaism.

Exploration of Freud's associations with Hannibal, one of his lifelong heroes, tells us something about his view of Judaism, as has been recognized ever since Freud discussed the subject in his letters to Wilhelm Fliess. The paper builds on the existing discussion by analyzing three other references to Hannibal in Freud's later writings that have previously escaped notice. Analysis of these tells us something further about both Freud's general view of life, which can be characterized as "heroic resignation," and how he chose to view Judaism. Specifically, his tendency was always to make the strictly religious side of it as dark as possible, through mentally associating it with the Carthaginian religion followed by Hannibal.

Soren Bangu (University of Toronto)

An Old Problem in a New Setting: Maddy's Mathematical Naturalism and Wigner's Puzzle about Applicability

Does Wigner's old puzzle (Wigner 1967) about the applicability of mathematics to physics pose a problem for Maddy's mathematical naturalism (Maddy 1997)? In this paper I argue that this doctrine faces a number of difficulties in dealing with the puzzle. After I sketch out Maddy's position, Wigner's puzzle and the usual strategies to dissolve it, I show that these strategies are not available to the mathematical naturalist.

The mathematical naturalist holds that the appropriate standards in assessing mathematical theories and concepts are intra-mathematical, i.e. internal to mathematical practice; they arise neither from philosophy, nor from science (Maddy 1997, 184). That is, mathematics, a venerable successful practice deserves to be evaluated, in the spirit underlying all naturalism, in its own terms. Yet many other complex organized practices (astrology, scientology, theology, etc.) could make such a claim, in so far as they are undeniably successful by the standards of the practice itself. So, why adopt mathematical naturalism and not, for instance, astrological naturalism? Maddy's response is that mathematics, unlike astrology (or any other non-scientific practice for that matter), is consistently applied to science, playing a seemingly indispensable role in physics (Maddy 1997, 205). In this paper I show that this response is unconvincing. Since not all mathematics is applied, Maddy's response is prima facie problematic. But, I argue, the difficulty is even deeper. I show that Maddy's position faces a serious problem in gaining epistemological credits for

naturalism even for the portions of mathematics that are applied. The problem is that the mathematical naturalist can't deal with Wigner's old puzzle about the applicability of mathematics to natural science.

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Ori Belkind (University of Richmond)

Newton's Argument for Absolute Space

In this paper I argue that existing reconstructions of Newton's argument for Absolute Space in the *Principia* are flawed. The traditional reading of Newton's argument classifies it as an inference to the best explanation. Since relative accelerations cannot be correlated with inertial effects, it seems as if we have to posit the existence of an unobservable entity, i.e. Absolute Space, in order to explain these inertial effects. Recently, DiSalle argued that this reading is too strong, since Newton is interested in finding a *definition* of true motion that coheres with the laws of motion, not in giving an *explanation* of inertial effects. Moreover, this DiSalle argues that the justification for the definition of true motion is the empirical adequacy of the Newton's Laws of Motion. I give support in my paper to this new interpretation, but argue that it misses an important component. In his argument from the properties and causes of motion, Newton argues in effect that Descartes's *definition* of true, relative motion is inconsistent with his *definition* of Quantity of Motion. Thus, Newton's argument points out a *conceptual* incoherence in Descartes's physics, and introduces Absolute Space in order to render the definition of Quantity of Motion coherent.

Robyn Lynne Bluhm (University of Western Ontario)

Beyond Blobology? - Is Functional Neuroimaging the New Phrenology?

Functional neuroimaging has often been criticized as being merely "the new phrenology," in which bumps on the skull have been replaced by "blobs" of neuronal activity. In both phrenology and neuroimaging, the argument runs, investigators merely look for evidence of areas that might be the seat of – poorly defined – cognitive functions of interest. In large part, however, this criticism is based on PET studies and older fMRI studies, which use the technique of "subtraction analysis" to isolate activity associated with processes of interest by comparing activity during performance of a cognitive task to activity during a baseline state or control task. More recently, statistical techniques have been developed that, according to their proponents, promise to move fMRI analysis "beyond blobology." These techniques analyse patterns of connectivity between brain regions, rather than activity in discrete sets of regions. This paper examines the analogy between phrenology and neuroimaging, suggesting that it highlights some potential problems with neuroimaging studies. I also discuss the claims made in support of the statistical techniques mentioned above; since these analyses take subtraction analyses as their starting point, it is not obvious that they can overcome the limitations of "blobology." I argue, however, that the apparent limitations of neuroimaging experiments can be turned into strengths. For example, variation in results due to different experimental and control tasks can actually help to elucidate brain regions underlying cognitive functions and so lead to a better understanding of both the neurological and the psychological processes being examined.

Cornelius Borck (McGill University)

Constructing the utopian body out of its dismemberment in WWI: Raoul Hausmann's philosophy of prosthetic synaesthesia

Avant-garde cultures of the 1920s, revolving around then-new media, envisioned the fusion of art and technology as a decisive step in the shaping of the "new human," liberated from constraints of nature and tradition. The work of Raoul Hausmann, most widely known for his leading role in Berlin Dada, provides a particularly rich case for analyzing the multivalences of this utopian view of the relationship between body and technology, art and science. For example, the cyborgian visualizations of highly technologized bodies that he created in the new medium of photomontage contrast sharply with his critique of prostheses as a means to fix maimed bodies for more war. A similarly deconstructive strategy can be found in his "optophonetic poetry," a typographic as well as linguistic effort to blur differences between sound and vision. At the same time, he engaged in the construction of machines converting the optical and acoustic to immersive data spaces. This was conceived of as a project designed to augment human perception to hitherto unknown realms. Hausmann's vision of re-engineered human bodies opened up for productive explorations into the epistemology of modern technosciences still to be pursued.

A. Boucenna (Université Ferhat Abbas)
Sur L'origine des chiffres arabes

A travers la pagination d'un manuscrit arabe algérien du début du 19ème siècle nous redécouvrons la forme originale que les chiffres arabes avaient avant de passer en Europe et subir les transformations qui ont donné les chiffres arabes modernes. Cette forme originale, dont l'utilisation a complètement disparu, montre que ces chiffres ont pour origine les lettres arabes. Contrairement à ce que prétendent certaines hypothèses, particulièrement celles qui les présentent comme dérivant de caractères indiens, les 10 chiffres arabes que nous utilisons ne sont en fait que 10 lettres arabes plus ou moins modifiées et données dans l'ordre "Abjadi". L'hypothèse de l'origine indienne des chiffres arabes se révèle une erreur démentie par la forme des chiffres arabes et par la logique de la sociologie droite-gauche de la représentation des nombres et des algorithmes des opérations élémentaires. Les chiffres arabes qui ont beaucoup simplifié l'écriture des nombres et les algorithmes des opérations élémentaires sont nés au Maghreb (Afrique du nord). De Béjaïa (Bougie) ils sont passés en Europe pour donner après évolution les chiffres arabes modernes : 0, 1, 2, 3, 4, 5, 6, 7, 8, 9. Ils sont aussi passés au Moyen Orient (le Mashrek) pour donner après transformations, remise en forme et en ajoutant deux lettres hébraïques, les chiffres arabes "Mashreki" utilisés actuellement au Moyen Orient : ٠ ١ ٢ ٣ ٤ ٥ ٦ ٧ ٨ ٩ .

Robert Brain (University of British Columbia)
Memories of Protoplasm: Doctines of Force in the Cell-State

With but few exceptions, historians of science have ignored the non-darwinian theories of inheritance which proliferated during the late nineteenth-century. Contesting Darwin's pangenesis theory, neo-lamarckian biologists put forward several different analogies between memory and inheritance, including the influential theory of "perigenesis" or wave-generating inheritance. The originator of the argument, physiologist Ewald Hering, contended that vibrations of an external stimulus are transferred to the nervous system and hence to all other organs, especially to the gametes. Ernst Haeckel, E.D. Cope, and others turned this into a theory of cell division and of speciation, arguing that the branching waveform motions imprint themselves in the colloid chemistry of the cell as unconscious memory carried from generation to generation. The broad influence of this theory stemmed from its joining of the study of heredity with the thriving methods of experimental physiology, and more generally, its ability to link evolutionary theory with the science of energy. Furthermore, the perigenesis theory found great appeal among psychologists, anthropologists and aestheticians who sought to examine cultural patterns in terms of the gestural rhythms of psycho-physiology. For these reasons wave-theory of inheritence found one of its most enthusiastic proponents in the young Erwin Schrödinger, and later provided key assumptions in his famous essay, "What is Life?"

Keynyn Brysse (IHPST, University of Toronto)
"Groping in the Dark": The Burgess Shale and Evolutionary Theory

The Burgess Shale, discovered in 1909, has given paleontologists a rare glimpse of what life was like shortly after the Cambrian explosion, the rapid initial diversification of multicellular organisms approximately 540 million years ago. Desmond Collins has identified three "phases of perception"of Cambrian life. During the first phase, from the initial discovery until the mid-1970s, Burgess Shale organisms were classified as primitive members of modern animal groups. In the second phase, covering the next decade until 1985, many Burgess Shale organisms were thought to belong to strange phyla now extinct. The third phase, from the late 1980s to the present, marks a shift back towards phase one, in that many Burgess Shale creatures are now being classified within modern phyla. The evolutionary affiliations of the Burgess Shale organisms, which continue to be highly contested, have deep ramifications for our understanding of the history of life, the nature of diversity, and the mechanisms of evolution. Macroevolutionary theory has itself seen some major shifts in the past century, including the Modern Synthesis of the 1940s and the punctuated equilibrium revolution of the 1970s and 80s. I will examine how Collins's three phases map onto these major changes in evolutionary theory, to show how developments in evolutionary theory have affected paleontologists' interpretation of the Burgess Shale, and how changes in the perception of the Burgess Shale have in turn shaped evolutionary theory.

Gabriele Contessa (London School of Economics)
Contextual Realism: Scientific Models, Representation, and the Breakdown of the Scientific Realism Debate

Philosophers of science today agree that models play a central role in science. According to some, scientific theories are collections of models. According to others, scientific models mediate between abstract scientific theories and the real world by representing aspects or portions of the world. The consequences of these views, however, still have to be fully appreciated. This paper argues that, if models are representations of aspects of the world, then, contrary to what is still widely accepted, there are no *general* answers to questions of realism but only *specific* answers to questions about how realistically specific models represent specific systems for specific purposes.

More precisely, one cannot accept that models are representations of the world and be realist or anti-realist about scientific models in general. First, different models of the same system are more or less realistic representations of the system. Second, even the most realistic model is always, to a certain extent, unrealistic insofar as it contains abstractions and idealisations. Therefore, one can be realist about some aspects of the model (i.e. believe that some aspects of the model represent aspects of the target system realistically) and only partially realist about the model in general. Third, one can be realist about certain aspects of the model when it is used to represent certain systems but not when it is used to represent systems of a different kind.

Dr. Angelo De Bruycker (Catholic University of Leuven)
Jesuits doing mathematics in the seventeenth-century Spanish Netherlands

In the first half of the seventeenth century the Jesuits became the key players on the scientific scene in the Spanish Netherlands. In the sixteenth century, the only two figures around which some mathematical (teaching) activity had been taken place were Gemma Frisius and Adriaan Van Roomen, both renowned professors at the university of Leuven. The Flemish Jesuits did something completely new: in 1617 they founded a special course of mathematics. This kind of course had never been offered by the university. A reappraisal of the specific conditions and underlying motives of the inauguration of this mathematics course shows that this course cannot be reduced to a mere copy of Clavius's Academy in Rome (back then the epicentre of Jesuit teaching). Nor can the course be considered to be a straightforward implementation of the *Ratio Studiorum*, promulgated in 1599. It appears that the foundation and the organisation of this special course was not merely the result of some general decrees taken by the General of the Jesuit Order, but rather responded to characteristic contextual impulses. We will give some features of this special course of mathematics, with special attention to the tuition of Joannes Ciermans (1602-1648), one of its most remarkable professors.

This local study fits in the renewed attention for the positive aspects of Jesuit science and it responds to the leading French historian Antonella Romano's appeal for more evidence-based studies of the Jesuit teaching of the mathematical sciences throughout early modern Europe.

James Delbourgo (McGill University)
Colonial Collecting: Science and Slavery in Hans Sloane's Natural History of Jamaica

In discussing a well-known eighteenth-century ethnographic object, a historian of slavery recently noted that an Asante drum had "made its way into Hans Sloane's collection in London." This is an interesting use of the passive tense, because it is physically impossible for inanimate objects to move themselves, as scholars of museology and ethnography have emphatically shown. This paper examines the social practices by which medical and ethnographic information about African slaves was actively collected by Sloane on his Jamaican voyage of 1687-1689. Best known as the "great collector" who posthumously founded the British Museum in 1753, Sloane traveled to Jamaica with the incoming governor, Christopher Monck, Duke of Albemarle, and remained on the island for fifteen months, working as a physician and naturalist. He collected 800 plant specimens, and ultimately published the *Natural History of Jamaica* (1707-1725), a 2-volume work regularly cited as evidence of the conditions and culture of Afro-Caribbean life on the cusp of Jamaica's turn to slavery in the late 17th century. But what kind of "science" *was* Sloane's early modern African ethnography in an era without coherent concepts of "race"? Although virtually ignored by scholars, Sloane is a pivotal figure at the intersection of histories of science, enlightenment, slavery and empire in Britain, who became himself a slave-owner with investments in sugar and cocoa. The interest of this paper thus turns on beginning to excavate the colonial origins of an iconic enlightened career, by connecting early modern ethnographic, medical and natural-historical description to histories of Atlantic commerce, enslavement, collection and display.

Michael J. DeMoor (Institute for Christian Studies, Toronto)

Friedman's Neo-Kantianism and Quinean Holism: Possibilities for Rapproachment

Michael Friedman, in *Dynamics of Reason*, makes the case for understanding the history of science in terms of certain a priori constitutive principles. That is to say, he argues that a proper understanding of scientific theories requires viewing certain coordinating principles not just as necessary parts of a theory but as constitutive for it. As such, he develops and revises Reichenbach and Carnap's neo-Kantianian project, which tries to give an account of the possibility of meaningful scientific discourse in terms of revisable constitutive principles. Friedman interprets this project as fundamentally opposed to Quinean holism which, he believes, treats all parts of a theory as "symmetrically functioning elements of a larger conjunction." This paper is an attempt to show that Friedman's project does not in fact run afoul of Quinean holism, and this for two reasons. Firstly, unlike Carnap's, Friedman's understanding of constitutive principles does not rely on the analytic/synthetic split, which Quine rejects so forcefully. Second, Quine's holistic understanding of language and its empirical significance is more nuanced than Friedman believes and actually provides the resources for an account of the coordinating principles that Friedman insists are necessary and constitutive for theorizing.

Mina Kleiche Dray (Institut de Recherche pour le Développement, Paris)

Impact des interactions entre université/Etat/entreprise dans la construction de la communauté des chimistes au Mexique

La question qui semble à jour aujourd'hui pour les politiques s'occupant de la science au Mexique est de mieux faire interagir le milieu universitaire avec le milieu de l'entreprise. Mais celle-ci n'est pas nouvelle, elle est apparue dès le début du siècle quand le pays a pris conscience des potentialités économiques des matières premières dont il était doté, à savoir d'abord ses ressources minières puis son pétrole. La chimie a alors été considérée comme la science centrale pouvant aider à exprimer économiquement ces potentialités. Celle-ci s'est alors développée dans le pays en rapport avec plusieurs disciplines académique - la chimie des substances naturelles, l'ingénierie chimique et les domaines des sciences chimiques plus industrielles comme les polymères, la catalyse –en harmonie avec les usagers, les « repreneurs » de résultats comme les entreprises pharmaceutiques (SYNTEX, PROVIMEX) et l'industrie du pétrole (PEMEX puis l'IMP).

Que s'est-il passé par la suite pour en arriver au constat d'aujourd'hui ? On sait que dans les années 1970, sous le gouvernement d'Echeveria, la création du CONACYT (Conseil National pour la Science et la Technologie), a donné une impulsion au développement d'une science académique dans les universités avec un grand appui de l'Etat. De quelle manière, alors l'engagement de l'Etat a agi sur ce lien qui semble avoir été tissé de façon naturelle et organique entre l'entreprise et la recherche scientifique dans les années 1950 ?

L'objectif de notre communication est d'établir les premiers éléments de réflexion sur les dynamiques de construction de la communauté des chimistes au Mexique à travers le récit de la vie politique mexicaine depuis le début du siècle.

Ruthanna Dyer (York University)

Reconstitution of the Organism: The Organism as a Research Technology

Charles Manning Child (1869-1954) used *Tubularia*, a colonial hydrozoan, as one of his early research systems to investigate the process of regeneration or reconstitution in his terminology. Child's use of radially symmetrical "persons" within a linear colony contributed to his field theory of gradients related to functional differences within the organism. The significance of Child's choice of *Tubularia* and other hydrozoans on the design of reconstitution experiments appears to have been central to the development of concepts of regulation of metabolism and development in organisms. Child also based his concept of the "individual" on observations of reconstitution and agametic reproduction in the hydrozoans in response to environmental changes. The effect of using a colonial organism composed of multiple "organ/individuals" and a predictable pattern formation based on nodal and axial growth will be examined with respect to Child's theses of developmental and metabolic fields as well as his later work on communication within the organism and interactions between organisms. Child is probably remembered best for his work on *Planaria*, but his work on *Tubularia* appears to have established the framework for reconstitution experiments on the flatworms.

Martin Fichman (York University) and Jennifer Keelan (JFK School of Government)
Resister's Logic: The Anti-vaccination arguments of Alfred Russel Wallace and their role in the debates over compulsory vaccination in England, 1870-1900

From the 1870s onward, Alfred Russel Wallace launched himself into the centre of a politicised and polarised debate over the unpopular compulsory vaccination laws in England. Drawing upon years of intensive research into the question of vaccination's effectiveness and his own original statistical work on the issue, Wallace argued compellingly against vaccination (particularly with regard to smallpox). Other Victorian scientific figures such as Charles Creighton (1847-1927) — the most famous Victorian epidemiologist — and the eminent pathologist Edgar March Crookshank (1858-1928) joined Wallace in demonstrating that widely-held views on the effectiveness of vaccination and evidence for immunity were inconclusive in the light of (then) contemporary standards of evidence.

In this paper we provide the first detailed examination of statistics as a key component of Victorian anti-vaccination arguments—arguments that have been downplayed or neglected by medical historians because of the impressive advances in twentieth century immune therapies. In particular, we examine Wallace's and his colleagues' scientific and sociopolitical deconstruction of late nineteenth-century vaccination science and epidemiology. These arguments have been reborn in the modern biopolitics of universal vaccination. Scientific and popular sentiment against vaccination (as *the* method to combat contagious disease) and arguments structurally similar to Wallace's and his contemporaries' have resurfaced since the 1980s. We suggest that there are strong parallels in the way in which a given society perceives the ecology of disease and the scientific and popular enunciation of suspicions of the underlying rationale of universal vaccination and its safety.

Doreen Fraser (University of Pittsburgh)
A Dilemma of Theory Choice for Philosophers of Quantum Field Theory

One respect in which relativistic quantum field theory (QFT) differs from ordinary, non-relativistic quantum mechanics is that there exist different formulations of the theory. In this talk, I investigate the implications of this state of affairs for the philosophy of QFT. The existence of alternative formulations of QFT poses a prima facie challenge for the philosophical project of interpreting QFT because different formulations support different ontologies (e.g., they disagree about whether quantum particles exist). It is not possible to appeal to empirical success as a criterion for choosing among the alternative formulations because they all yield the same set of experimental predictions. More precisely, this is an instance of strong underdetermination: the choice among formulations is underdetermined by all possible experimental evidence. I argue that the way out of this conundrum is to recognize that, nevertheless, we do have good reasons to prefer one formulation of the theory over the others. Consistency plays a central role in this argument.

Benny Goldberg (University of Pittsburgh)
Early Modern Reproductive Anatomy and the One-Sex Model: A Case Study of the Work of Regnier de Graaf

Thomas Laqueur has proposed in a number of places (see, especially, Lacquer 1990) that there was a one-sex model of reproductive anatomy up until the Enlightenment. The one-sex model represents the dominant ideology and construed the male and female bodies as hierarchically aligned, women being lower than men; that is, women were but inferior analogues of men. In this paper I hope to add my voice to a number of important criticisms of Laqueur, and I argue that there was, in truth, no one-sex model. This paper explores an alternate way of viewing reproductive anatomy through a detailed case study of the work of Regnier de Graaf (17^{th} century). The goal is to provide a more nuanced and historically contextualized view of anatomical practice and theory as it was actually carried out at the time. In doing so, I propose a number of factors that might explain why de Graaf held the views he did, and, by extension, would also explain other anatomical practice including that which Laqueur labels as the one-sex model. These factors are the following: (1) *Natural Theology*; (2) *Humanism*; (3) *Analogies and the Microcosm/Macrocosm*; (4) *Function, Morphology and Etymology*; and (5) *Empirical Discovery*.

Edward Halper (University of Georgia)
Hegel's Criticism of Newton's Mechanics

Hegel's criticism of Newton's mechanics in his Philosophy of Nature is nearly always dismissed as scientifically incompetent and philosophically bizarre. In this paper, I argue that Hegel not only discovered a real contradiction in Newton, but proposed a solution that carried the day in its tenor if not in its substance. Hegel's criticism is that the inertness of matter presumed in

Newton's three laws is inconsistent with the active character that the law of gravity ascribes to matter. The usual objections to this criticism—they are many!— are undermined by understanding his peculiar terminology (which was also that of Newton and Kant) and by recognizing that Hegel is not rejecting Newton's method of calculating the path of body subject to both inertia and gravity. Hegel's concern is rather with the nature of matter, a subject about which Newton pointedly refused to hypothesize. He thinks Newton is implicitly committed to contradictory notions of matter, and that a consistent treatment of matter shows that curved (specifically, elliptical) motion is essential to it, rather than rectilinear motion, as Newton presumes. Hegel points to the solar system as a place where this natural motion manifests itself; the (Bohr) atom is a more contemporary example. For us, what is really significant is Hegel's idea that matter must by its very nature be active. This is, arguably, the insight embodied in Einstein's identification of matter as a form of energy and in general relativity. Particularly interesting are Hegel's a priori arguments that matter is active, rather than inert, his insistence that matter has a unitary nature, and his notion that the nature of matter is important for science and, thus, that a priori metaphysics can advance science.

Michelle Hoffman (IHPST, University of Toronto)

Factoring History into the Equation: History of Science in Ontario High School Physics Textbooks, 1911-present

Does the history of science belong in science textbooks? Can history of science profitably be used as a pedagogical tool in science education? It is a long-debated question. In *The Structure of Scientific Revolutions*, Thomas Kuhn famously suggested that the history of science and science teaching do not easily mix, arguing that textbooks regularly distort and revise the history of science. Stephen G. Brush has likewise argued that textbooks *must* distort the history of science because they can only use stories that support their learning objectives. The stories that don't, says Brush, get "rated X." But several historians of science and educators today, such as Michael R. Matthews and Arthur Stinner, maintain that science teaching can and should be informed by case studies from the history of science.

My paper considers the changing uses of history of science in four widely-used Ontario physics textbooks: *The Ontario High School Physics* (1911), *PSSC Physics* (1960), *Physics: A Human Endeavour* (a Canadian adaptation of *Harvard Project Physics*, published in 1970) and *McGraw-Hill Ryerson Physics 12* (2002). My survey of these textbooks highlights some of the challenges of integrating history of science into the science curriculum. I argue that the varying extent to which these textbooks have emphasized (or ignored) the history of science reflects changing views about the goals of science teaching and the nature of science itself. Finally, I suggest that history of science can indeed play a valuable – but ultimately peripheral – role in science textbooks.

Jeremy Howick (London School of Economics)

Placebo Controls: Epistemic Virtue or Vice?

Randomized, placebo-controlled trials are often considered the gold standard for medical research. However, placebo controls have been challenged on ethical and practical grounds. I argue that, in addition to cases where placebo controls may be unethical or impractical, there are cases where placebo controls are impossible to construct. In particular, treatments whose characteristic effects are not readily distinguished from their incidental effects do not lend themselves to being imitated by placebos.

Attempts to test these treatments against placebos lead to misguided accusations of methodological errors such as unblinding, and false negative results. If I am correct, then for at least a certain class of treatments, requiring placebo controls is an epistemological vice. Treatments that are impossible to test in conventional placebo-controlled trials include surgery, psychotherapy, acupuncture, and exercise

I critically evaluate attempts to solve the problem with placebo controls in studies of complex treatments (Kirsch 2005; Paterson and Dieppe 2005), and conclude that they are all lacking. I then argue for a method that takes the strengths of the suggested methods into account while avoiding their problems. I suggest replacing placebo-controlled trials of certain treatments with comparative trials. A comparative trial compares one treatment against another treatment instead of a placebo.

Robert G. Hudson (University of Saskatchewan)

Dark Matter and Realism

My goal in this paper is to examine the reasoning used by contemporary astrophysicists to argue for the claim that the universe contains dark matter, specifically in the form of WIMPs (Weakly Interacting Massive Particles). Contemporary astrophysicists, as I reveal, are (theoretical) realists in their inquiries into dark matter, and I explore some of the reasons why they are so inclined. Philosophically, one of the main supports on which astrophysicist adhere to realism is their allegiance to a specific metaphysical vantage-point, one they describe as ‘Copernican’. Essentially, Copernicanism stands for a meta-theoretical perspective which expresses a preference for theoretical accounts of empirical phenomena that make such phenomena appear inevitable given past history. This Copernican viewpoint plays an explicit role in theoretical arguments for the claim that the universe has critical density, a claim that in turn plays a role in establishing the existence of dark matter. It is also instrumental in the specific arguments astrophysicists provide for the existence of WIMPs.

I contend that, from the perspective of Copernicanism, astrophysicists assert a realist commitment with their results, and I set myself the task of examining the nature and significance of this (Copernican) realist commitment whilst drawing connections to recent philosophic approaches to realism. My conclusion is that Copernican approaches to realism are overly narrow and that they wrongly conflate epistemic and ontological constraints on theorizing.

Alexandre Korolev (University of British Columbia)

Indeterminism, Acausality, and Time Irreversibility in Classical Physics

Classical physics is often taken to be a paradigm example of a fully deterministic physical theory that never violates our intuitions about causality and determinism, or violates them only in the most extreme circumstances which render such situations as plainly unphysical. A number of authors have argued that this is not so, and that classical physics is a poor choice of hunting ground for such beliefs (Earman 1986). A recent proposal of John Norton (2003) presents a simple Newtonian system that seems to exhibit stochastic acausal behaviour in that it allows generation of spontaneous motion of a mass without any external intervention or any change in the physical environment. This system is of particular interest since, unlike most of Earman's examples, it does not seem to involve any singularities, wild divergences, or any other bullying with infinities of physically meaningful parameters. I show that the existence of anomalous non-trivial solutions in this case is due to the violation of an often overlooked and yet important Lipschitz condition and discuss its physical significance. I show that the Lipschitz condition is closely linked with the time reversibility (or markovianity) of certain solutions in Newtonian mechanics so that the failure to incorporate this condition within Newtonian mechanics may unsurprisingly lead to physically impossible solutions that have no serious metaphysical import, as, for instance, in Norton's causal skeptical anti-fundamentalist program.

Henry Kreuzman (The College of Wooster)

Where, When, Who, and What: The Empirical Nature of Alexander Gordon’s Arguments that Childbed Fever is Infectious

This paper argues that Alexander Gordon (1752-1799) had a series of empirical arguments that lead him to the conclusion that puerperal fever is an infectious disease. He thus provides a case study of the shift during the 18th century to an empirically based approach to medicine.

Gordon was the physician for the Aberdeen Public Dispensary and treated many of the women who became ill during the epidemic of childbed fever that extended from 1789-1792. This paper first examines the geographic distribution of cases of childbed fever by plotting each case on the 1789 Milne map of Aberdeen. The geographic distribution led Gordon to reject the miasma theory and to seek an alternative causal explanation. A second important observation was when the illness arose. He observed that the onset of the fever never preceded and always followed childbirth and thus focused upon this time as the causal interval. A third and fourth line of argumentation emerges from the detailed records that he kept as the physician for the Public Dispensary. As a result of these records, he observed a pattern linking specific midwives and physicians with who become ill, and he concluded that it was a disease carried by midwives and physicians from woman to woman. Finally, he observed a correlation between the number of cases of puerperal fever and erysipelas (i.e., a recognized infectious fever) and noted that a surgeon can acquire an inflammation and fever as the result of a scratch during a dissection of a patient who died of puerperal fever. As a result of this empirical line of inquiry, Gordon concluded that childbed fever is an infectious disease carried by midwives and physicians.

Daryn Lehoux (University of Manchester)

Eyes and Observations in Ancient Science

In trying to work out the causes of rainbows, Aristotle *(Met.* III.4) argues that air, if condensed, can reflect vision. In what initially appears to be an aside to this argument, he mentions that air can also act as a mirror in the case of people with weak vision: their eyes are not strong enough to push their sight through the resisting air, and so their vision is reflected back on itself, which causes the unfortunate sufferer to see an image of himself reflected just in front of his eyes, wherever he goes.

The claim, to be charitable, is a little *surprising* to the modern reader. Nevertheless, we can ask what kind of work the story is doing for Aristotle and why he thought it to be true. Attempting to answer these questions in the framework of the different ancient visual theories raises some important corollary issues around ancient theories of optics and of perception. What emerges is that, by the Hellenistic period, we see a very tight interdependence of the mechanics of vision (in optics) and theories of perception (in medicine and psychology), where writers such as Ptolemy and Galen are also conscious of methodological and epistemological limits determined in part by the use of *known observed phenomena* as part of the evidential basis for the establishment of a theoretical understanding of vision and perception itself. The ways in which this evidence, the known observed phenomena, are handled in the development of different theories of vision and perception will offer us a window into the sophisticated interdetermination of observation, theory, and epistemology in ancient science.

Nicolas Lesté-Lasserre (l'École des Hautes Études Sciences Sociales)
L'astronomie pratique au 18ème siècle, une forme d'aristocratie

De la naissance de l'astronomie moderne dans le dernier quart du 17ème siècle et jusqu'à une répartition des tâches plus rationnelle au début du 19ème siècle, les astronomes observateurs ont bien sûr développé des méthodes adaptées aux différents instruments, mais aussi des outils de justification de leur travail, tant dans un souci d'objectivité que de statut social. Parce que l'observation astronomique est très rarement reproductible et très souvent faite sans possibilité de témoignage, le savant doit utiliser un arsenal d'armes rhétoriques et de savoir-faire pratiques plus ou moins reconnus. Ce déploiement a donc pour but de faire accepter comme valides ses observations, mais aussi parfois d'affirmer son statut d'observateur, avec les qualités qui se rattachent à l'idée qu'en a l'ensemble de la communauté savante.

Par une étude minutieuse des journaux d'observation du français Jean-Baptiste Chappe lors de ses observations en Californie en 1769, il est possible de dégager une volonté assez marquée de l'acteur concernant son statut de chef d'expédition mais aussi – et de façon plus originale – son statut de savant observateur. Tous les arguments que Chappe nous livre pour démontrer la qualité et la validité de ses observations peuvent, conjointement à d'autres éléments d'apparence plus anodine, être interprétés comme une affirmation de son statut aristocratique d'observateur de la nature, mettant en avant ses qualités toutes personnelles et apposant un sceau quasi charnel au travail effectué.

Matthew D. Lund (Rowan University)
Beyond the 'Discovery Machine': Analogy as a Logic of Discovery

Nearly all major schools of thought within philosophy of science from the middle of the nineteenth century until the present time have been unified in the belief that there is no logical or rational way of coming up with new hypotheses in science. Thinkers as diverse as Whewell, Mill, Einstein, Hempel, Popper, and Kuhn were all in agreement in rejecting the notion of a logic of discovery. This rejection, however, was guided more by preconception than by explicit argument. Some of the main presuppositions made about the logic of discovery are that it should be infallible, productive of unique results, context-independent, a mechanical procedure, and that it gives an exhaustive account of how all theories are generated. It is argued that these requirements are far too stringent and that defenders of the logic of discovery (Aristotle, Peirce, and Hanson) were not committed to such a rigorous logic. I argue that replacing these rigid requirements with a weaker set (inspired by the thinking of Peirce, Hanson, cognitive psychology and AI) generates a general pattern of inquiry useful and rigorous enough to qualify as a logic of discovery. I argue that analogical thinking constitutes a logic of discovery. Historical case studies dealing with Franklin, Ampère, and Kepler make clear the productive role of analogy in scientific thinking. I will further demonstrate that the analogues from which we can most profitably generate candidate inferences are at the basic level of abstraction (Rosch). Finally, I utilize a recent *in vivo* study (Dunbar 1994) to draw conclusions about the fruitfulness and proper limits of analogical reasoning in research science.

J. J. MacIntosh (University of Calgary)
Models, Method, and Explanation in the Early Modern Period: 4 case studies

In this paper I consider the explanatory role of models in early modern natural philosophy by looking at four central cases:

Descartes, Boyle and Hobbes on the spring of the air;
Digby et al. and Boyle on heat and cold;
Descartes, Boyle and Hooke on perception; and
Marten on the germ theory of disease.

Did models in the early modern period have explanatory power or not? Were they taken literally or not? Did they have an heuristic function or not? Consideration of these four cases (along with a brief look at some others) leads to the conclusion that the answer to each half of these interrogative disjunctions is yes (depending on the model, and the modeller, in question). In addition to looking at the primary sources, I draw on Alan Gabbey's helpful threefold distinction among explanatory types as set out in "Mechanical Philosophies and their Explanations" (in *Late Medieval and Early Modern Corpuscular Matter Theories* (Leiden: Brill, 2001), eds. Lüthy, Murdoch and Newman).

Dan McArthur (York University)

Discovery, Theory Change and Structural Realism

I this paper I consider two recent and prominent accounts of scientific discovery, Robert Hudson's and Peter Achinstein's. I assess their relative success and I show that while both approaches provide some promising similarities they do not finally address the concerns of the discovery sceptic who denies that single individuals can be uniquely identified as a discoverer or that any one instant counts as a unique discovery event. To this I add the novel objection that these accounts of discovery sometimes provide misleading analyses about who ought to be credited as a discoverer. In the final sections of the paper I work out some revisions to the accounts of discovery that I discuss by drawing from a so-called structural realist view of theory change. Finally, I try to show how such a modified account of discovery can answer critics who are sceptical of discovery claims, such as Musgrave or Wolgar, without producing misleading analyses about who ought to receive credit as a discoverer.

Patrick McCray (University of California-Santa Barbara) and Robert W. Smith (University of Alberta)

Building the Next Big Machine

Very large-scale machine-centered projects have been a key feature of the physical sciences in the post-World War II world. Costing hundreds of millions or even billions of dollars and engaging the efforts of armies of scientists and engineers, the biggest sorts of these projects have typically taken decades to bring into being, although the journey from conception to completion has usually been fraught with assorted difficulties that have led on occasion to a project's demise. The most striking example of this sort of turn of events is the cancellation in 1993 of the Superconducting Supercollider, a high-energy physics accelerator, after over $4 Billion had been spent on its design and construction.The scientific and engineering communities engaged in such enterprises have also faced a number of critical and sensitive issues beyond keeping their project alive. A key one is when to start serious design work on new machines that will replace those in operation, being built, or being planned. Given the very long lead times from conception to operation, scientists have often wanted or been forced to begin developing the `next generation' machine years before securing any scientific results from the one under construction, results that of course might well have the potential to shape or revise design decisions. In this paper, we will examine these issues by focusing on a very large-scale project now called the `James Webb Space Telescope,' or JWST, although it was known for many years as the Next Generation Space Telescope or NGST as it was initially conceived of in the 1980s as a successor to the Hubble Space Telescope, which itself did not begin its scientific operations in orbit around the Earth until 1990.

Jeffrey K. McDonough (Harvard University)

Leibniz's Two Realms Revisited

In his attempt to reconcile piety and the new science, teleology and mechanism, final causation and efficient causation, Leibniz often speaks of there being two realms – a "kingdom of power or efficient causes" and "a kingdom of wisdom or final causes." In this essay, I explore Leibniz's attempt to apply this doctrine to the natural world. The essay falls into three main parts. The first part looks to Leibniz's much neglected work in optics for the roots of his view that the world can be seen as being governed by two complete sets of equipotent laws. The second part offers an account of how this picture of lawful over-determination is to be reconciled with Leibniz's mature metaphysics. Finally, the third part addresses a line of objection proposed by David Hirschmann to the effect that Leibniz's two realms doctrine as applied to the physical world undermines his stated commitment to an efficient, broadly mechanical, account of the natural world. The essay closes with some brief remarks concerning the relationship between Leibniz's conception of lawful overdetermination and Spinoza's thesis that God (or nature) can be conceived through more than one attribute.

Letitia Meynell (Dalhousie University)
The facts about pictures: A response to Perini

In "The Truth in Pictures" (2005), Laura Perini argues that any theory of the epistemic efficacy of scientific images must start with a theory explaining how pictures can be true. She attempts to provide such an account, using Alfred Tarski's theory of truth (1958) and Nelson Goodman's theory of representation (1968). It is Perini's use of Goodman's theory, which characterizes all representations, pictorial or otherwise, as symbol systems, that appears particularly problematic. Three of Perini's central examples will be the focus of my critique. A graph and a chemical diagram are taken as exemplars of the type of Goodmanian symbol system that can be parsed, translated into natural language and assessed. An electron micrograph in contrast, is treated as a worrying but marginal case. Though it speaks against her theory it is not taken as sufficiently significant to warrant its rejection. I argue that Perini has inversed the significance of these images. Far from marginal, the electron micrograph is a type of representation that is paradigmatically pictorial. It has spatial properties that 'resemble' those of the represented objects—not accidentally, but crucially—and is thus fundamentally distinct from Perini's other examples, wherein the spatial properties of graph/diagram are arbitrary with respect to the object depicted. Indeed, I will show that Perini's graph (and arguably the diagram also) are not pictorial in any interesting sense. The analytic tools developed by John Willats (1997) provide both the groundwork for the critique and offer the promise of a more empirically grounded account of the function of pictures in science.

Goodman, Nelson (1968), *Languages of Art: An Approach to a Theory of Symbols*. New York: Bobbs-Merrill.
Perini, Laura (2005), "The Truth in Pictures," *Philosophy of Science* 72: 262-285.
Tarski, Alfred (1956), "The Concept of Truth in Formalized Languages", in *Logic, Semantics,Mathematics*. Oxford: Clarendon Press.
Willats, John (1997), *Art and Representation: New Principles in the Analysis of Pictures.* Princeton: Princeton University.

Susan Mills (University of Pennsylvania)
Teleology and the Healthy Human Body

The history of medicine champions Descartes as one of the most influential founders of the mechanistic turn that occurred in medicine in the seventeenth century. A central idea of Descartes' mechanism is that the organization and operations of the human body be explained solely in terms of matter and its modal variations. Teleology is notably and intentionally absent from Descartes' account of the body "machine." In contrast, teleology has a prominent place in the medical theories of Descartes' predecessors. Specifically, the association of teleology and health establishes that the healthy state of the human body is also the end state that the parts and activities of the body really are for the sake of achieving. That is to say, the end of medicine – the health of the human body – corresponds to the end of the human body. In this paper, I shall consider two types of teleological accounts of the human body, namely, intentional teleology and immanent teleology. It is of note that Descartes explicitly rejects both of these types of teleology in his treatment of the human body. Moreover, I shall examine the role that these kinds of teleology have in the medical theories of two of Descartes' predecessors: Galen, who draws upon intentional teleology in his accounts of the healthy human body; and Harvey, for whom the health of the human body accords with an immanent teleological end state of the body.

Matthias Neuber (University of Tübingen)
Psychology Meets Physics: Schlick's "Method of Coincidences" and Its Scientific Background

Schlick's "method of coincidences" [*Methode der Koinzidenzen*] is commonly regarded as an outcome of his reception of Einstein's general theory of relativity. This view seems plausible insofar as the method of coincidences was initially put forward in Schlick's celebrated *Space and Time in Contemporary Physics* (1917), the overriding aim of which is to elucidate the philosophical implications of Einstein's relativistic conception of space and time. Moreover, Einstein himself made use of the coincidence concept in his 1916 article on the "foundation" of general relativity, implying that space-time coincidences – such as the intersection of possible world lines – determine the invariant content of a generally covariant space-time theory. Yet, focusing attention exclusively on relativistic physics leads to a serious distortion of the real scientific background and the actual philosophical significance of the coincidence method. For there was another inspiring – and systematically equally important – influence on Schlick's attempt to make coincidence the fundamental ingredient of a "general theory of knowledge" – namely, psychological researches into spatial perception, especially during the second half of the 19th century. I shall argue that without its origin in 19th century psychology (and sensory physiology) Schlick's method of coincidences – and with it his whole approach toward the "problem of space" – would never have been developed. In particular, I shall point out that it was principally the writings of Wilhelm Wundt and William James that set the stage for Schlick''s attempt to show how the "objective space" of physics can be constructed out of the "subjective spaces" of psychology.

Kathleen Okruhlik (University of Western Ontario)
Conventions, Bifurcations, and Constitutive Principles

It has become almost commonplace in the past two decades to gesture toward certain similarities between Carnap's L-principles and Kuhnian paradigms. Without claiming that there are no similarities worth remarking upon, this paper focuses instead on critical differences that are sometimes elided in the recent literature–differences that are critical to a proper understanding of central issues in history and philosophy of science.

I examine different meanings of 'conventional' and 'constitutive' as applied to framework principles of various kinds, taking issue at some points with Michael Friedman's treatment of similar questions. The emphasis placed by many logical empiricists on the role played by *decisions* in science plays a key role in the discussion, leading to an examination of the relationship between conventionalism and epistemic voluntarism. Issues around the historicity and revisability of various kinds of framework principles also play a role here, as does Hans Reichenbach's distinction between 'conventions' and 'bifurcations'. Lurking in the background is Kant's synthetic a priori, which functions as model or foil for more recent innovations.

Allan Olley (IHPST, University of Toronto)
Savour of Extravagance: Punched Card Accounting Machines in Science, 1928-1945

Before the advent of electronic computers, electro-mechanical punched card accounting machines were a way to partially automate calculations. Originally invented to speed up the US Census of 1890 they were widely used in business by 1928. Despite this these machines were not commonly used in complex scientific calculation, in 1928. Drawing from published accounts of the scientific work that was done with these machines, from 1928 to 1945, the advantages and disadvantages of the machines for this work will be discussed. I will also draw on letters and other archival sources to show the diffusion of this practice to new fields. In the beginning the majority of the scientific work was in Celestial Mechanics done by two men Wallace J. Eckert and Leslie J. Comrie. Eckert and Comrie's work blazed a trail for the spread of these methods and are the prime examples of how scientists used such machines. For example, Eckert's work inspired the use of a punched card installation by the Los Alamos division of the Manhattan Project. However, the methods were slow to catch on before World War II. Cost was probably a decisive factor judging from the difference in extent of punched card work done by the frugal Comrie versus Eckert who benefited from the philanthropy of IBM President Thomas J. Watson. Further indication of cost as factor is the proliferation of punched card shops during the War as more resources were dedicated to computation.

Trevor Pearce (University of Chicago)
Complication of Circumstances: Darwin and the Economy of Nature

One hundred years before the publication of Darwin's *Origin of Species*, Benjamin Stillingfleet translated and published a collection of dissertations presided over by Carl Linnaeus, entitled *Miscellaneous Tracts Relating to Natural History, Husbandry, and Physick* (1759). Among the dissertations translated was "The Oeconomy of Nature" (1749). This phrase should be familiar to readers of Darwin; it appears in his famous metaphor of "a hundred thousand wedges trying [to] force every kind of adapted structure into the gaps in the oeconomy of Nature" (Notebooks), and also throughout the *Origin*.

In this paper, I will trace the history of the term 'economy of nature' prior to Darwin. In Linneaus, it represents a generalization of the idea of the animal economy to nature as whole in the service of physico-theology. Darwin's immediate sources are the proto-ecological writings of De Candolle and Lyell (the idea of 'stations') as well as Hunter's work on the animal economy. Subsequently, I will show that Darwin begins to use the term 'economy of nature' just as he is first developing his idea of natural selection. In conclusion, I will argue that Darwin's work on nature's economy suggests a reevaluation of his place in the history of ecology.

Slobodan Perovic (St. Mary's University)

Does The Principle Of Natural Selection As A Law Concerning Physical Systems Grant Explanatory Autonomy Of Biology?

A. Rosenberg and D. Kaplan argue that their account of the Principle of Natural Selection (PNS), as a law of physical systems (including those systems studied by biology) underived from other physical laws, provides the precisely explanatory autonomy of biology sought after by antireductionists, without violating the principles of reductive physicalism.

However, I argue that the possibility of the PNS being an underived law of physical systems is neutral to the explanatory autonomy of biology. In fact, if wedded with reductive physicalism (the possibility considered by these authors), it may yield only a very limited explanatory autonomy of biology, no stronger than the quasi-autonomy generally ascribed to it by reductionists. In the physicalist world, the PNS is discoverable as a law only if it is true at all ontological levels (those studied by physics, chemistry, and biology), because the operation of a law concerning higher-level systems (organisms, populations, and species) is grounded in its operation with respect to lower-level systems (atoms and molecules). Consequently, in terms of the explanatory criterion, a generalization discovered by biologists may be established as a law only if its status is confirmed in the form of its applicability to molecular and other systems studied by chemistry and physics. Otherwise, there is a danger that it could be a ‘just so story,’ not a genuine law with an explanatory power, which is extendable to lower-level physical systems. The authors’ account provides only an illusory vindication of the explanatory autonomy: in the case of the PNS, although biologists happened to be the first to utilize it, they could not possibly have established it as a law.

I argue that a substantial explanatory autonomy of biology requires an ontological framework that violates the principles of reductive physicalism, where the PNS, or any other biological law, is a basic law of nature in that it is concerned with the entities whose causal power is not reducible to that of the lower-level entities, but it rather determines their behavior. Thus, only if confirmable at the levels higher than the molecular, generalizations discovered by biologists could provide such autonomy.

Mike Pettit (University of Toronto)

Hunting Duck-Rabbits

Historians and philosophers of science frequently use the ambiguous figure known as the duck-rabbit to explain the concept of paradigm shifts. Since being taken up by the philosophers Ludwig Wittgenstein and Thomas Kuhn in the mid-twentieth-century, the duck-rabbit has held a cherished place in the pedagogy of science studies scholars. As an image the duck-rabbit possesses a history that long antedates these canonical representations. Just as scholars have been exploring the historical epistemology of the sciences, this paper will explore the historical epistemology of the discipline of science studies itself. The origin of the duck-rabbit lies in the commercial culture of the late nineteenth-century: it first appeared in a satirical magazine in Germany to amusingly depict the similitude between the two creatures. I trace how it entered the purview of the sciences through the popular essays of the Wisconsin psychologist Joseph Jastrow at the turn-of-the-twentieth-century. To make sense of the duck-rabbit one has to understand the historical traffic between commercial culture and experimental psychology in optical illusions: images that were simultaneously edifying and entertaining. Once established as a scientific object, the duck-rabbit was circulated back into commercial culture. For example, the psychologist Walter Dill Scott used the image, among other illusions, to illustrate psychological principles to members of the advertising industry. The image became a tool for the commercial management of perception. What do these earlier interpretations tell us about our own evidence for incommensurability?

Francis Remedios (University of Leuven)

Mirowski’s Social and Philosophy of Science

Mirowski’s “The Scientific Dimensions of Social Knowledge and their Distant Echoes in 20th-Century American Philosophy of Science,” (2004) argues that philosophy of science’s self proclaimed turn to the social dimensions of scientific knowledge by Longino and Kitcher has been a sham. Hands, in his response to Miroswki, distinguishes three senses of “social” in Mirowski’s paper:

- the social as the Kuhnian historical dimension;
- the social as what is constitutive of the practices of an organized activity of inquiry;
- and the social as the dimension of political intervention broadly conceived.

For Hands, Mirowski’s concern lies with a still deeper sense of the social that is not yet addressed by the respondents to his above paper. This sense of the social is that the science has become globalized and commercialized, which this paper addresses.

Since Mirowski’s paper is a response to Howard’s “Two Left Turns Make a Right,” (2003), which asks why philosophy of science became disconnected from politics in the course of its professionalization, I start with Howard’s paper. Then, I link

Howard's paper's major theme to Reisch's *How the Cold War Transformed Philosophy of Science* (2005), which argues that the Cold War depoliticized logical empiricism in North America. Finally, I argue that Fuller's notions issues of governance of science and the commercialization of science address Mirowski's concerns. Fuller has a two stage model where discoveries are left to the private sector and justification and distribution of scientific knowledge is the job of universities. Fuller also talks about a constitution of science which would provide background conditions where competition can occur. I provide criticisms of Fuller.

Andrew Reynolds (Cape Breton University)
From Elementary Organisms to Chemical Factories: Changing Metaphors of the Cell

From the mid-nineteenth century through to the early twentieth century the metaphor of the cell as an elementary organism enjoyed considerable popularity among biologists. But gradually, as increasing numbers of researchers adopted a biochemical approach to the problems of biology and cytology (in particular the physiology of metabolism), the metaphor of the cell as a chemical laboratory or factory came to dominate, as it continues to do today. This paper explores some of the factors which motivated this change, the assumptions upon which it rested and the research questions it helped to promote. Some of the topics to be discussed include debates over holistic and reductionistic forms of explanation, and morphological versus experimental approaches in biology. As an endnote, I will discuss how the biotech industry has recently literalized the factory metaphor by turning yeast and bacterial cells into factories for the production of various profitable organic molecules. What began then as a suggestive image for a new experimental approach to cytology ends as a dead metaphor and a potentially lucrative economic opportunity.

Jesse Richmond (University of California, San Diego)
Digging for Agency: Explaining Historical Change in 20th C. Paleoanthropology

The only professional historian of science to have directed significant attention to the study of human evolution in the latter 19th and earlier 20th centuries has argued that the theoretical aspect of this field, as opposed to the fossil evidence, ought to be the focus of the historian's investigations. I argue that this subordination of fossils to theories reflects a model of historical change in science that locates agency *a priori* in the minds of scientists to the exclusion the rest of the world. Further, I argue that no change in "theory" alone can explain why over the course of the 20th C. scientists came to see Africa, rather than Europe or Asia, as the primary theatre of human evolution. In order to achieve a better explanation of this transformative process, the historian must characterize the roles played by a variety of agents, and portray their interactions without deciding beforehand that one sort of entity will necessarily dominate the others. In pursuance of this end, this paper will explore the roles of fossils in making Africa the center of the paleoanthropological world.

Guillaume Rochefort-Maranda (University of Bristol)
Rasmussen on the Construction of Facts

In his conceptual analysis of the constructionist trend in sociology and philosophy of science, Ian Hacking (Hacking, 1999) unearths three theses to which every constructionist is more or less committed: *a)* the contingency of the results given by a successful science, *b)* nominalism and *c)* external explanations of the stability of scientific knowledge as opposed to internal ones. Here I shall use Hacking's results as a method of analysis and expound the in-and-outs of Nicolas Rasmussen's thesis on the distinction between facts and artefacts. In two of his articles (Rasmussen, 1993; 2001) Rasmussen sets forth a series of arguments in favour of the possibility of constructionism in science based on a controversy concerning the ontological status of mesosomes.

I shall begin with a brief presentation of constructionism followed by a fictional debate which will hopefully clarify Rasmussen's position and maybe explain where he is coming from with his philosophical analysis of the mesosomes debate. Second I shall expound Rasmussen's positive arguments. Third, I shall analyse his critical arguments. Fourth, I shall make an improvement on Rasmussen's critical arguments. Fifth, I shall discuss the possibility of an anti-constructionists reply as envisaged by Rasmussen. Finally, I shall explain why, and in this agreeing with Hacking, I think the debate between constructionists and their antagonists is irresoluble.

Dana Rovang (University of Chicago)

"The bird wants to fish": free-will, species development, and educational movements in early nineteenth-century Britain

Prior to the nineteenth-century, conceptions of free-will were confined to the domains of philosophy and theology. However, notions about free-will were expanded with the work of Erasmus Darwin and Jean-Baptist Lamarck, taking on new import when placed within developments in biology and natural philosophy. Although Lamarck never strictly advocated the will as a causative agent in species development, radicals from England in the 1820s and 30s were exposed to his theories while studying under Etienne Geoffroy Saint-Hilaire in Paris, and returned to Britain with theories of transmutation that energized radical movements that promoted education of the lower classes. Consequently, the subject of will with respect to education and social status was taken up by natural philosophers, politicians, and radicals at the advent of the industrial revolution, and in the wake of political revolutions in France and America, which is also the period when Darwin is formulating his theories. Ideas concerning education were conflated with nascent notions of Empire and global status, and developments in education were instituted "from above" and "from below" to aid in this, giving rise to the archetypes of the hero-industrialist and the "self-made man." As Darwin believed that species change for humans could be effected through education - yet held that free-will was a "delusion" - an examination of the social, scientific and political environment when he was writing his notebooks can shed light on a turbulent period that gave rise to Darwin's theories of species development and "moral sense."

Krisanna Scheiter (University of Pennsylvania)

Lewes and the Physiological-Psychological Problem

In this paper I examine a general shift in the philosophy of mind in the 19th century as reflected in the work of George Lewes. The traditional mind-body problem from Descartes into the 19th century is largely concerned with metaphysical questions concerning the immaterial mind and its relation to the material body. During the 19th century, in response to scientific work in physiology and psychology, there is a shift away from these metaphysical concerns. This shift is reflected in the pages of the journal of *Mind.* Lewes is an early scientifically influenced advocate of dual-aspect monism, a position that plays a critical role in the shift from the metaphysical aspect of the mind-body problem to the physiological-psychological problem. Lewes's monism claims that the mental and physical are two aspects of the same 'stuff'. However, his concerns in advocating this position are not primarily ontological. Instead Lewes and others are asking what are the functional dimensions of the mental and what, if any, are the explanatory relations between mental events and physical events (including especially brain events).

David Shein (Bard College)

On Retroactive Reference and Promiscuous Realism

The scientific realist reasons from the success of science to scientific realism: the thesis that the central terms of mature scientific theories genuinely refer and that the sentences of mature scientific theories are true. Laudan's pessimistic induction seeks to undermine this argument by exploiting the historical record of successful but non-referring and false mature theories. One way of responding to this argument is to re-cast the reference assignments of the theories in question so that they refer to entities that we think exist or to recast the accounts of their success to obviate appeal to the non-referring terms. Call this defense of scientific realism *retroactive reference*.[1]

I offer two reasons to reject retroactive reference. First, it is philosophically promiscuous. It allows us to say, for any successful theory that its success is due to whatever entities most resemble those implicated in the theory and which are said to exist by current science. This blanket defense allows us to account for the successes of our theories, but it tells us nothing about the structure of the world or about how our best theories connect to it.

Second, retroactive reference is historically promiscuous. It amounts to the following claim: 'any entity that is similar to that described by the historical theory in question and held to exist by contemporary science is responsible for the success of the theory.' Since this account of theoretic success changes with the ontology of 'contemporary science', all it tells us is that we can manipulate our understanding of past theories to bring them in line with current theorizing. This leaves us incapable of explaining the successes of the historical theories; it reduces historical successes to the successes of contemporary science.

This inability to explain the successes of past theories is a significant problem for realism: a philosophical account of the theory-world relationship ought to account for that relationship's past as well as present. Indeed, the success argument for scientific realism gains much of its force from what it essentially an induction over historical cases: realism is not just the best

[1] There are two kinds of retroactive reference- one that trades on revision of reference assignments and one that trades on a distinction between working (referring) and idle (non-referring) parts of theories. I have in mind both versions.

explanation of current successes, but it is the best explanation of the long history of successes that science has enjoyed. If this history of success becomes inexplicable, the initial reason for adopting realism becomes weakened.

Moreover, this defense of realism is methodologically suspect: it gives mistaken pride of place to the secondary discipline over the primary one. In asking us to re-write history, the realist asks us to ignore actual practice to make sense of a philosophical account of that practice. In getting the order of explanation backwards in this way, the realist is within the grips of a theory. To be sure, that theory is a seductive one, but this is philosophy done in bad faith: our job as philosophers of science is to clarify scientific practice and to help us understand the relation between science and the world, not to force that practice into a philosophic mold.

Martin Thomson-Jones (Oberlin College)

Models and Idealized Systems

As we all know by now, descriptions of idealized systems play a number of central roles in science. In pursuing inquiries in the epistemology and methodology of science, some philosophers speak as though there are systems fitting such descriptions (much as scientists do when doing science) – as though there is such a thing as the simple pendulum, and it fits the descriptions we find in our classical mechanics texts. Though we might think it harmless, I argue that this practice can get us into trouble all too easily. I first show that, precisely because it employs such talk, Giere's (1988, 1999) account of models and theory structure is inconsistent when taken at face value, and that the obvious ways of interpreting the account so as to avoid inconsistency all result in significant losses of attractiveness or plausibility. Suppe's (1989) account of the methodology of science and Achinstein's (1968) taxonomy of models can then readily be seen to be afflicted by similar difficulties.

I lay out a range of implications for the evaluation of Giere's "constructive realism" (1988, 1999), his account of scientific representation (2004), and the methodological accounts Giere and Suppe have offered, and for our understanding of the semantic view and the notion of model. Engaging in this sort of critique also serves to underscore the importance of finding an explicit and defensible way of understanding descriptions of idealized systems. With that goal in mind, I consider the idea that such descriptions should be thought of as miniature fictions.

Jonathan Y. Tsou (University of Chicago)

Hacking on the Looping Effects of Psychiatric Categories: What is an Interactive and Indifferent Kind?

In a series of papers, Ian Hacking (1995a; 1995b, ch. 2; 1999, ch. 4; 2002, ch. 6) has described a phenomenon that he calls the "looping effects of human kinds." Generally, looping refers to instances wherein the meaning of a human science category (e.g., "multiple personality disorder" or "schizophrenia") affects the behaviors of individuals who are classified as such (e.g., a person diagnosed with multiple personality disorder acts in accordance with expectations fostered by that classification). Much of Hacking's discussion has been made with reference to psychiatry. The aim of this paper is to examine and evaluate Hacking's treatment of psychiatric categories and, in particular, his claim that some "psychiatric kinds" are both interactive and indifferent. I begin by explicating Hacking's general account of looping, focusing on his discussion of schizophrenia and depression. Subsequently, I argue that Hacking is not entitled to claim that some psychiatric disorders (e.g., autism and schizophrenia) are interactive and indifferent kinds given the way that he introduces his interactive-indifferent distinction (viz., as a mutually exclusive distinction). After attempting to resolve this incoherence in Hacking's account, I address some issues concerning the types of "kinds" investigated in psychiatry.

K. Brad Wray (State University of New York, Oswego)

Defending a Selectionist Explanation for the Success of Science

Van Fraassen (1980) develops a selectionist explanation for the success of science. He argues that our current best theories enable us to make accurate predictions because they have been subjected to a selection process similar to the process of natural selection operative in the biological world. Unsuccessful theories have been eliminated.

Van Fraassen's explanation has been criticized on a number of grounds. In an effort to deepen our understanding of the power of a selectionist explanation for the success of science, I review and address criticisms raised by Blackburn (2005), Lipton (2004), Psillos (1999), Kitcher (1993), and Musgrave (1988). The critics are concerned that van Fraassen does not explain (i) why we have any successful theories at all, (ii) why we should expect theories that enabled us to make accurate predictions in the past to afford accurate predictions in the future, and (iii) what is common to all theories that are predictively accurate. I argue that the plausibility of the competing explanation, the realists' explanation, rests on an inaccurate understanding of the nature of predictive success. The predictive success of our best theories is a relative success, relative to changing standards, and relative to the existing competitors.

Organizer: Brian Baigrie (IHPST, University of Toronto)
Novel Prediction in Historical Practice

This session will explore the value placed on novel prediction in the context of scientific practice. The question of the proposed session is this: Do theories that advance novel predictions, that are subsequently corroborated, have greater epistemic value than rival theories that provide 'mere' empirical accommodation of extant data? Many philosophers – perhaps the majority of those who are interested in this question - want to hold that theories that merely accommodate extent data should be seen to enjoy less empirical support. Other philosophers, who are in the minority, object that a novel prediction that is corroborated by experience testifies that the associated theory is consistent with the data and, to this extent, that this theory is in the same epistemic position as its rival that merely accommodates extent data.

Philosophers have traditionally looked at novel prediction sui generis to bolster a philosophical take on the sciences, such as scientific realism. Few have been interested in novel prediction for its own sake, exploring the annals of science for clues as to the value placed on novel prediction in scientific practice. This session will explore the place (presence or absence) of novel predictions in historical practice with the aim of teasing out some of the consequences, if any, for some current philosophical views about the value of novel predictions vs. empirical accommodation.

Brian Baigrie (IHPST, Toronto)
Novel Prediction and mid-19th Century Chemistry

Paul Thompson (IHPST, Toronto)
Novel Prediction and Evolutionary Biology

James Robert Brown (Toronto)
Novel Prediction and Particle Physics

Alison Wylie (Washington)
Commentary

Organizer: Marionne Cronin (IHPST, University of Toronto)
Whistling Past the Graveyard: Obsolescence and Canadian Technology

As each technology nears the end of its historical life, it enters into a form of what John Staudenmaier terms senility. Though it may continue to function as it ever did, its users now see it as obsolete and choose to adopt new technologies in its place. This replacement of old technologies by new is often treated as purely a matter of technical advance, improved function, or greater efficiency. However, obsolescence is intriguing because it is a condition that is not necessarily internal to the technology. It also reflects the judgements of technology's users about what constitutes a good technology, judgements that often reflect a complex network of interactions between social, political, economic, and technological factors that lead to technologies and technological artefacts being declared obsolete. Investigating this topic can offer significant new insights into the important relationship between technologies and their ambient contexts, demonstrating that historical circumstances have a role to play throughout a technology's life cycle.

While the concept is treated briefly by scholars such as Staudenmaier and David Hounshell, the history of technology remains preoccupied with the earlier stages of a technology's life, namely invention, innovation and adoption, diffusion, or adaptation. An investigation of obsolescence prompts us to ask, how does a technology move from this initial stage, from being innovative, to being obsolete? Drawing on examples from the history of 20th-century Canadian technology, this session will explore the nature of technological obsolescence, the process by which a technology becomes obsolete, and address the often neglected subject of how technologies decline.

Scott Campbell (IHPST, Toronto)
Why Johnny Couldn't Program: Obsolescence and the History of Computing

A new technology is never embraced immediately by all users. Instead, the uptake follows a curve from early to late adopters. Historians have studied the advantages of being a first-mover, but rarely do they consider the users who take up a technology long after others have done so. For example, in the immediate years following the invention of modern computers most scientists and engineers continued to use older, simpler, and cheaper techniques to solve numerical problems impossible by analytical means. Even though it was widely acknowledged that electronic computers which could produce solutions in seconds had rendered these techniques obsolete, most people continued to rely on mechanical desktop calculators, numerical tables, and mental stamina to calculate answers over periods of weeks or months. The first decade of electronic computing had little effect on the daily computational needs of scientists, engineers, and mathematicians. For them, until the 1960s, a computer was still a person. This story can be partially explained with some straightforward reasons, but interesting aspects also exist that can shine a light on the meaning of obsolescence. I will explore the phenomenon of late adoption using detailed examples taken from my research on the history of computer science in Canada.

Marionne Cronin (IHPST, Toronto)
Fading Away: Technological Decline in Canadian Aviation

When Western Canada Airways began flying its pioneering air route down the Mackenzie Valley in 1929, it did so with a fleet comprised entirely of Fokker Super Universal aircraft. By the end of 1933, however, the airline had declared the Super Universals obsolete, replacing them with other aircraft models. In only four years, the Fokker Super Universal had gone from state of the art to obsolescent. Exploring Western Canada Airways' decision to replace the Supers, this paper offers insight into the nature of obsolescence, demonstrating that the aircraft did not achieve this status because of technical changes or dramatic technical breakthroughs in the design of other aircraft. Instead, the Super Universal became obsolete because of changes in its use-context. The rise of the Great Bear Lake mineral rush, the introduction of new sorts of aircraft into the region, increased competition, and changing passenger expectations all encouraged Western Canada Airways' management to re-evaluate their definition of what constituted a superior technology within the context of the Mackenzie Valley. While conditions in 1929 made the Super Universal the best possible choice, by the end of 1933 those circumstances had changed such that the Super was now obsolete. By tracing this episode in Canadian aviation history, this paper will explore the contextual dimensions of obsolescence and the role of historical circumstances in effecting technological decline.

Jonathan Turner (IHPST, Toronto)
The Arrow of Progress: The Canadian Aerospace Industry in the Cold War Arms Race

During the nuclear arms race, new weapons systems were constantly implemented. A common assumption of historians is that each new system surpassed the old one in both military and technical specifications, and the old weapons system was declared obsolete. For instance, because of intercontinental ballistic missiles, bombers were declared obsolete as nuclear payloads could now be delivered faster and more accurately. With heavy bombers went the fighter-interceptors designed to engage them. For example, the Avro Arrow program, which is thought to be the best all-weather fighter-interceptor ever imagined, was cancelled by John Diefenbaker in 1959. Obsolescence was the main factor cited by Diefenbaker in the program's cancellation. However, a close analysis of the Diefenbaker era, based on Jon Mclin's work, reveals a more complicated picture of the obsolescence of weapons systems in the Cold War. Politics, foreign relations, economics, strategic planning, social pressures, military factors, and purely technical considerations all play a role in rendering different weapons systems obsolete.

Organizer: Mélanie Frappier (MSU–Mankato)
Reconsidering Einstein's Evidence for Relativity

William Harper (Western)
Turning Data into Evidence

Isaac Newton's argument for universal gravity exemplifies a method that adds features which can significantly enrich the basic hypothetico-deductive model that informed much of philosophy of science in the last century. On this familiar basic H-D model, hypothesized principles are tested by experimental verification of observable conclusions drawn from them. Empirical success is limited to accurate prediction of observable phenomena. What it leads are confirmations taken to legitimate increases in probability.

We shall see that Newton's inferences from phenomena realize an ideal of empirical success that is richer than prediction. To achieve this richer sort of success a theory needs to, not only, accurately predict the phenomena it purports to explain, but also, to have those phenomena accurately measure the parameters which explain them. 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. We shall see that, contrary to Thomas Kuhn, Newton's method applies to endorse the radical theoretical transformation from Newton's theory to Einstein's. We shall also see that this rich empirical method of Newton's is strikingly realized in the development and application of testing frameworks for relativistic theories of gravity today.

Mélanie Frappier (MSU-Mankato)
Thought Experiments and Empirical Adequacy: Einstein and Poincaré on Distant Clocks Synchronization

In their respective defense of a "semi-classical" and a relativistic interpretation of the Lorentz transformations, Poincaré and Einstein appealed to the "same" thought experiment about distant clock synchronization. Whereas Poincaré interpreted the results of the thought experiment through local time and the Lorentz-FitzGerald contraction, Einstein used it to defend the relativity of simultaneity. From similar cases of thought experiments reanalyzed from different, incompatible perspectives in modern physics, Bokulich (2001) concludes that thought experiments differ from ordinary physical experiments with respect to their evaluative function. She believes that, while ordinary experiments essentially aim at testing the empirical adequacy of theories, the goal of thought experiments is to test for theoretical consistency. By contrast, I claim that, althought the main aim of thought experiments is to evaluate theoretical consistency, they are also sometimes used to assess the empirical adequacy of a theory. It is true that, when thought experiments are used to test for the intratheoretic consistency of a theory, they do not bring further empirical justification to the theory. But thought experiments can also be used to test for the intertheoretic consistency existing between a new theory and a well-established one. In such cases, thought experiments can be viewed as indirect arguments in favor of the empirical adequacy of a new physical theory: the new theory's adequacy with known physical phenomena being guaranteed through its consistency with the older, better tested theory. This double evaluative function can, of course, be played by the "same" thought experiment as illustrated here by Poincaré's and Einstein's use of distant clocks synchronization.

Robert DiSalle (Western)
The Philosophical Origins of Relativity Theory, Reconsidered

For much of the 20th century, relativity was seen as a physical theory with a special connection to philosophy. According to its most prominent early interpreters—not only the logical empiricist philosophers of science, but also Einstein himself—the theory arose from Einstein's *epistemological analysis* of the classical conception of time, and the unexamined role it had been playing in classical electrodynamics. In the later 20th century, however, this interpretation came to be seen as rather simplistic. The general reaction against logical positivism, and in particular the influence of Kuhn's view of scientific revolutions, meant that Einstein's revolution would appear to be not so distinct, philosophically, from other shifts in scientific thinking.

This paper reconsiders the philosophical aspect of Einstein's revolution. It contends that Einstein's reasoning did, after all, crucially involve a kind of philosophical analysis. But it was not the sort of epistemological analysis that can be characterized by positivist strictures about meaning and verification, or about the metaphysical excesses of the Newtonian view. Instead, it was a much more complex process of *conceptual analysis*: an analysis not of "the concept of time," but of the ways in which conceptions of motion and descriptions of physical process implicitly depend on assumptions about the measurement of time. The new definition of simultaneity that Einstein introduced, and that formed the basis for special relativity, was not a mere arbitrary stipulation to give empirical meaning to a concept that had had none before. Instead, it showed precisely where the classical notion of simultaneity confronted the empirical facts, and how the new criterion of simultaneity was, already, *implicitly* supplying what the classical notion was lacking. By understanding Einstein's argument in this manner, we can grasp how the theory of relativity could have been, at once, both a philosophical and an empirical argument against the prevailing theory of his time. Moreover, we can articulate more clearly how this radical change of perspective represents, in a perfectly objective sense, both philosophical and scientific progress.

Carrie Klatt (Victorial)
A Non-Empirical Preference for General Relativity

Einstein's theory of general relativity (GR) boasts both empirical success and mathematical elegance. These achievements provide good reason, for many, to accept the rather radical account of space and time that GR presents—namely, that the phenomenon of gravity results, not from a force field, but from a four-dimensional curved spacetime. However, alternative representations that depict gravity as an independent field existing within a flat spacetime background can account for many of the same empirical observations (e.g. Feynman[1995], Schiff[1960]). Given this apparent redundancy, it is reasonable to ask which account of space and time is to be preferred. In this paper, I argue that there are *non-empirical* grounds on which to prefer GR's curved spacetime representation of gravity. I will contrast how the deflection of light due to gravity is explained in both GR and in Schiff's flat-spacetime representation. The difference will be shown to depend on how each theory uses the equivalence principle (EP). In GR the EP plays two distinct roles: first, it permits one to replace free-fall frames of reference with inertial frames. Second, the EP is used to show that gravitational free-fall determines the geometry of spacetime. Schiff's account adopts the former use of the EP while rejecting the latter. As a result, flat-spacetime theories like Schiff's seem contrived because, in not accepting that the EP indicates the nature of spacetime, they not only fail to provide a complete interpretation of the EP but also introduce an arbitrary feature (i.e. a flat-spacetime) into the theory.

Organizer: Delia Gavrus (IHPST, University of Toronto)
Setting Standards? The Increasing Authority of Regulatory Bodies in the Age of Public Health

Beginning in the latter half of the nineteenth century, regulatory bodies were established to set professional standards and guard the interests of the general public. In Ontario, the newly formed College of Physicians and Surgeons was forced to contend with public demand for new and unorthodox therapies, including the treatment of disease by electricity. At this early stage the College was not able to command the authority necessary to limit public access to a therapy they did not sanction. By mid-twentieth century we begin to see an increasingly successful effort to protect public health though standardization and regulation. In 1963 the international standard-setting Codex Alimentarius Commission (CAC) created the Food Hygiene Committee to develop guidelines for food quality and safety. This expert committee, charged with the task of setting scientific standards, attempted to establish itself as the new authority on food safety, and yet, in its early stages, was reliant upon other groups within the CAC, the World Health Organization and the Food and Agriculture Organization for advice. An examination of this process reveals that the setting of standards involved a host of other factors in addition to scientific evidence. In the case of the CAC, lack of authority was one of these factors, just as too much authority became a central factor in the Food and Drug Administration's (FDA) approval of Prozac and the related class of antidepressants. In the last two decades of the twentieth century, the FDA felt it wielded enough power to influence the deliberations of a committee of outside experts in order to guide it toward the approval of these drugs. Through these three case studies, this session will raise questions about the evolving authority of regulatory bodies, as well as the role of scientific evidence in setting health standards.

Vivien Hamilton (IHPST, Toronto)
The Curative Powers of Electricity: Appealing to Public Authority in Victorian Canada

In 1875, shortly after becoming the first woman in Canada to obtain a license to practice medicine, Jenny Kidd Trout began to advertise, "special facilities for giving treatment to ladies by galvanic baths or electricity." These facilities grew into the commanding Toronto Medical and Electro-therapeutic Institute (TMEI), which occupied three city lots, and, at the height of its success, employed two other physicians besides Dr. Trout in a Ladies' and a Men's department. While recent studies have examined the history of electrotherapeutics in both the United States and Britain, no equivalent study exists for Canada. The evidence provided by *The Curative Powers of Electricity Demonstrated,* a pamphlet issued by Dr. Trout's Institute, offers an excellent starting point for an analysis of the status of electricity as a therapy in Victorian Canada. Considering the long-standing suspicion that the application of electricity to the body was simply quack medicine, we might expect the physicians at the TMEI to attempt to establish their legitimacy through association with regular or allopathic medicine. Instead, we find them distancing themselves from that community of practitioners, choosing to appeal directly to the public by employing just the kind of strategies that were guaranteed to lessen their respectability in the eyes of the newly forming medical profession. While the Ontario College of Physicians and Surgeons did exist as a regulating body at this time, the opinion of the consumer offered a powerful, and often opposing, source of authority, allowing practices like the TMEI to carve out a legitimate space for electrotherapeutics in medicine.

Brigit Ramsingh (IHPST, Toronto)
'Fit for Human Consumption:' The Codex Alimentarius Commission and the Development of Food Safety Standards (1963-1970)

The history of the development of international food safety standards was shaped by the *Codex Alimentarius*, or "food code", and its parent standard-setting body the Codex Alimentarius Commission (CAC). The CAC was established in 1963 by the United Nations' Food and Agriculture and World Health Organizations (FAO, WHO). One of the original subsidiary groups of this organization was its Committee on Food Hygiene, chaired by the Government of the United States. This expert committee had a mandate to set microbiological standards and develop a set of general principles for food hygiene, the collection of which first appeared in the *Codex* by 1969, beginning with products like milk, meat, and canned fruit. Despite its apparent success at developing microbiological standards, the Food Hygiene Committee demonstrated a lack of authority. At times, the Committee attempted to refer specific questions and problems to other regulatory bodies recognized as 'experts' by the WHO and FAO, partly because of lack of familiarity with techniques in food microbiology and partly because of organizational problems.

Using committee reports and the general principles for food hygiene as starting points, I examine the context in which these standards were adopted, particularly the food microbiology and the scientific evidence which informed these principles. Furthermore, I reveal the process by which scientific standards were developed, and I explore debates over the role of expert advice and scientific authority within the context the CAC. This case study illustrates how the process of setting standards involves a host of factors in addition to scientific evidence.

Delia Gavrus (IHPST, Toronto)
Asserting Authority: The FDA's Strategy in Approving the New-Generation Antidepressants

Only three years after Prozac was approved by the Food and Drug Administration (FDA) for the treatment of clinical depression, it became the most frequently prescribed psychiatric drug. Recently, however, critics have expressed doubts about its efficacy and the wisdom of prescribing it so generously. Some psychiatrists have argued that Prozac and its sister selective serotonin reuptake inhibitors (SSRIs) are no better than placebos at relieving depression, and historians have explored the socio-political context in which these new drugs were synthesized and marketed. This recent scholarship raises questions about the standards and the evidence required by the FDA, as well as other factors that influence the approval process.

A review of the transcripts of the Psychopharmacological Drugs Advisory Committee meetings in which the FDA presented its case for the approval of Prozac, Zoloft and Paxil sheds light on this process, on the Agency's evidentiary benchmarks for assessing psychopharmacological compounds, and on the role played by Paul Leber, the director of the Division of Neuropharmacological Drug Products. I show that the Advisory Committee, which consisted of outside experts, expressed concerns about the evidence submitted by the pharmaceutical companies, as well as about the FDA's evaluation process. The FDA's requirements were perceived as lax and seemed to be biased in favor of approving new drugs. However, Paul Leber skillfully maneuvered the Committee's discussion away from issues of efficacy and emphasized instead the SSRIs' safety. He defended the Agency's guidelines as reasonable and practical, and he dismissed criticisms as mere attempts at academic idealism.

Thus the key to the story of the SSRIs' approval lies in an interaction between a concern with safety over efficacy, permissive guidelines, and a supportive regulator who facilitated the approval process by asserting authority over what constituted evidence and how this evidence ought to be weighed.

Organizer: Philippe Huneman (CNRS, Paris)
Natural selection and the causes of evolution

Recent debates in philosophy of biology have raised the necessity of a revised analysis of the kind of causation at work within natural selection, and therefore, of the status of causal explanations in evolutionary theory. The role and nature of causation affect many related questions about the nature of evolutionary theory: the controversy over the units of selection, the debate about adaptationnism, the creative vs. merely discriminative character of natural selection, etc. Hence the need for a re-examination of causal claims in evolutionary theory. This session will investigate the levels and status of causation in evolutionary theory, the limits of natural selection as a causal factor, and the causal structure of evolutionary processes.

Frédéric Bouchard (Montréal)
Hierachical selection and the necessity for causal claims about individuals in natural selection.

A consequence of adopting a hierarchical view of selection is that not only interactors will be found at various levels, but replicators as well (i.e. there will be non genic replicators). The fact that replicators could be whole organisms (e.g. some clonal species) demands that we re-examine the relation between individual and population in evolutionary explanations. I will argue that the existence of these clonal replicators recasts the role of the concept of population in evolutionary explanations, thereby justifying a return to the individual (broadly construed) and the causal interactions it has with its environment. Contrary to some recent claims, evolutionary theory cannot do without explicit causal claims about individuals. I will argue that an emphasis on evolutionary biology's purely statistical claims fails to provide an explanatory framework in which we could make sense of the evolution of some plants and ecosystems.

Philippe Huneman (CNRS, Paris)
Natural selection as a cause and as explanation

Natural selection is a selection for effects. Therefore, various causal processes of different kinds operating between several entities become identical regarding to natural selection. This compels us to think that if natural selection is a causal process, it has to supervene on the particular causal processes occurring between organisms and their environment. The issue is then how this supervening causation has to be conceived: whether it should be viewed as a statistical outcome, or as an emerging new causality. To decide, one must elucidate the conception of causation that is needed in order to think this supervenience of causation. I argue first, that a counterfactual account of causation is better than a statistical account or a covering-law account of causation. One main argument here is that natural selection has this distinctive character that it allows non interactive causal relationships upon which it can supervene. Second it is argued that natural selection as a cause is explanatory in such a way that its explanatory relevance is not related to its causal relevance in the same manner than explanatory relevance and causal relevance are bound when it comes to the individual causal processes involving particular organisms. It follows that in order to decide on the causal status of natural selection one has to explicitate what is supposed to be the explananda of natural selection (for ex., origin of traits vs maintenance of traits in organisms vs distribution or traits in populations). The conclusion compares the causal status of drift and selection.

Mohan Matthen (UBC)
On causes of evolution

Darwin saw evolution as the cumulative outcome of numerous individual interactions, but he had no satisfactory way of describing these interactions as aggregates. With the development of statistics throughout the late 19th and early 20th century, Darwin's theory was reformulated in ensemble-level terms. This reformulation brings with it certain perplexities concerning causation. What are the causal relations among ensemble-level terms? Do they replace causal relations among properties of individual organisms? In this paper, a two-level causal scheme is defended and expounded: "process-causation" at the individual level, and "aggregative causation" at the level of ensembles. To preserve certain principles of causation in physics, aggregative causation must be regarded as
nothing more than a mathematical accumulation of process-causes. Some consequences for causation in the theory of natural selection are explored. It is argued, for example, that natural selection and drift are not causes of evolution, and are not distinct processes.

Denis Walsh (IHPST, Toronto)
Dynamical vs statistical interpretation of selection and drift

There are currently two interpretations of the Modern Synthesis theory of evolution: dynamical and statistical. According to the dynamical theory Modern Synthesis explanations--those which use the concepts selection and drift--articulate the causes of evolutionary change. According to the latter drift and selection explanations merely invoke the statistical structures of populations, without identifying the causes of evolutionary change. I adduce some considerations in favour of the statistical interpretation.

Organizer: Trevor Levere (IHPST, University of Toronto)
Natural Philosophers, Physicians, and Industrialists in Eighteenth-century England

This session examines networks in science and medicine, and the crossing of boundaries between medicine and natural history, collections, the laboratory, chemistry, and industry. They do so in political and institutional contexts, and together invite a reassessment of the practice of science and medicine in the eighteenth century.

Robert Iliffe (Imperial College, London)
Medicine, Natural History and Empire in early eighteenth century London

Historians have drawn a great deal of attention to the world-wide networks linking imperial, trading, medical and collecting interests in France and Holland throughout the eighteenth century. In the case of Britain, much has also been written about the effects of similar networks controlled by Joseph Banks (President of the Royal Society 1778-1820) in the last decades of the century. For the earlier part of the century, Newtonianism and the physical sciences have dominated accounts of English natural philosophy. Historians have shown the importance of a new sort of metropolitan natural philosophy and technology, increasingly linked to the ‘market’, which was investigated and demonstrated both inside and outside the orbit of the Royal Society. However, relatively little attention has been paid to the intersections between medicine, collecting and natural history that dominated the period. Led by medical luminaries such as Hans Sloane and Richard Mead, eighteenth century London was the centre of a rapidly expanding empire in which collectors of all sorts played dominant roles. From this perspective, Newton's presidency of the Royal Society (1703-27) was a peculiar interlude in the growing authority in the metropolis by physicians and natural historians, who occupied central places in the Royal Society and indeed in British learned society at large. In this paper I argue that a reassessment of the role of learned physicians in London in the first half of the eighteenth century is overdue, and I attempt to place them in their proper contexts.

Larry Stewart (Saskatchewan)
His Majesty's subjects and the experimental realm of the late eighteenth century.

This paper seeks to explore some of the epistemological issues which issued from the infirmary and the medical laboratory in the late eighteenth century. Deeply antagonistic figures as Joseph Priestley and Edmund Burke ultimately shared an explosive vision of experimental nitre that might bring subvert a British elite. But what was it about the laboratory experience that led to such claims? Most particularly, in this paper I seek to explore the role of humans in the eighteenth-century laboratory, most notably as subjects in and proponents of experimental treatments utilizing electricity and pneumatic medicine that turned many practitioners into reformers. The methods of the republican Priestley, the democratic Doctor Thomas Beddoes and the young James Watt, son of the steam engineer, set off political alarms. Burke had them all in his sights should they dare transgress. By examining the role of subjects who were also patients, and the trials of new treatments that expanded the realm of experimental epistemology, we can begin to grasp the link between experiment, republicanism, and visions of reform.

Trevor Levere (IHPST, Toronto)
Collaborations in Medicine and Science: Thomas Beddoes and the Lunar Society of Birmingham.

James Watt, Matthew Boulton, Josiah Wedgwood, and Erasmus Darwin were all vigorously engaged in the fusion of theoretical, practical, and applied science. The Watts and Wedgwoods were also patients of Thomas Beddoes. Darwin, Watt, and Beddoes worked together in the design of instruments, from gasometers and breathing apparatus for pneumatic medicine to a centrifugal bed for treating consumptive patients. They were all fertile in hypotheses and impatient of disciplinary boundaries. Watt acted as a physician for members of his workforce, sometimes with lethal results; Darwin proposed improvements to Watt's inventions; and Beddoes combined instrument design, chemical practice, and medical theories. In examining such collaborations, which were at the heart of entrepreneurial activity, we can better understand unifying features in science, medicine, and industry in late 18th-century Britain.

Organizer: Sara Scharf (IHPST, University of Toronto)
Paths not taken: inevitability, contingency, and thought experiments in the history of science

Gregory Radick (Leeds)
The factual and the counterfactual in the history of science

An old complaint about counterfactual history is that it is an evidence-free zone. While that may be true for some varieties of history, it is not true for the history of science. In this paper I distinguish three classes of evidence that may be useful in judging the contingency or inevitability of a scientific achievement. First is evidence to do with the convergence of multiple, independent lines of inquiry. Second is evidence to do with divergence, that is, whether lines of inquiry angled away from the eventually successful one were themselves successful. Third is evidence to do with the existence or not of plausible alternatives to the eventually successful theories, techniques, programs etc. In illustrating these points I shall turn to the early history of genetics, in particular the question of whether, had certain events turned out differently in the early years of the twentieth century, there might have been a successful nongenic biology.

Christopher D. Green (York)
What difference would it have made to the history of psychology if the Johns Hopkins Philosophy Department had not imploded in the mid-1880s?

It is well known in history of psychology circles that Granville Stanley Hall – founder of the American Journal of Psychology and soon-to-be founder of the American Psychological Association – left his appointment as professor of philosophy at Johns Hopkins

University in 1888 to take up the presidency of the newly-founded Clark University, leaving the first experimental psychology research laboratory in America closed behind him. Somewhat less well known is that Hall's chief competitor for the Johns Hopkins philosophy professorship had been Charles Sanders Peirce, who was let go from his Johns Hopkins philosophy lectureship in 1884 because of a scandal involving his private life, and almost immediately after having co-authored the first American-published work of experimental psychology. Almost unknown is that another philosophy lecturer at Johns Hopkins, George Sylvester Morris, then supervising John Dewey's doctoral dissertation, was also passed over for the professorship in favor of Hall and soon after left, with Dewey, for the University of Michigan. How would the history of American psychology played out had either Peirce or Morris won the professorship at Johns Hopkins instead of Hall? Would there ever have been an American Journal of Psychology or an American Psychological Association? What sort of impact might Perice's more logico-mathematical approach, or Morris' more Hegelian ethos, come to have on the course American psychology took in the coming decades if either of them had had the Johns Hopkins chair from which to speak? Might pragmatism have come to be known to Americans and the world primarily from Peirce's mouth directly, rather than through William James' interpretation two decades later?

Sara T. Scharf (IHPST, Toronto)
Re-inventing the dud: are theoretical dead ends inevitable?

Priority disputes arising from multiple discoveries of a natural phenomenon or inventions of a technology are a staple of the history of science. Multiple, simultaneous discoveries are usually explained in terms of similarities of social experiences of the inventors, their awareness of previous discoveries, and, in the case of natural phenomena, the reality of the discovered phenomena themselves. But what should we make of non-simultaneous, multiple inventions of a technology that does not work? In the history of botany, six different men independently invented a technique to name plants in a "meaningful" way by having each syllable of a plant's name stand for a different property that it featured. The inventors lived in four different countries, wrote in four different languages, and the first and last lived more than a century apart. The technique never became popular among botanists at large: it did not work as well in practice as it did in theory. But clearly this 'dud' was attractive enough to inspire many different men to invent or claim to have invented it. Was its invention inevitable, given the circumstances the inventors shared?

P. Kyle Stanford (California, Irvine)
Theories of generation and the problem of unconceived alternatives

I have previously argued that the most serious potential challenge to scientific realism arises from what I call the problem of unconceived alternatives: our repeated failure to even conceive of serious alternatives to our best scientific theories that were nonetheless well-supported by the evidence available at the time. Here I argue that this epistemic predicament characterizes the foundational 19th century contributions of Charles Darwin, Francis Galton, and August Weismann towards developing a material theory of inheritance and generation. Each of these foundational theorists put forward a positive theoretical account of the phenomena, but failed even to conceive of an entire class of equally well-confirmed alternatives that would ultimately come to dominate later scientific practice. In particular, Darwin failed to conceive of the possibility that hereditary resemblances between parents and offspring might be the product of a common cause (such as a shared germ line). Galton failed to conceive of the possibility that inherited material might simply direct the formation of rather than mature into the constituent parts of the organism, or that it might act in a contextually determined rather than an invariant fashion. And Weismann failed to conceive of the possibility that the hereditary material is itself an enduring, facultative piece of biological machinery rather than a resource divided and consumed in the course of ontogeny. Each of these failures of theoretical imagination supports the challenge posed by the problem of unconceived alternatives to believing the central theoretical claims of even the most successful contemporary scientific theories.