

Program of the annual meeting of
the Canadian Society for History and Philosophy of Science
University of Toronto, 26-28 May, 2002

Société Canadienne D'Histoire et Philosophie des Sciences
CSHPS
Canadian Society for the History and Philosophy of Science

Annual Meeting / Réunion annuelle

University of Toronto

2002

*Programme de la réunion annuelle de
la Société canadienne d'histoire et de philosophie des sciences
Université de Toronto, 26, 27 et 28 mai 2002*

Canadian Society for the History and
Philosophy of Science

*Société canadienne d'histoire et
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Janis LANGINS

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Philosophy and Physics / *Philosophie et Physique*

(9:00-10:15 Victoria College 323)

William HARPER (University of Western Ontario)
chairman / *président*

Slobodan PEROVIC, *A Historical Reminder to the Proponents of the Many Worlds Interpretation of Quantum Mechanics: Why did Schrödinger take Ill in 1926?* — Upon closer examination Hugh Everett's compelling account of relative state displays some remarkable similarities on fundamental issues to the ideas of Schrödinger proposed at the beginning of the quantum revolution, in 1926. In his 1926 papers E. Schrödinger argued against N. Bohr's discontinuous and indeterministic account of quantum phenomena, suggesting that the atomism of classical mechanics fails for very small dimensions of the path and for very great curvatures. Ray optics and classical mechanics fail in an analogous way. The true laws of quantum mechanics show that the particle cannot be treated as a single unit, but as a manifold of paths. Everett similarly started from the idea of the completeness of the wave mechanical account, and argued that the results of the quantum experiments obtained in classical terms (as single values) are just appearances derivable from the wave mechanical account. Feynman's path integrals approach and Gell-Man's work in the 1950's might prove to be critical steps which encouraged Everett to pursue these ideas. Schrödinger's interpretation however was confuted by Bohr. He agreed with Bohr's experimental critique based on the experiments with Compton's effect and the experiments performed by J. Franck, and Geiger and Bothe that described the observed interactions in terms of ordinary classical particles. I will examine whether Everett's ideas and the ideas of his successors can deal with the experimental evidence that Schrödinger found to be so strong against the universal wave-mechanical account of the physical world that it led him to acknowledge Bohr's points. [Department of Philosophy, York University, 4700 Keele st., Toronto (Ontario) M3J 1P3, Canada. Email: perovic@yorku.ca]

Mélanie FRAPPIER, *The Influence of Hilbert's Programme on Heisenberg's Closed Theories* — This talk explores the relations existing between Hilbert's programme and Heisenberg's theory of theories. In an interview with T.S. Kuhn, Heisenberg claimed that, under Hilbert's influence, many physicists came to believe "that we may be forced to describe nature by means of an axiomatic system which was thoroughly different from the old classical physics." As I show, Hilbert's ideas on axiomatization pervades Heisenberg's thoughts on the role played by mathematical formalism in the definition of theoretical concepts. The influence Hilbert had on Heisenberg is particularly evident in the latter's notion of closed theories, defined as systems of axioms, definitions, and laws giving us an "eternally valid knowledge" of the laws of nature. I also show how Hilbert's influence is present in Heisenberg's concerns about the limits of our "intui-

tion" (*Anschaulichkeit*), in the central role Heisenberg attributes to completeness and consistency in the evaluation of physical theories, and in his belief that, in physics, understanding consists essentially in the knowledge of the relations existing between the different concepts of a theoretical framework, rather than in the direct acquaintance of the things associated with those concepts. I finally argue that the knowledge of the relations existing between the philosophies of the two men should play an important role in our reconstruction of Heisenberg's thoughts on the interpretation of physical theories. [Department of Philosophy, University of Western Ontario, London (Ontario) Canada. Email: mfrappie@uwo.ca]

Letitia MEYNELL, *Why Feynman Diagrams Represent* — Feynman diagrams are a basic tool of the physicist. Invariably, any discussion about strong, weak, or electromagnetic forces at the subatomic scale will produce a Feynman diagram or two, and calculations of probabilities of various kinds are made immeasurably easier with a string of Feynman diagrams keeping track of the terms in the integrand. There are, however, disagreements as to the nature of the role that Feynman diagrams should or do play in physics. Two distinct interpretations are available: Feynman diagrams as calculational devices and Feynman diagrams as representational devices. In this paper I will argue that Feynman diagrams are representations. To avoid possible pitfalls concerning the ambiguity of the term "representation," I will adopt a definition from aesthetics, Kendall Walton's definition. I trust this captures what those who oppose the idea that Feynman diagrams are representations find objectionable about the notion. I will address what I take to be the three main objections to this view. 1. That the paths pictured are incompatible with Heisenberg's uncertainty principle, which suggests that particles cannot have a definite position and momentum simultaneously and thus cannot have a path. 2. That it is problematic having a visual representation of something you necessarily cannot see. 3. That the extensive background knowledge required to read these diagrams suggests that they are not representational. I will conclude by arguing for the compatibility of representational and calculational views, the two uses being distinct, but related functions that Feynman diagrams fulfill. To support this I will offer historical evidence showing that Feynman, when he introduced these diagrams in the late 1940s, seems to have used them in both ways. This alone suggests the compatibility and propriety of such uses. [Department of Philosophy, University of Western Ontario, London (Ontario) N6A 3K7 Canada. Email: lmmeynell@uwo.ca]

Relations Between Academic Knowledge and Industry / *Relations entre les savoirs académique et industriel*

(9:00-10:15 Victoria College 206)

Jean-François AUGER (Université du Québec à Montréal)
chairman / *président*

Hanna E. H. MARTINSEN, *Textile Dyeing: Colour Magic, Dyer's Art or Academic Science* — The myth that the French Gobelin family had made a pact with the Devil to achieve a brilliant scarlet red manifests the concept of textile dyeing as a magical process. Some early dye manuals were written in code and clearly illustrate the secrecy that surrounded both the recipes as well as the dye-methods. Our concept of textile dyeing as a dyer's art are strengthened by Captain Beaulieu's important report from 1735 to M. Dufay, who probably was the French inspector of dye-works, describing methods used in India for mordant printing textiles. However, in the eighteenth century, scientists contributed extensively to further European knowledge of both textile dyeing and printing. The French chemist Claude-Louis Berthollet's work guided the textile bleaching- and dyeing-industries from a craft based tradition towards a science-based industry. His publication, *Éléments de l'art de la teinture*, corresponded with a traditional dye manual and also introduced a scientific evaluation. Berthollet's theoretical research on oxymuriatic acid led to the development of a totally new bleaching process, which reduced bleaching time as well as the demand for bleaching fields. However, he used two respected and successful textile-printing companies to conduct the practical trials; Haussmann's in Logelback outside of Colmar and Oberkampf's in Jouy-en-Josas near Versailles. Both companies had a special connection to academic science; Jean-Michel Haussmann had studied at college des apothicaires in Paris before joining the family factory, while Oberkampf sent his nephew to study with Berthollet and Berthollet's son practiced at Oberkampf's factory. [IHPST, Victoria College, University of Toronto, Toronto (Ontario) Canada. Email: prostar@interlog.com]

Françoise OLIVIER-UTARD, *Le mécénat industriel à la faculté des sciences de Strasbourg (1918-1998): recherche libre ou prestation de services aux entreprises ?* — Après le retour de l'Alsace à la France, en 1918, l'université de Strasbourg sert à la fois de vitrine face à l'Allemagne et de laboratoire d'expériences pour la France. Elle a un rôle moteur original dans la compétition internationale. Le mécénat industriel finance une partie de la recherche libre jusqu'à la crise des années 1930. Le Conseil national de recherches scientifiques (CNRS) prend ensuite le relais du financement de la recherche. Après la guerre, le débat s'articule sur les conflits d'intérêt public et intérêts privés, dans un contexte marqué par la volonté d'indépendance nationale face aux États-Unis. A partir des années 1980 une nouvelle problématique voit le jour, liée aux réseaux d'innovation et

aux transferts de technologie, et inscrite dans un contexte de néolibéralisme plus marqué quoique souvent contesté. Les instituts de chimie et de physique de la faculté des sciences de Strasbourg ont répondu diversement à ces évolutions, en fonction de facteurs locaux mais aussi nationaux, voire internationaux. La communication a pour objectif de décrire et analyser ces deux exemples strasbourgeois, pour apprécier les oscillations du mécénat industriel entre la recherche libre et la prestation de services aux entreprises dans le court XX^e siècle. [IRIST Université Louis-Pasteur, 7, rue de l'Université, 67070 Strasbourg, France. Courriel : francoise.olivier-utard@gersulp.u-strasbg.fr]

Mike ALMEIDA, *Le développement d'un conseil provincial de recherches : l'Office provincial des recherches scientifiques du Québec, 1937-1960* — À sa création en 1937, l'Office provincial des recherches scientifiques avait pour mandat de coordonner la recherche effectuée dans les laboratoires de la province de Québec et de pourvoir à la formation de nouveaux chercheurs. En 1960, à l'époque des grands bouleversements institutionnels qui caractérisent la Révolution tranquille, il est intégré au Bureau des recherches économiques du ministère de l'Industrie et du Commerce. Ainsi, selon Raymond Duchesne, l'Office était un échec relatif dans le développement de la science au Québec ; tandis que pour Yves Gingras, le Conseil national de recherches du Canada (CNR) demeurait l'unique organisme vers lequel les scientifiques francophones pouvaient se tourner pour obtenir des subventions de recherche. Une étude approfondie de l'organisme nous permet de nuancer ces résultats. Bien qu'il n'ait jamais eu l'ampleur ni les moyens du CNR, l'Office a joué un rôle vital dans la constitution de la communauté scientifique francophone. Nous avons découvert que l'Office finança au cours de son existence pas moins de trois cents mémoires de maîtrise et thèses de doctorat, assurant ainsi un recrutement plus régulier d'étudiants des cycles supérieurs, là où s'acquiert le « métier » de chercheur et la connaissance des règles du jeu en vigueur dans chacune des disciplines scientifiques. De plus, l'Office s'est efforcé d'orienter les recherches universitaires vers des applications pratiques à l'industrie, formant donc l'une des premières, sinon la première, structure institutionnelle de liaison entre l'université et l'entreprise privée au Québec. Finalement, il appliquait une politique d'assistance technique et scientifique à la petite et moyenne entreprise. Ces résultats devraient conduire à réévaluer le rôle de l'Office dans le développement institutionnel de la recherche au Québec avant 1960. [CIRST, Université du Québec à Montréal, CP 8888, Succ. Centre-Ville, Montréal (Québec) H3C 3P8, Canada. Courriel : t12619@internet.uqam.ca]

Canadian Society for the History of Medicine AMS Paterson Lecture /
Discours Paterson AMS de la Société canadienne d'histoire de la médecine
(9:00-10:15 Emmanuel College, Electronic Classroom)

Bert HANSEN, *Has the Laboratory been a Closet? Gay and Lesbian Lives in the History of Science and Medicine* — Neither the sexual orientation of scientists nor its potential relevance as a biographical dimension of their work has received much attention from historians or sociologists. Through a series of biographical sketches of researchers and teachers in a range of scientific fields from the 1850s through the 1950s, this illustrated lecture explores what is lost when such concerns are neglected. Fascinating individuals, both famous and obscure, reveal some of the myriad ways that an often hidden queer presence has contributed to anthropology, biology, chemistry, mathematics,

medicine, and the social sciences. These lives were chosen with four goals in mind: (i) to record the stories of a diverse group of exemplary lives; (ii) to make a plea for historians of science and medicine to provide readers a more frank acknowledgment of the potential relevance of personal life to intellectual work, even in the sciences; (iii) to open a scholarly discussion of the varying importance of scientists' private life, by examining several different kinds of interaction; and (iv) to highlight the problems of securing evidence and making nuanced and sound historical interpretation of intimate and sexual matters. [City University of New York]

Break / *Pause* (10:15-10:30)

Method / *Méthode*
(10:30-12:00 Victoria College 323)

Kyle STANFORD (University of California)
chairman / *président*

Robert G. HUDSON, *The Relativity of Novelty* — It is a common view that the confirmation of a hypothesis by experimental evidence is better if this evidence is novel. I think this common view is right, and my goal in this paper is to explain why this is so. The bulk of the paper is taken up addressing two important problems for the use of novelty considerations in science. These two problems result from what I term the “relativity of novelty,” the fact that what counts as novel evidence is relative to a person’s background knowledge. I call these problems the “ignorance problem” and the “biography problem.” According to the ignorance problem, given that ignorance generates proportionately more novel evidence (the less one knows, the more that is new), it appears that ignorance should be valued in science just to the extent that novelty is. According to the biography problem, since the novelty of evidence depends on a person’s state of knowledge, it follows that to evaluate the novelty of evidence and so assess its confirmatory significance we need to engage in various psycho-social investigations that reveal how much background knowledge a person possesses. But surely such investigations are foreign to the normal practice of science. My strategy in responding to these problems is to introduce a new interpretation of novelty (which I call “prima facie heuristic novelty”). After philosophically motivating this new version of novelty and illustrating its use through some historical examples, I argue that this new version of novelty provides an effective remedy to the ignorance and biography problems and thus explains the legitimate role of novelty

considerations in science. [Department of Philosophy, University of Saskatchewan, 9 Campus Drive, Saskatoon, Saskatchewan S7N 5A5, Canada. Email: r.hudson@usask.ca]

Matthew D. LUND, *Theory-Laden Observation and the Logic of Discovery* — Most inquiries into the nature of scientific observation over the past forty years have centered on the objectivity of science. While such discussion has been interesting, the context in which the subject of theory-laden observation (in its present form) originally arose was that of the logic of discovery. These two topics, theory-laden observation and the logic of discovery, have, since Hanson’s *Patterns of Discovery*, ceased to be treated as closely related subjects. I shall argue that separating these issues is a mistake. Hanson’s primary concern in his discussion of scientific observation was not to demonstrate observation’s lack of objectivity, but to elucidate the process through which perception meshes with knowledge: only through the application of concepts to the data of perception are we able to have an epistemologically useful interaction with the world. However, Hanson was never able to produce a fully articulated theory of concepts, and I shall argue that the vitiating theory-ladenness which seemed a consequence of his account was merely a result of his underdeveloped conceptual story. A more detailed and satisfactory account of concepts, and their role in perception, is given by Fred Dretske in *Knowledge and the Flow of Information*. Application of Dretske’s framework to the problems dealt with by Hanson will both demon-

strate the objectivity of observation and afford some positive prospect for a logic of discovery. [Department of Philosophy, 601, S. Morgan St., University of Illinois, Chicago, Chicago, IL 60607-7114, United States of America. Email: mlund@uic.edu]

Kent STALEY, *Demarcation and Severe Testing* — The aims of the largely abandoned demarcation project deserve continued attention, and this essay highlights two of them: the identification of theories worthy of pursuit, and of methods of theory testing and acceptance. My proposals are premised on the assumption that a central aim of a scientific community is the generation of evidence for those theories it employs in giving explanations, and on a “severe testing” account of evidence. First, for a theory to be worth pursuing, I propose that we must have some reason to believe that we can have evidence for the theory. A severe testing account gives this requirement the right degree of stringency. For a test result to

constitute evidence for a theory, it must fit the theory, and the theory must pass a severe test with that result. Thus, a theory is worth scientific pursuit only if it is reasonable to believe that the theory can be severely tested. Second, I propose that a community is conducting itself according to a scientific method with regard to theory acceptance just in case it is acting according to maxims that will allow for the acceptance of a hypothesis only on the condition that the hypothesis has passed a severe test. I then consider obstacles facing my approach concerning its treatment of “high-level” theoretical claims, and assess the prospects for overcoming such obstacles. I conclude that at least, the articulation of a clear theory of evidence can help to force a clarification of the aims of science. [Department of Philosophy, Saint Louis University, 3800 Lindell Blvd., St. Louis MO 63156, United States of America. Email : staleykw@slu.edu]

Early Modern Science / *La science au XVII^e siècle*

(10:30-12:00 Victoria College 206)

Brian BAIGRIE (University of Toronto)
chairman / *président*

Anna Marie ROOS, *Israel Hiebner's Astrological Amulets and the English Sigil War of 1698* — This paper analyzes the role of Israel Hiebner's *Mysterium Sigillorum* (1651) in the astrological reform program of late seventeenth-century English astrologers. Hiebner was a professor of astronomy and mathematics at Erfurt, and the translation of his tract into English in 1698 was considered to be a landmark event among reforming and scientific astrologers such as Henry Coley and John Gadbury. In the face of astrology's declining reputation among learned elites, Coley and Gadbury wished to cleanse their discipline of superstitious dross and illustrate it was “experimentally true” via Baconian induction as well as by incorporation of discoveries in astronomy and natural philosophy. Hiebner's insistence on accurate planetary observations in making astrological sigils, his use of maps in Hevelius' *Selenographia* as guides in stamping his medical amulets, as well as his detailed and precise lists of ascendant planetary aspects thus was appealing to these astrological reformers. This paper also analyzes the role of the *Mysterium Sigillorum* in the “English Sigil War,” a larger debate that existed among astrological physicians and natural philosophy about the role of these medals in medical healing. [Department of Philosophy, University of Saskatchewan, 9 Campus Drive, Saskatoon, Saskatchewan S7N 5A5, Canada. Email: r.hudson@usask.ca]

Stephen D. SNOBELEN, *Newton and Women* — Isaac Newton made two pointed statements on his deathbed in 1727. One came in his refusal to take the sacrament of the Church of England. The other was the confession that he was dying having never known a woman. The first statement likely signalled the heretic Newton's final break with the

Anglican Church. The second was uttered as a final triumph of a scholar who took seriously the purest ideals of the true philosopher to reject carnal and domestic pleasure and withdraw into the world of learning. This paper recovers Newton's espousal of this culture of learned celibacy. Beginning with antecedents in the seventeenth century and earlier, I situate Newton in the context of his time at Cambridge (where Fellows were forbidden to marry) and London (where he remained unmarried), outline his relationships with women and demonstrate how Newton's actions suggest a deliberate strategy of self-denial. I also explicate Newton's actions with illuminating material from his unpublished manuscripts that provide clues as to his views on women, marriage, scholarly discipline and avoiding the temptations of the flesh. Finally, I show how Newton's celibacy, like that of Robert Boyle, exemplifies an insufficiently understood early modern tradition in which the natural philosopher is not only modelled as a “high priest of nature,” but whose body and soul are devoted to the pursuit of learning and the discovery of God's truth in nature. [History of Science and Technology Programme, University of King's College, Halifax (Nova Scotia) B3H 2A1, Canada. Email : snobelen@IS.Dal.Ca]

Kathryn MORRIS, *Lines of Light: Hobbes, Refraction, and the Human Heart* — Like many of his contemporaries, Hobbes was fascinated with optics. He wrote widely on the subject, and his work in this area represents the bulk of his early scientific efforts. This paper will compare two proofs that Hobbes offered for the sine law of refraction. The first proof, which can be found in an early optical treatise (published by Mersenne in 1644 as part of his *Cogitata Physico-Mathematica*), is based on Hobbes's physical expla-

nation of refraction in terms of the changes of speed that light rays undergo when moving from medium to medium. In *De Corpore* (1655) Hobbes presents a much less successful proof, which attempts to demonstrate that the sine law describes not only the behaviour of light, but also that of propagated motion in general. It is difficult to understand why Hobbes, in *De Corpore*, chose to abandon his earlier, more successful approach to the problem of refraction (which had

attracted the attention of Robert Hooke, among others). I will argue that this puzzling shift was due to his determination to develop a comprehensive philosophical system and, in particular, to his growing conviction that optics could provide a crucial link between his physics and his account of human nature. [Early Modern Studies Programme, University of King's College, Halifax (Nova Scotia) B3H 2A1, Canada. Email: kmorris@Admin.UKings.NS.Ca]

Private Passions - Public Pursuits / *Passions privées et intérêts publics*

(10:45-12:00 Emmanuel College, Electronic Classroom)

Alison LI (York University)
chairman / *président*

Martin FICHMAN, *Private Passions/Public Pursuits: A Study of the Controversial Career of Alfred Russel Wallace* — The eminent position of Alfred Russel Wallace (1823-1913) in the history of evolutionary science is indisputable. But Wallace chose to go beyond what were already emerging as the professional disciplinary boundaries of science, and participated actively in controversial social, political, philosophical, and religious movements. This combination of left-wing politics and a theistic, teleological world view has made it all too easy for his contemporaries, as well as most recent historians, to view Wallace as a bit of an oddball—but brilliant—scientist, who lapsed unfortunately into spiritualism, theism, and radical cultural activism. Yet Wallace's life belies such a facile categorization of polarities. The main theme developed in my paper is that there is an identifiable unity and coherence in Wallace's thought and life. This unity, coupled with Wallace's passion to understand and change the world into which he was born, helps explain his multifaceted and, frequently, highly controversial career. My central theme is that Wallace used evolutionary theory as a springboard for an innovative exploration of the human condition. It is Wallace's unabashed desire to incorporate science into the broader cultural context that makes his career so intriguing—and illuminates certain of our own contemporary concerns about the intertexture of science and society. [Division of Humanities, Bethune College 313, York University, 4700 Keele St., Toronto (Ontario) M3J 1P3, Canada. Email : mfichman@yorku.ca]

Janet GODDEN and Carol HEMSTADTER, *Nightingale Nursing in the Colonies: Conflict in the Victorian Ideology of Class and Gender* — The Nightingale Fund sent only two teams of the Nightingale trained nurses to the colonies. Lucy Osburn was the Lady Superintendent of a team sent to the Sydney Infirmary (later Sydney Hospital), Australia in 1868, and Maria Machin was the Lady Superintendent of the team sent to the Montreal General Hospital in Canada in 1875. Osburn remained in Sydney for sixteen years and is acknowledged by historians as the founder of trained Nightingale nursing in Australia. In contrast to Osburn, Machin left the Montreal General Hospital in 1878, not quite

three years after her arrival, taking her nurses with her. The hospital then reverted to the old untrained nursing system and it was not until 1890 that a training school was introduced. The verdict of Canadian historians is unanimous that Machin was unsuccessful in her attempt to establish reformed, trained nursing along Nightingale lines in Canada. Nightingale's opinion of the relative successes of Machin and Osburn, however, was contrary to historians' later judgements. Nightingale and her circle in London condemned Osburn's work in Australia as a failure and broke off all contact with her. In contrast, Nightingale continued to reassure Machin that her Canadian experience might not have been the failure it seemed. Nightingale continued her warm friend and mentor and in 1879 was to find her a position as Lady Superintendent of St. Bartholomew's Hospital, the oldest and richest of the prestigious London teaching hospitals. This paper addresses the key question of why Nightingale's assessment of Osburn's and Machin's attempts to found Nightingale nursing in the colonies was so different from that of historians. In answering this question we particularly examine the importance of the Victorian concept of ideal womanhood, the gendered ideology of nursing, and the way the personal lifestyles of these two Lady Superintendents affected their professional careers. This paper draws on extensive research in the Nightingale papers at the London Metropolitan Archives and the British Library, the Machin Papers in the University of Toronto archives, the Sydney Hospital archives and the Montreal General Hospital archives. [Department of Clinical Nursing, University of Sidney (M02) NSW 2006, Australia. Margareth Allemang Centre for the History of Nursing, 34 Chestnut Park, Toronto (Ontario) M4W 1W6, Canada.]

Barry ADAMS, "Aids Optimism," *Semiotic Snares, and the Biomedical Subject* — Abstract non available. *Résumé non disponible*. [University of Windsor]

Note: Joint session with the Canadian Society for the History of Medicine, Canadian Gay and Lesbian Studies Association, Canadian Society for Nursing History. Note: *séance conjointe avec la Société canadienne d'histoire de la médecine, la Société canadienne d'études gay et lesbienne, la Société pour l'histoire des infirmières*

Lunch / *Dîner* (12:00-14:00)

Quantifying the Sea and Forecasting Fish / *Quantifier l'océan et prévoir les poissons*

(14:00-15:00 Victoria College 323)

Marianne FEDUNKIW (Oxford University)
chairman / *président*

Eric L. MILLS, *Forgotten Calculation: Making the Physics of the Sea Quantitative, 1876-1900* — During the 1870s there was no appreciation of how a great debate over ocean circulation—whether it was due to the wind or to thermohaline processes—could be resolved. There was general agreement that measurement was required—but how could measurement alone, in the absence of some new world picture, do more than increase confusion and debate? The answers came first in Scandinavia, first Sweden, then Norway, due to their concern with climatic change, fishery failures, and the needs of industry. Swedish work led the way, with an increasing database of information about oceanic circulation, and in Norway the meteorologist Henrik Mohn recognized about 1880 that the mathematical models he had been developing for cyclonic circulation of the atmosphere could be applied to data from the sea. His monograph *The North Ocean of 1887* provided a dynamic approach to circulation for the first time. This remarkable innovation fell upon unprepared ground, although it was similar to the dynamic approach developed successfully by Bjerknes, Helland-Hansen and Sandstroem only a decade and half later. Mohn's work has been described as “an abandoned mine: the veins have been exploited and their products have been incorporated into daily use, perhaps worn and recast, so that their presence is no longer recognizable,” although he had solved the problems that lay beyond the powers of earlier students of ocean circulation. [Department of Oceanography, Dalhousie University, Halifax NS, B3H 4J1, Canada. Email: E. Mills@Dal.Ca]

Jennifer HUBBARD, *Forecasting Fish* — The collapse of the Northern Cod and Northwest Atlantic groundfish stocks in the late 1980s and 1990s marked the destruction of the world's historically most productive fishery. This disaster, ironically, marked roughly the one hundredth anniversary of fisheries biology, a discipline organized around the premise that science could be used to monitor, improve, and increase the fisheries. It also occurred about twenty years after the international community agreed to extend the national territorial waters out to a 200 mile limit: a move partially predicated on the need to better manage ocean resources. This, naturally, has created skepticism about the value of this science, and particularly the Canadian practice of fisheries biology. The roots of the modern crisis indeed lie within the historical contingencies which shaped the science. Fisheries biology was a new field in the process of developing its own methodologies and sub-disciplines in the first decades of the twentieth century. Scientists who entered this new discipline arrived from a variety of backgrounds and with different presuppositions. This paper will discuss the very different research traditions that developed on Canada's Atlantic and Pacific coasts, shaped by such contingencies as the different periods in which fisheries biology became established on the two coasts, the contrasting nature of the coastal environments and fisheries, and the individuals involved. These various factors, impinging on an imprecise science that seeks to quantify and predict a largely invisible resource, has inevitably led to the mixed successes (or failures) which have characterized Canadian fisheries science in the recent period. [11 Bledlow Manor Dr., Scarborough (Ontario) M1E 1B1, Canada. Email: jennifer.hubbard@sympatico.ca]

Logical Empiricism Revisited / *L'empirisme logique reconsidéré*

(14:00-15:00 Victoria College 206)

Eric PALMER (Allegheny College)
chairman / *président*

Jonathan Y. TSOU, *The Justification of Concepts in Carnap's Aufbau* — This paper concerns the recent debate on the nature and motivations of the epistemological project advanced in Rudolf Carnap's (1891-1970) *Aufbau*. Much of this debate has been initiated by Michael Friedman (1999) and Alan Richardson (1998) who argue, against the received

view of the *Aufbau* as a foundationalist defense of empiricism, that Carnap's epistemological project is located in the tradition of neo-Kantian epistemology. On this revisionist reading of the *Aufbau*, Carnap's project is not motivated to address traditionally empiricist questions regarding the justification of knowledge, but rather to show how objective

knowledge is possible. A central aspect of the *Aufbau* that is neglected in the revisionists' analysis is the role of epistemic justification in Carnap's project. The aim of the present study is to argue that although the nature of the epistemology in the *Aufbau* is neo-Kantian, Carnap's method of construction theory (or rational reconstruction) is formulated precisely as a method for the justification of conceptual knowledge. Carnap's project radically redefines "justification" into a formal-conventional notion, and is part of Carnap's more general agenda of redefining epistemology as a purely formal discipline. [The Committee on Conceptual and Historical Studies of Science, University of Chicago, 1126 East 59th Street, Room 205, Chicago, IL, USA, 6063. Email: jtsou@midway.uchicago.edu]

Alan RICHARDSON, *Kuhn's Structure and Logical Empiricism: Beyond Epic, Tragedy, Irony, and Farce* — This talk seeks to sort out historical and interpretative issues regarding the place of Kuhn's *Structure of Scientific Revolutions* in the history of philosophy of science in

North America. After reviewing certain narrative structures already extant in the field (epic, tragedy, irony, and farce), I argue that there is an important dimension of the history that everyone has neglected by assuming a monolithic and professional image of logical empiricism: Kuhn's account of logical empiricism derived from its public image (he says so in the 1995 interview), and this public image is very different from the self-image of the program as a project in technical philosophy. Some lessons for our understanding of the place of logical empiricism in the history of philosophy of science and of related disciplines, especially history of science, derived from attending to a divergence between the "internal" professional and technical image of logical empiricism and its "external" public image will be outlined. These will be placed within a more general framework about a peculiar agony of technical "scientific philosophy": those outside its professional bounds have a hard time seeing it as philosophy precisely because it is technical. [Department of Philosophy, University of British Columbia, 1866 Main Mall E370, Vancouver BC V6T 1Z1 Canada. Email : alanr@interchange.ubc.ca]

Break / Pause (323: 15:00-15:15 ; 206: 15:15-15:30)

Philosophy Meets Sociology / *La philosophie rencontre la sociologie*

(15:30-17:00 Victoria College 206)

Bart SIMON (Concordia University)
chairman / *président*

K. Brad Wray, *Scientific Specialisation* — As Kuhn notes, "over time a diagram of the evolution of scientific fields [and] specialties ... comes to look strikingly like a layman's diagram for a biological evolutionary tree" (1991, 7-8). It is agreed that the creation of a new scientific specialization is a complex process, involving both social and epistemic or cognitive changes. On the social side, scientific specializations are characterized by groups of people concerned with the same problems. They conference together, publish in the same journals, compete for funding, and often collaborate with one another. But, scientific specializations are also thought to reflect differences that are epistemically significant, for example methodological differences. A thorough account of scientific change should include an analysis of the processes by which new scientific specializations are created. A key to developing an adequate account is to explain how the epistemic and social dimensions relate. My aim in this paper is to advance our understanding of scientific specialization. I examine three competing accounts of the processes that lead to the creation of new specializations, and distinguish between accounts which emphasize the social dimension of the change and those that emphasize the epistemic dimension. I argue that a modified version of Kuhn's account, which suggests that lexical changes in science are the principal cause of the creation of new specializations, is the account best supported by the available evidence. I also suggest that this dimension of Kuhn's account of scientific change provides insight into understanding his resistance to

the charge of irrationalism. [Department of Philosophy, University of Alberta, 4-115, Humanities Centre, Edmonton (Alberta) T6G 2E5 Canada. Email: kbwray@ualberta.ca]

Jay FOSTER, *Trust Within Reason* — The ways in which testimony can be used to justify epistemological claims have been the subject of at least two recent books: Steven Shapin's *A Social History of Truth* and Anthony Coady's *Testimony*. Shapin and Coady observe that most statements are accepted as true not on the grounds that they have empirical or logical justification but on the basis of the testimony of others. Moreover, we accept or reject testimony, not for empirical or logical reasons, but for normative reasons, like expertise, trustworthiness and credibility. This paper criticises accounts that use the idea of testimony to justify epistemological claims by arguing that these accounts fail to separate acts of testimony from acts of judgement. In broad terms, testimony consists of statements about states of affairs given by an individual to other individuals. In contrast, judgement is the formation of a belief or conclusion by an individual, typically in the form of an affirmation or denial, and, ideally, it follows deliberation. The paper argues that there is no logical or necessary linkage between testimony and judgement. Testimony from the most expert, trustworthy and credible source does not require favourable judgement, and vice versa. Once testimony and judgement are disentangled, it becomes difficult to reduce epistemological claims to issues about the credibility or trustworthiness of testimony. [IHPST, University

of Toronto, 466 Brunswick Ave., Toronto (Ontario) M5R 2Z5, Canada. Email : jfoster@mindraft.com]

Bryan BODDY, *Icons and Iconoclasts* — Harry Collins’s *Changing Order: Replication and Induction in Scientific Practice* (1985) opens with the muddy dictum: “Treat descriptive language as though it was about imaginary objects.” This prescription invokes the conventional distinction between the external world of objects and the internal world of ideas, typically offered by philosophers of science and empirical philosophers. However, Collins’s account of the distinction is strikingly iconoclastic. He requires the reader to abandon the idea that the descriptive language of science is about things in the world and, instead, accept the idea that scientific descriptions are icons which denote attributes,

functions and relationships. Taking *Changing Order* as a departure point, this paper will trace conceptual linkages and disjunctures between the account of scientific representation offered by sociologists of science and accounts of artistic and literary representation offered by Erwin Panofsky and W.J.T. Mitchell. Panofsky’s *Studies in Iconology* (1939) and Mitchell’s *Iconography: Image, Text, Ideology* (1986) offer organised approaches to understanding systems of representation that resemble the “actor-networks” offered by Bruno Latour. The paper argues that the so-called “social turn” in science studies is less about establishing the normative basis for epistemological claims and more about a entrenching a particular strain of philosophical iconoclasm. [IHPST, University of Toronto, Apt. 3, 103 Gloucester St., Toronto, (Ontario) M4Y 1M2, Canada. Email : bryan_boddy@yahoo.com]

Proof, Prediction and Mathematics in Ancient and Islamic Science /
*Preuve, prédiction et mathématiques dans les sciences
antiques et islamiques*
(15:15-17:00 Victoria College 323)

Craig FRASER (University of Toronto)
chairman / *président*

Alan C. BOWEN, *The Exact Sciences in Fourth-Century Greece and their Interpretation in Aristotle’s Posterior Analytics 1.13* — Usually one should segregate questions about the meaning of what ancient philosophers say regarding the exact sciences from historical questions about the truth of their claims. But this rule fails when the philosophical remarks are insufficient to allow a sure grasp of what was meant. Here, we must turn to what is known of the contemporary sciences to delimit possible interpretations. This is the case with Aristotle’s comments about mathematics and the sciences that use mathematics in *Posterior Analytics* 1.13. To understand him, we must determine what the exact sciences were like in his time, and then proceed tentatively on the charitable assumption that what he says is true. Accordingly, I will first argue that there are but three surviving sources of the exact sciences in Aristotle’s time: Aristoxenus’ *Elements of Harmonia*, his *Elements of Rhythm*, and Hipparchus’ quotations of Eudoxus’ *Phaenomena*. This will entail demonstrating that the common practice of including Euclid’s works among the scientific documents of the fourth century is based on a faulty appreciation of the evidence for his dates and of the signs in his treatises of later developments. Next, in light of these sources, I will propose that, when Aristotle says, for example, that optics is subordinate to geometry because it concerns the fact that something is the case and geometry gives the reason why, he means not that optics is applied geometry, but that optics often (but not always) draws on geometry to make its inferences, whereas geometry never draws on optics. [IRCPS, 3 Nelson Ridge Road, Princeton NJ 08540-7423, USA. Email: acbowen@princeton.edu]

Daryn LEHOUX, *Predictions in Ancient Astronomy and Astrology* — In looking at the mechanisms of astrological prediction, we see that by relying on authoritative texts and instruments, the ancient astrologer was able to forecast the fates of individuals, as well as events such as meteorological phenomena and crop yields. This tradition finds its origins in several different omen traditions, common throughout the ancient Mediterranean and Near East, where different kinds of fortuitous events (including astronomical events such as eclipses) frequently had ominous significance. By the fifth century B.C., however, astronomy distinguished itself from the other omen traditions by developing mathematical methods for predicting the events (e.g., eclipses) from which its omens were derived. But the very adoption of these predictive methods served to canonize the timing and character of the astronomical events. That is: instead of being, strictly speaking, predictive, the texts and tools of early mathematical astronomy were normative. This meant that in making the astronomical part of their predictions, the astronomer, in spite of his rhetoric to the contrary, is primarily working from texts or instruments, rather than from observations in the natural world. [History of Science and Technology Programme, University of King’s College, Halifax (Nova Scotia) B3H 2A1, Canada. Email : daryn.lehoux@ukings.ns.ca]

Alexander JONES, *Frames of Reference in Ancient Astronomy* — Modern astronomy follows Ptolemy (mid second century A.D.) in taking the Spring Equinoctial Point (one of the intersections of the ecliptic—the plane of the sun’s orbit—with the plane of the earth’s equator) as the zero point for measuring planetary positions along the ecliptic. In

Ptolemy's astronomy this frame of reference is clearly distinguished from the more "natural" frame of reference of the stars, which Ptolemy considers unsuitable on theoretical grounds. We know that in Babylonian astronomy the two frames of reference (respectively called "tropical" and "sidereal") are not treated as distinct. An examination of computed positions of the moon and planets in Babylonian and Greek astronomical texts under certain conditions makes it possible to find out which was actually being used. One might have expected a fairly neat transition from purely sidereal, observation-based positions in Babylonian texts to purely tropical, computation-based positions after Ptolemy; the reality turns out to have been considerably more complicated. [University of Toronto. Email: alexander.jones@utoronto.ca]

Note: Joint session with the Canadian Society for the History and Philosophy of Mathematics.

Glen Van BRUMMELEN, *Analysis, Synthesis, and Computation: From Pure Geometry to Applied Mathematics in Medieval Islam* — The paired techniques of analysis and synthesis, born in ancient Greece, provided both a direct means to find a path to the solution of a geometrical problem and a way to convert that path into a rigorous proof. In medieval Islam analysis was used in this way, but it was also put to use in the solution of applied problems. We shall concentrate on texts by 10th century geometer Abu Sahl al-Kuhi and 12th-century mathematician Al-Samaw'al, to witness the transformation of geometric analysis to solve the problems of the distance to the shooting stars and the dip angle to the horizon for an elevated observer. [Bennington College, Bennington (Vermont) 05201, USA. Email: gvanbrum@bennington.edu]

Note : séance conjointe avec la Société canadienne d'histoire et de philosophie des mathématiques.

CSHPS Executive Meeting /
Réunion du conseil d'administration de la SCHPS
(17:00)

Monday, 27 May 2002 / *Lundi, 27 mai 2002*

CSHPS Breakfast Council Meeting /
Déjeuner des conseillers de la SCHPS
(8:00-9:00 Victoria College 323)

Diverse Allegiances and Worldviews in Victorian Evolutionism /
Allégances diverses et visions du monde dans l'évolutionnisme victorien
(9:00-10:15 Victoria College 323)

Martin FICHMAN (York University)
chairman / *président*

Donald R. FORSDYKE, *Darwin's Research Associate, George John Romanes* — Darwin was well aware that there were major inconsistencies in his 1859 theory of the origin of species by natural selection and he spent the rest of his life attempting to resolve them. For eight years prior to his death in 1882 it is likely that, apart from his immediate family, he spent more time discussing these inconsistencies with his young Canadian research associate, George John Romanes, than anyone else. In 1886 Romanes presented a theory of the origin of species by "physiological selection" to the Linnean Society. He claimed to have resolved the inconsistencies, but invoked abstract elements (e.g. "a peculiarity of the reproductive system"), which were incomprehensible to his Victorian contemporaries. He was strongly attacked by the elder statesmen of science—Wallace and Huxley—and died of a brain tumour at age 46 in 1894. Results of recent research on various genome projects have led to a new reinterpretation of Darwin's theory that has much in common with that of Romanes (see *The Origin of Species, Revisited. A*

Victorian who Anticipated Modern Developments in Darwin's Theory. McGill-Queen's University Press, 2001). [Department of Biochemistry, Queen's University, Kingston (Ontario) K7L3N6, Canada. Email: forsydke@post.queensu.ca]

Erin MCLAUGHLIN-JENKINS, *Darwin as Revolutionary: T. A. Jackson on Vulgar Darwinism, History, Nature, and Society* — In 1936, Thomas Alfred Jackson, a working-class intellectual and socialist lecturer, wrote that "Darwin's work demonstrated—though to this day few Darwinian's realize it—that Nature proceeded not in a mechanical way but in a revolutionary (i.e. a dialectical) way." This statement, from *Dialectics: The Logic of Marxism, and Its Critics, An Essay in Exploration*, formed part of Jackson's caustic and witty indictment of the vulgar Marxism and Lamarckian evolutionism dominating the British Labour Party. The British Left had long relied on evolutionism as a prop for socialism, but the ambiguities in biological thought—created by the ongoing muddle of Darwinism and Lamarckism—had

commingled with parallel confusion in socialist theory and practice. Claiming Darwin for the side of revolution signaled Jackson's intention to rescue Darwin's theories from vulgar Darwinists such as Herbert Spencer. In an astounding analysis of the theoretical problems of Marxism and Darwinism, Jackson strove to distinguish the originals from the fakes, and to once again affirm the natural congruence between dialectical materialism and Darwin's version of evolution. This paper explores Jackson's attempt to establish a unified Marxist theory of evolution, history, and society. [York University, Toronto (Ontario), Canada. Email: erink@yorku.ca]

Bernard LIGHTMAN, *Huxley and Scientific Agnosticism: The Strange History of a Failed Rhetorical Strategy* — T. H. Huxley's invention of the term "agnostic" in 1869 is often seen as a brilliant rhetorical strategy. Portrayed as an effective weapon in Huxley's public debates with defenders

of the Anglican establishment, the creation of scientific agnosticism has been interpreted as a turning point in the relationship between science and religion. In this paper I will challenge this interpretation of the rise of scientific agnosticism. Huxley was reluctant to identify himself unambiguously as an agnostic in public until 1883 and his restricted use of agnostic concepts during the seventies and eighties was compromised when other unbelievers, with different agendas, sought to capitalize on the polemical advantages of referring to themselves as agnostics. As a result, he was not always associated with agnosticism in the public mind and his original conception of it was modified by others to the point where he felt compelled to intervene in 1889 to set the record straight. But Huxley could not control the public meaning of "agnosticism," and its value to him as a rhetorical strategy was severely limited. [Division of Humanities, Bethune College, York University, Toronto (Ontario) Canada]

Explanation and Understanding / *Expliquer et comprendre*

(9:00-10:15 Victoria College 206)

Robert HUDSON (University of Saskatchewan)
chairman / *président*

Diana PALMIERI, *What is a Complete and Independent Explanation?* — Jaegwon Kim is right to stress that a good explanation ought to increase our understanding of its explanandum; indeed it is the goal of all virtuous scientific explanation to achieve just this. Kim goes on to argue that the coexistence of too many explanations for the same phenomenon hinders understanding. In an attempt to delimit the number of coexisting explanations, explanatory independence and completeness are introduced as individually necessary and jointly sufficient conditions for ideal explanation. With the help of his famous Explanatory Exclusion Principle, Kim concludes that we ought to be pursuing independent and complete explanations in scientific inquiry. However, when we set out to clarify the conditions in which explanations qualify as independent or complete, it becomes clear that neither of these can be explanatorily virtuous properties with respect to any species of scientific explanation. The reason: his account disqualifies obviously virtuous explanations. This paper shows that the independence and completeness conditions are each, on their own, sufficient for epistemically vicious explanation; it follows that the two are not jointly sufficient for mere adequate explanation. Rejection of Kim's stronger thesis regarding ideal explanation, in turn, follows from this. [University of Western Ontario, Department of Philosophy, Talbot College, London (Ontario), Canada. Email : dpalmier@uwo.ca]

Natasha MCCARTHY, *Incommensurability : Science and Common Sense* — Kripke and Putnam's views on naming and meaning, espoused in the early 1970's, probably still represent the majority view of natural kind terms. This holds that the meaning of terms such as "gold," "water," "tiger" and, possibly, "person" are fixed by a relation of physical sameness discoverable by science. Holding this view of natural kinds entails that certain terms in ordinary language are dependent upon science for adjudication regarding their correct application. I wish to argue that the fundamental mistake in this view is its assumption that it is possible to forge the necessary connections between ordinary language terms and concepts, and the terms of art used in scientific research. It is my claim that scientific and common sense knowledge are incommensurable, because the two "systems" employ concepts that cannot be intertranslated. In the paper I aim to explicate the notion of incommensurability that I am employing, drawing out the epistemological and ontological aspects of claims of incommensurability. I then intend to show just how the sciences differ conceptually to the knowledge of the layman, in the domains of physics, chemistry, biology, and even the human and natural sciences. I take this result to show the untenability of a notion of natural kinds based on the intuitions employed by Kripke and Putnam. I will claim that a good account of natural kinds must either proffer a different understanding of kind terms for the sciences and for ordinary language, or acknowledge that ordinary language contains no genuine natural kind terms. [Department of Philosophy, University of Toronto, 215 Huron Street, Toronto (Ontario) M5S 1A1 , Canada. Email : natasha.mccarthy@utoronto.ca]

Break / *Pause* (10:15-10:30)

Science, State and Society / *La science, l'État et la société*

(10:30-12:00 Victoria College 323)

Ernst HAMM (York University)
chairman / *président*

Martin S. STAUM, *French Social Scientists on the Stable Society, Race and Empire, 1815-1848* — The early nineteenth-century social sciences constructed images of the “Other” within France and abroad that reflected social fears of democracy and individualism as well as disdain for non-Europeans. They provided examples of power/knowledge in Foucault’s sense and Said’s “Orientalist” model of denying authenticity to others. Historians have long understood the idea that social sciences could be tools for avoiding social turmoil. However, recent analyses of learned societies before 1848 in phrenology, geography, and ethnology have presented a different image. They have portrayed phrenologists as anti-clerical social progressives, geographers as erudite notables, and ethnologists as anti-slavery enthusiasts. Challenging this recent image required use of the archives of the Société de géographie (Cartes et Plans, BNF) and the Foreign Affairs Archives, the manuscripts of the secretary of the Société ethnologique de Paris, as well as the published journals and minutes of the societies and the works of their individual members. The phrenologists were ambivalent enough about social regeneration to wish for experts to prescribe social roles and limit social mobility. They also condemned many non-European peoples to irremediable organic deficiency. The geographers attempted to provoke their reticent government into more ambitious colonial ventures. They employed the facial angle and phrenology to help supplement theories of racial hierarchy. The naturalists and former Saint-Simonians among the ethnologists, who anticipated the discourse of later anthropologists, were either hierarchical monogenists or polygenists. These arguments provided a condition for the thinkability of empire. If peoples were civilizable, European tutelage to guide them to civilization was indispensable. If peoples were uncivilizable, Europeans could dominate them and exploit their resources in good conscience. [Department of History, University of Calgary, Calgary (Alberta) T2N 1N4, Canada. Email: mstaum@ucalgary.ca]

James ELWICK, *Mementos of Individuality: Inscriptions, Surveillance and Emancipation in Victorian England* — This paper discusses a doublet of Victorian England, in which the documentation of individuals allowed not only their statistical investigation as a member of a group, but also their emergence as a discrete and demarcated legal individual. Investigations of statistics by Theodore Porter and Ian Hacking tend to investigate the first theme; this paper opts for the second. Two historical examples are given: one is the rise of civil registration, begun in 1836, and its gradual linkage with standardized nosologies and death certificates. Another is the

use of ‘tickets of leave’ for imprisoned men, allowing them to enjoy shortened sentences for good behaviour by allowing bureaucracies to note this good behaviour. Two analytical techniques are used. The first is a focus on how documents can stabilize and fix an individual’s identity. The tool used here is that of inscriptions to understand how experimental findings can become stabilized. It suggests that an individual’s identity can become fixed increasingly through these inscriptions, not simply through memory. A technique from science studies, then, can be used to understand a larger chapter in Victorian history. The second technique is to move away from hasty Foucauldian notions of “surveillance” and the “gaze of the state” that may have been ushered in by this documentation. It did allow the observation of people by outside agencies, by decontextualizing parts of their life into data (particularly through its standardization). But it also brought new, emancipatory possibilities into being, caused by this same decontextualization and standardization. Women and children, for one, gradually became full-fledged citizens of the Victorian state demanding procedurally-equal treatment by its agencies; by the same token, criminals could be treated as different individuals meriting different treatment by bureaucracies. In this sense the paper opts not for Foucault, but for Weber. [IHPST, Victoria College, University of Toronto, Toronto (Ontario) Canada. Email: jelwick@chass.utoronto.ca]

Judith Szapor, *A Matter of Life or Death: Soviet and Foreign Physicists at the Ukrainian Physical-Technical Institute in the 1930s* — The Ukrainian Physico-Technical Institute in Kharkov was founded in 1928, designed to become one of the largest and best-equipped centres for theoretical and applied physics in the world. At a time of scarce resources, the Soviet government spared no expense to build state-of-the-art facilities and attracting an outstanding scientific staff. In marked contrast with previous Soviet practice, foreign experts were hired and young Soviet scientists were sent to train at the most prestigious laboratories of Europe, from Cambridge to Berlin and Copenhagen. By 1932, the Kharkov Institute had become the leading Soviet centre in nuclear, plasma and low temperature physics and an integral part of the international network of physics. (The Institute’s later significant role in the Soviet nuclear programme is outside of the scope of this paper.) This scientific idyll, however, lasted for only a few years, before Stalin’s Great Purge reached the seemingly isolated world of scientific laboratories. By the end of 1937, most of the foreign scientists working in Kharkov had been either expelled from the Soviet Union or, along with many of their Soviet colleagues, arrested on trumped-up political charges. Based on published

memoirs, unpublished interviews, and recently opened archival material, I will trace the fate of the Soviet and foreign physicists associated with the Kharkov Institute in the 1930s, and assess the impact of Stalin's terror on the course

of physics in the Soviet Union. [Ontario Institute for Studies in Education, University of Toronto, 16 Welwood Ave Toronto (Ontario) M6C 1G9, Canada. Email: jszapor@yorku.ca]

20th Century Philosophy of Science / *La philosophie des sciences au XX^e siècle* (10:30-12:00 Victoria College 206)

Alan RICHARDSON (University of British Columbia)
chairman / *président*

Alain BEAULIEU, *La philosophie des processus de Whitehead et la doctrine de l'évolution* — On distingue habituellement trois phases dans le développement de la pensée de Whitehead. Une première où il s'intéresse aux mathématiques (*Principia Mathematica*, 1912, en collaboration avec Bertrand Russell), une seconde où il s'occupe de physique (*The Concept of Nature*, 1920), et une troisième où Whitehead expose sa cosmologie (*Process and Reality*, 1927). La métaphysique de la dernière période culmine dans un hommage à la puissance de Dieu. S'agit-il alors pour Whitehead de renouer avec de vieilles thèses créationnistes? L'absence de toute référence à la doctrine évolutionniste, dans *Process and Reality*, pourrait le laisser croire. Dans ses entretiens, Whitehead en vient d'ailleurs à qualifier Darwin de personnage « immensément grand, mais aussi du plus ennuyeux des grands hommes » (*truly great, but he is the dullest man I can think of*). Ce néant de sympathie pour l'évolutionnisme s'accorde également avec une affirmation tirée de *Science and the Modern World* (1925) où Whitehead oppose à la doctrine évolutionniste un principe de changement dépourvu de toute idée de progrès (*There is nothing to evolve [...] There can merely be change, purposeless and unprogressive*). Comment alors articuler la théodicée whiteheadienne à la possibilité d'un devenir non progressif? Le Dieu de Whitehead n'est pas créateur et transcendant. Il s'identifie plutôt à un potentiel immanent présent en chaque parcelle de la matière vivante et auto-organisée (panthéisme/panpsychisme). La philosophie organique de Whitehead suppose une nature pleinement dynamique, composée d'événements et de processus sans plan prédéfini, et dépourvue de toute norme stabilisatrice. Par contraste avec la pensée de Leibniz, tous les mondes possibles deviennent compossibles; l'ordre et le désordre sont des éléments indispensables dans l'univers. D'où l'aspect chaotique souvent attribué à la conception whiteheadienne de la vie organique. Ainsi, Whitehead résiste à la thèse darwinienne d'un devenir par lente

accumulation de modifications insensibles (le gradualisme), mais il se rapproche aussi des thèses néo-darwiniennes (ou théorie synthétique de l'évolution) en admettant l'existence d'un devenir non continu (*There is a becoming of continuity, but no continuity of becoming*) où les transformations par sauts brusques sont autorisées (le saltationnisme). Whitehead participe ainsi à la définition d'un nouveau paradigme scientifique dominé par l'absence de formes de transition. Nous tenterons de clarifier les rapports de Whitehead aux thèses néo-darwiniennes. [Université de Paris, VIII. 33, Côte Ste-Catherine, app. #911, Montréal (Québec) H2V 2A1, Canada. Email : beaulieual@hotmail.com]

Dan MCARTHUR, *Why Bachelard is not a Scientific Realist?* — Analytic philosophers have largely neglected Gaston Bachelard's philosophy of science. This oversight is unfortunate since Bachelard's philosophy shares some revealing similarities with much analytic work of later decades. Nevertheless, despite years of relative neglect, some commentators in the analytic tradition, like Garry Gutting and Mary Tjiattas, have seen the possibility of constructing novel defences for scientific realism in Bachelard's work. These thinkers have found in Bachelard's work an account of experiments that shares similarities with the experimental realism of Hacking while at the same time giving due account to the internal-realism of Putnam and the historical views of Kuhn. This paper shows that this reading of Bachelard's views on experimental practice misunderstands some important features of his philosophy of science. It also demonstrates that no defence for scientific realism can be derived from Bachelard's philosophy of science. This paper, then, provides an historical clarification of the nature of Bachelard's position. However, it also has a bearing on the debate over scientific realism because if my conclusions are sound, then one approach to defending scientific realism is ruled out. [Philosophy Department, University of Regina, Alberta, Canada. Email : daniel.mcarthur@sympatico.ca]

Lunch / *Dîner*

CSHPS Annual General Meeting / *Assemblée générale annuelle de la SCHPS*
(Victoria College 323)

Note: Lunch will be provided. / *Note: le repas sera gracieusement offert.*

Philosophy and History of Engineering / *Philosophie et histoire du génie*

(14:00-15:15 Victoria College 206)

Janis LANGINS (University of Toronto)
chairman / *président*

Steven L. GOLDMAN, *Contingency, Will and the Production of Technical Knowledge: Towards a Philosophy of Engineering* — Engineers produce and apply technical knowledge that incorporates as boundary conditions explicitly contingent value judgments underlying the selective application of that knowledge by others. Engineering thus employs a very different form of reasoning than science does, or than has dominated Western philosophy for over 2300 years. From Plato on, the Western intellectual tradition has consistently subordinated practice, will and contingency to theory, reason and necessity; subordinated the concrete and the individual to the abstract and the universal; experience and prudence to reality and wisdom; probability and application to certainty and contemplation; open-ended, pluralistic inquiry to closed, unique explanations; the historical and culturally contextual to the eternal and culturally utopian. Engineering is associated with the subordinate members of these contrasting concepts, science with the superordinate. Favoring science as emblematic of knowledge and rationality, makes practical decision-making in the face of uncertainty, contingency, and values appear non-rational, a concern already found in Isocrates, Aristotle and Cicero. Studying engineering reasoning, however, could help redefine “rationality” in ways valuable to intelligent contingency-based action generally, and to intelligent technological action in particular. This paper describes the distinctiveness of engineering as a mode of reasoning and identifies the engineering-derived intellectual tools capable of supporting rational action incorporating willfulness, contingency and uncertainty. [Department of Philosophy, Lehigh University, 15 University Drive, Bethlehem, Pennsylvania 18015, USA. Email : slg2@lehigh.edu]

Natasha ZWARICH, *Enseignement supérieur et montée des experts: la formation des ingénieurs sanitaires à Montréal, 1880-1930* — Au XIX^e siècle, dans la foulée de l’industrialisation et de l’urbanisation, les grandes villes canadiennes mettent en place des infrastructures permanentes. Pour ce faire, les dirigeants municipaux font appel à des experts de plusieurs domaines, dont les ingénieurs, qui s’efforcent de solutionner les problèmes posés par l’urbanisation (égouts, aqueduc, voirie). Plus tard, les médecins deviennent des acteurs importants dans l’élaboration de politiques en matière de santé publique. Dans les années 1880, les ingénieurs sanitaires dont les connaissances allient, à la fois le savoir médical et les connaissances de l’ingénieur, font leur

apparition dans les officines de l’administration publique. Or, si les ingénieurs et les médecins ont suscité de l’intérêt chez les historiens, les ingénieurs sanitaires demeurent largement méconnus. Nos travaux en cours proposent, justement, de jeter un éclairage sur cet épisode de l’histoire des sciences et des techniques au Canada. Ainsi, nous avons tenté de mieux saisir l’implication des établissements d’enseignement supérieur dans la formation des ingénieurs sanitaires. Nous avons dégagé les savoirs et les techniques propres à cette catégorie d’ingénieurs. En effet, outre les cours de chimie et de physique, les étudiants en génie sanitaire devaient acquérir des notions générales de biologie et de bactériologie. Ces matières leurs permettaient de résoudre, entre autres, les problèmes de purification et d’épuration des eaux. Les ingénieurs diplômés des établissements d’enseignement supérieur étaient majoritairement employés par l’administration municipale et le Conseil d’hygiène de la province de Québec. Nous nous sommes appuyés sur les archives de l’Université McGill, de l’École Polytechnique de Montréal et de l’Université de Montréal. Notre communication est une contribution à l’histoire de la rencontre entre la santé publique et de l’urbanisation. [CIRST, Université du Québec à Montréal, CP 8888, Succ. Centre-Ville, Montréal (Québec) H3C 3P8, Canada. Courriel : natashazwarich@hotmail.com]

James E. TOMAYKO, *The CF-105 Arrow and the Price of National Self-Esteem* — During the 1950s, the Canadian Avro Arrow was the sort of project that can be considered an excellent example of “big science.” Many think that it was “Canada’s Apollo Program,” a designation easy to believe when considering that two dozen of its engineers joined the American Apollo Program after the Arrow was cancelled. This paper examines the interplay between such types of projects and national self-esteem. In some cases, completing expensive programs with marginal actual needs is more important to the national psyche than the fulfillment of the program for the original purpose. The Arrow program seems such a project. This paper examines the meaning of the program for Canadian technological and scientific achievement in comparison with others that are contemporaries or successors, some strange and portentous aspects of its cancellation, resulting in an almost unnatural international obscurity, the impact of its loss, and the reasons for the almost cult following the program has acquired over the years since its cancellation. [Carnegie Mellon University, 147 Vander Road, Wexford, PA 15090, USA. Email: jet@cs.cmu.edu]

Author Meets Critics / *L'auteur confronte ses critiques*

(14:00-15:15 Victoria College 323)

Danny GOLDSTICK (University of Toronto)
chairman / *président*

Author / *Auteur*

James Robert BROWN (University of Toronto)
Who Rules in Science? An Opinionated Guide to the Wars (Harvard University Press, 2001)

Critics / *Critiques*

Ernst P. Hamm (York University)
Gordon R. McOuat (University of King's College)

Break / *Pause* (15:15-15:30)

Stillman Drake Lecture / *Lecture Stillman Drake*

(9:00-10:15 Victoria College 206)

Lesley CORMACK (University of Alberta)
CSHPS President / *Présidente de la SCHPS*

Ian HACKING, "*True, Values and the Sciences*" — The recent "science wars" generated a great many sophisticated criticisms of scientists' visions of what they do. But both sides of the debate were conducted at a pretty rarefied level. This lecture tries to analyze one "grass roots" worry about the sciences. There is a feeling that some profound kind of truth is missing from scientific results, that the sciences fail to answer to a deep human craving for truth and understanding. One root of the problem is that there are two Western traditions of truth. One is Greek, Aristotelian, truth is correspondence to how things are. The other is Hebrew, Biblical,

truth is steadfastness, being worthy of trust. In the disciplines named history and philosophy of science, discussion always focuses on the first tradition and ignores the second. The values that many people want are discarded. One example of the grass roots worry is used to illustrate this theme. The lecture concludes with another set of human values. They arise from the way in which the sciences emerge in history from a number of innate and specifically human faculties. And it is these faculties themselves that provide the criteria for truth in the sciences. [University of Toronto / Collège de France. Email : ian.hacking@college-de-france.fr]

CSHPS President's Reception /
Réception de la présidente de la SCHPS
(17:00-19:00)

CSHPS Banquet /
Banquet de la SCHPS
(19:00)

Opportunities for Science / *Les réseaux de la science*

(9:00-10:15 Victoria College 206)

Trevor LEVERE (University of Toronto)
chairman / *président*

Hannah GAY, *Science and Opportunity in London, 1871-1885: The Diary of Herbert McLeod* — Herbert McLeod FRS (1841-1923) was Professor of Experimental Science (practical physics and chemistry) at the Royal Indian Engineering College situated just outside London at Cooper's Hill. This paper is an attempt to recreate something of his scientific and social world during his first fifteen years as a professor. It draws on a number of sources but is heavily based on McLeod's diary which contains daily entries from 1860 to 1923. Many events, places, scientific fashions and practices are mentioned, and for the fifteen years under consideration in this paper, the diary has a large cast of characters, including about three hundred people engaged in scientific or technical work. It recounts how McLeod and his peers moved through a range of scientific spaces such as laboratories and learned societies, through intellectual and technical sites such as spectroscopy, telephones and electrical lighting, through religious sites such as churches and mission meetings, and through a range of conventional social sites such as clubs, visits to each other's homes, and holiday locations. The sites can thus be defined in geographical, intellectual, disciplinary, institutional, material cultural or social terms. Movement through them led to the formation of many different personal, ideological and material associations and, as will be discussed, gave people access to a wide range of scientific and technical activity. [Imperial College of Sci-

ence, Technology and Medicine, Exhibition Road, London, SW7 2AZ, Great Britain. Email: h.gay@ic.ac.uk. History Department, Simon Fraser University, 8888 University Drive, Burnaby, BC V5A 1S6, Canada. Email: hannah_gay@sfu.ca]

Sylvia T. WARGON, *The demography of Enid Charles (1894 - 1972) in historical perspective* — Enid Charles's work in demography and population statistics from the early 1930s bear the imprint of certain demographic, social and intellectual factors and trends of 19th, particularly late 19th and early 20th century Britain. They help to explain the focus/subjects, methods/techniques and key philosophical ideas/concepts featured in her published and unpublished work. Moreover, Charles readily absorbed other European influences, such as the thought and ideas of some of France's foremost demographers (Dumont, Landry), unknown—or more likely ignored?—in Britain before her time. Charles's demographic work in the early 1930s aroused considerable controversy and criticism and was henceforth ignored, even reviled. However, in the closing decade of the last century, a British social historian revealed the “prescience” and continuing relevance of some of her ideas; her practical work in population statistics in the 1940s and 1950s was similarly “avant-garde” in nature. [Statistics Canada, retired. Email: sylvia.wargon@statcan.ca]

Experiments / *L'expérimentation*

(9:00-10:15 Victoria College 323)

James BROWN (University of Toronto)
chairman / *président*

Margaret SCHABAS, *Hume on Thought Experiments* — Thought experiments became commonplace in the early modern period in the hands of Galileo, Harvey and Descartes, among others. It is still a matter of debate as to whether the understanding imparted by thought experiments stems from appeals to previously accumulated experience, or draws upon some non-empirical intuition. Thought experiments often commence with jarring counterfactuals and unfold without introducing any new empirical evidence. In principle, the demonstration of the thought experiment is so compelling that there is no need to repeat the experiment or seek greater approximation to the actual world, as one would in laboratory work. By exaggerating the dimensions of the relevant variables or creating a situation which is thoroughly fictitious, there is no confusion that it is the mind alone that is conduct-

ing the experiment. Moreover, the aim of the experiment is not that of measurement or identification, as is true of most laboratory experiments. Usually the aim is to establish a tendency, a pattern, and in that sense the experiment is more a demonstration than an open-ended inquiry. Most if not all cases of economic analysis involve abstraction, the distortion of reality either by idealization or by oversimplification, but these manipulations in themselves do not constitute thought experiments. Part of my efforts here will be to isolate those cases of genuine thought experimentation in economics during the early modern period. I will argue that David Hume (1711-76) was one of the first to conduct thought experiments in what we now call economics, notably on the subject of money and that these bear some resemblance to his thought experiments in his *Treatise of Human Nature*. In his essay

“Of the Balance of Trade,” for example, Hume supposes first that four-fifths of all money is annihilated overnight and then, in another experiment, that the quantity of money is increased fivefold overnight. In both cases, he spells out the consequences for prices given foreign trade and shows that the money would be restored to its original level (Hume 1985, 311). Here and in similar trains of thought, Hume employs unrealistic quantitative shifts so as to help the mind grasp that it is the relative proportion of money to prices, and not the absolute quantity of money, that matters. In other essays and letters, Hume entertains the complete and sudden annihilation either of gold or of paper money, arguing that the void would be immediately filled by silver in the first case, and foreign specie in the second (Hume 1985, 296, 320). Money thus abhors a vacuum as much as nature. The implication of this, for Hume, is to downplay the importance of money—it will flow on its own accord—and underscore the importance of people and industry. “A government has great reason to preserve with care its people and manufactures. Its money, it may safely trust to the course of human affairs, without fear or jealousy” (Hume 1985, 326). Hume’s facility with thought experiments speaks to a skill he may well have inculcated from the study of natural philosophy. [Department of Philosophy, University of British Columbia, 1866 Main Mall E370, Vancouver BC V6T 1Z, Canada. Email: schabas@interchange.ubc.ca]

Daniela MONALDI, *The Conversi-Pancini-Piccioni Experiment and the Beginning of Modern Particle Physics* — At the beginning of the 1940’s, the particles called mesotrons, or mesons, held the promise of a unified theory of nuclear phenomena. Mesotrons had been found to constitute the most penetrating component of cosmic rays. They were heavier than electrons and lighter than protons, just as the hypothetical quanta of a theory formulated by Yukawa. Moreover, the mesotrons decayed emitting electrons, and Yukawa’s nuclear quanta were expected to be responsible for b-radioactivity by disintegrating into electrons and undetectable neutrinos. Were not the mesotrons the key to the atomic nucleus, to its binding as well as to its radioactivity? From 1941 and through the difficult years of World War II, three young Italian physicists, Oreste Piccioni, Marcello Conversi, and Ettore Pancini, carried on a series of observations of mesotrons stopped in matter, which seemed at the beginning to support Yukawa’s predictions. They were able to achieve a high level of precision and stability by innovating upon instruments and techniques of the Italian cosmic ray tradition. At the end of 1946, they reported that the rates of absorption of mesotrons in light materials were in catastrophic disagreement with the theory. If the mesotrons were not the nuclear quanta, what were they? And what new kind of phenomenon was their b-like decay? The physics community would have to sort through a bewildering diversification before prospects of unification could reappear, on a more remote level. [University of Toronto, 133 Rumsey Rd, Toronto (Ontario) M4G 1P3, Canada. Email: daniela.monaldi@utoronto.ca]

James A. MARCUM, *Tempering Nature: Exploring General Features of Experimental Practices in the Natural Sciences* — During the first-half of the twentieth century, the notion of a scientific method common to the natural sciences carried considerable currency. By that century’s end, however, the notion was bankrupt. In this paper, I propose a notion of a general method for experimental practices in the natural sciences. To that end, I expound upon several key concepts of this method. The first is that of scientific worldviews, in which student-scientists are instructed through textbook traditions. Those worldviews are composed of values and metaphysical assumptions, to which scientists are committed in their experimental practices, and of theories, laws, and hypotheses. The next concept is that of experimentation, which includes the design and execution, as well as such characteristics as controllability and reproducibility, of experiments. The final concept is that of the natural world, which is taken to manifest itself in terms of experimental results that are interpreted as scientific facts. Scientists then accept these facts as a representation of the natural world. There are two sequences that characterize this notion of method: from scientific worldviews to knowledge of the natural world via experimentation and from scientific facts about the natural world back to scientific worldviews. I call this notion of a general method for experimental practices “tempering” nature, since scientists seek to understand nature by tempering or controlling it experimentally—especially by measuring or quantitating it. Scientific knowledge, then, is the product of a tempering understood as a combining of the natural world with a viewing of that world through experimental practices. [Department of Philosophy, Baylor University, PO Box 97273, Waco TX 76798, USA. Email: James_Marcum@baylor.edu]

Break / *Pause* (10:15-10:30)

Environmental Epistemologies / *Épistémologie de l'environnement* (10:15-11:45 Woodsworth College, 111 Kruger Hall)

Edward JONES-IMHOTEP, *Knowing the North: Meteorological Instruments and Malevolent Nature* — This paper explores how understandings of the Canadian northern environment and its effects on both humans and instruments in the late 1940s and early 1950s called into question the ability of scientific workers to capture the North graphically and epistemically after the Second World War. It employs an expanded notion of “environment”—one rooted in climate and ecology, but also in magnetic storms, atmospheric disturbances, and solar prominences—to illuminate the role of machines in mediating the tension between unreliable witnesses, a malevolent North, and its articulation through scientific images. [York University, Toronto (Ontario) Canada. Email: e.jones.imhotep@utoronto.ca]

Lorraine CODE, *Knowing Ecologically: Feminist Theory and the Politics of Knowledge* — In this paper I outline the potential of an ecologically modeled epistemology to disrupt a hegemonic social imaginary of domination and control. Ecological naturalism interrogates the instrumental rationality, abstract individualism, reductivism, and exploitation of people and places that scientific epistemologies underwrite, to promote a social-political imaginary sensitive to human and geographical diversity, respectful of the natural world, and responsible in its democratic epistemic practices. Ecological thinking generates a freedom-enhancing conceptual apparatus that engages with feminist, multi-cultural, and other post-colonial issues to expose the local and global, human and environmental harms that epistemologies of mastery have enacted and to develop situated critiques of the imbrication of knowledge with power. I show this apparatus at work in the scientific practice of American naturalist

Rachel Carson and Canadian biologist Karen Messing. And I argue that the feminist question “whose knowledge are we talking about?” when issues about public trust and responsibility both collective and individual displace quests for necessary and sufficient conditions for knowledge “in general.” My larger claim, at which I can merely gesture here, is that despite the profusion of ecological discourses and despite contestations in the politics of ecology, the creative, restructuring possibilities of ecological thinking have yet to be realized. As humanism vied with theism in the seventeenth and eighteenth centuries, ecological thinking vies with capitalism in the twenty-first century: it engages so many interwoven, often contradictory issues—feminist, classist, environmental, post-colonial, homophobic, racist, sexist—that it requires multi-faceted chartings. [School of Women Studies, York University, 4700 Keele Street, Toronto (Ontario) M3J 1P3, Canada. Email : lcode@nexus.yorku.ca]

Neil EVERNDEN, *Nature, Discourse, and the “Crisis of Imagery”* — Persons wishing to initiate radically novel conversations often claim to be thwarted by deficiencies in conventional language. In the consequence of this lament, considerable attention has been paid to the vagaries of vocabulary. But there have also been recurring suggestions of a non-linguistic element in the genesis of ideational novelty, an element frequently, though not exclusively or adequately, referred to as “imagery.” And if, as claimed, this is an essential factor in the genesis of alternative forms for thought and discourse, further consideration seems warranted. That possibility, along with the intimations of a “crisis of imagery,” constitutes the concern of this paper. [York University, Toronto (Ontario) Canada.]

Metaphysics / *Métaphysique* (10:30-12:00 Victoria College 206)

Jean LEROUX (Université d'Ottawa)
chairman / *président*

Anjan CHAKRAVARTTY, *The Structuralist Conception of Objects* — Structural realism, a version of scientific realism, has recently re-entered mainstream discussions in the philosophy of science. The central notion of structure, however, is contested by its proponents. This paper explores the consequences of the two most prominent forms of contemporary structural realism for the notion of objecthood. Epistemic structuralists hold that we can know structural aspects of reality, but nothing about the natures of the relata whose relations define structures. Ontic structuralists hold that we can know structural aspects of reality, and that there is nothing else to know—objects are useful heuristic posits, but are

ultimately ontologically dispensable. I argue that neither of these forms of structuralism succeed in ridding a structuralist ontology of objects. What structural realism implies is not that objects do not exist, but rather that objects should be understood structurally. The question of how we get from causal properties (the “nodes” of detected structures) to a conception of objecthood may have different answers, depending on the putative objects and problems under investigation. Scientific investigations shed light on the ontological natures of particulars, but objects in general comprise a heterogeneous kind. [King's College, Cambridge CB2 1ST, UK. Email: Anjan.Chakravartty@kings.cam.ac.uk]

P. Kyle STANFORD, *Selective Confirmation: No Help for Scientific Realism* — Scientific realists have recently responded to Laudan's Pessimistic Induction with what I call the selective confirmation strategy: arguing that only the idle parts of genuinely successful past theories have been rejected, while their truly success-generating features have been confirmed by further inquiry. I first argue that existing efforts to apply this strategy suffer from a crucial unrecognized defect, in that one and the same present theory serves both as the standard to which components of a past theory must correspond in order to be judged true and as the background used to decide which of that theory's components genuinely contributed to its successes. Selective confirmation is therefore unconvincing without some prospectively applicable criterion of idleness that could have been applied by past theorists at the time to identify the idle posits of their theories in advance of future developments (and can now be similarly applied to our own). I go on to argue, however, that existing efforts to address the problem are thoroughly unconvincing: Kitcher's grounds for regarding the optical/electromagnetic ether as idle or "merely presuppositional" would convict all theoretical posits whatsoever of idleness (including the genes, atoms, molecules and electromagnetic field he hopes to defend as genuinely confirmed by their successes), while Psillos's proposal that scientists' own judgments of selective confirmation have turned out to be historically reliable stands refuted by sufficiently detailed consideration of his own central

historical examples: optical/electromagnetic ether and caloric fluid. Nor, I suggest, are the avenues for improving on these proposals at all promising. [Department of Logic and Philosophy of Science, University of California, Irvine, 5100 Social Sciences Plaza, Irvine, CA 92697-5100, USA. Email: stanford@uci.edu]

Jillian Scott MCINTOSH & Christian LACROIX, *Non-Supervening Emergence?* — Jaegwon Kim has recently claimed that, since emergence is a form of non-reductive materialism, it is part of "the new orthodoxy" in contemporary philosophy of mind (1999, p. 5). In his classic exposition of Emergentism, C.D. Broad (1925) tried to provide a view that was scientifically informed, ontologically parsimonious, and non-eliminativist. Current emergentists have the same desiderata, but the details of their theories vary. First, we sketch the motivation for emergence along with an immediate problem (or problems, depending on how one counts), informed by Kim's own notion of emergence as a form of supervenience. What's troubling about reductionism, how does a supervenience form of emergence attempt to avoid that, and why think it fails? Second, we outline Paul Humphreys' (1997) alternative view of emergence as a non-supervening relationship and assess its success at meeting said problem(s), arguing that it does not. Finally, we offer a tentative diagnosis of why our verdict differs from his. [University of Western Ontario. Email: mcintosh@uwo.ca]

Biomedical Sciences in Social Context / *Le contexte social des sciences biomédicales* (10:30-12:00 Victoria College 323)

Gordon MCOUAT (University of King's College)
chairman / *président*

Andrew REYNOLDS, *Metazoic Politics and Cellular Citizens: The Cell-State Metaphor in 19th Century Cell Biology* — In 1861 the German physiologist Ernst Brücke (1819-92) dubbed individual cells the "Elementarorganismen." This encouraged some scientists and philosophers to think of human beings as Leviathan-like colonies of these "elementary organisms" come together to form giant "cell-states" or "social colonies." The founder of cellular pathology, Rudolf Virchow (1821-1902), referred to multicellular organisms (metazoa) as "cell-republics", "democratic cell-states", and "commonwealths." Ernst Haeckel (1834-1919), the German biologist and philosopher, also made frequent use of this type of analogy. His evolutionary accounts of the origin of multicellular organisms from unicellular ancestors (amoebae) pays homage to natural selection and to radical liberal sentiment. These political metaphors naturally reflect the authors' political inclinations. (Virchow was dismissed from his hospital teaching position for actively participating in the 1848 uprising in Berlin. Haeckel was a radical liberal who opposed the repressive and

religious aristocracy of the Prussian Junkers.) Although these metaphors seem to reduce human beings to social colonies of independent cells, there is also a principle of holism involved. After all what makes all these independent cells obey the broader interests of their respective tissues, organs, and organisms as a whole? Were similar types of political metaphors used to describe cells and multicellular organisms in a more liberal and democratic country like Britain? The prevalence of Milne-Edwards' "division of labour" idea will be discussed, as well as the historical roots of the colonial view of complex organisms in Lorenz Oken and Leibniz. [Dept. of Philosophy and Religious Studies, University College of Cape Breton, Nova-Scotia, Canada. Email: areynolds@ucb.ns.ca]

Anne-Julie HOULE, *The Impact of Molecular Biology on Oncology: The Case of the Montreal Cancer Institute* — From the 1960s through to the 1980s, cancer was generally described as a problem of the regulation of one or many biological systems. However, a new definition of

cancer emerged in the scientific community following the discovery of the first oncogene in 1976 (Morange, 1997). According to the oncogene theory, oncogenes establish a specific pathway for the growth of cancer. Our study aims to evaluate how this paradigm shift transforms practices and research objects in oncology at the Montreal Cancer Institute (MCI). We studied the policies set up for the development of the Institute, the research organisation and the practices of the researchers. We have taken a special interest in the transformation of the research objects and experimental systems and the restructuring of scientific production and researcher training. Our research is based on an analysis of the MCI annual reports from 1970, researchers résumés and their publications and interviews with researchers and the scientific directors since 1975. [CIRST, Université du Québec à Montréal, CP 8888, Succ. Centre-Ville, Montréal (Québec) H3C 3P8, Canada. Courriel : houle.anne-julie@courrier.uqam.ca]

Ted EVERSON, *The Political Economy of Canadian Genomics* — I will outline a history of Canada's commitment to genomics, and an analysis of public health policy issues resulting from this commitment. My paper will describe the creation of CGAT and Genome Canada, the two

public institutions that have funded genomics research in Canada. I will focus on a variety of groups who influenced these developments, including the genomics community in Canada, the media, policymakers and the private sector. Analysis of the activities of these groups illustrates that Canadian genomics owes its success to its presumed economic, rather than public health, benefits. Economic potential and economic necessity were the primary lobby tools used to create momentum for a Canadian genomics infrastructure. The potential for future medical benefits, in addition to other public benefits, were also briefly identified in lobby efforts, but in a much more limited fashion. The creation of an industry—for the production of technological tools, and as a means for increasing production output in other industries such as agriculture, fisheries, pharmaceuticals and diagnostics—was the main lobby emphasis, and consequently the main motivator of Canadian genomics support. Debates regarding the potential benefits of genomics to public health, therefore, appear not to have been of primary consideration during the establishment of genomics programs in Canada. [IHPST, Room 316, Victoria College, 91 Charles Street West, University of Toronto, Toronto (Ontario) M5S 1K7, Canada. Email: ted.everson@utoronto.ca]

Lunch / *Dîner* (12:00-14:00)

Science, Literature and Popularization /
La science dans la littérature et la vulgarisation
(14:00-15:15 Victoria College 206)

Bernard LIGHTMAN (York University)
chairman / *président*

Eric PALMER, *Pangloss Identified* — Scholars have associated the character of Pangloss in Voltaire's *Candide* variously with the ideas of Gottfried Leibniz, Alexander Pope, and Christian Wolff. With them he is associated, but on whom is he modeled? Pangloss is the image of a French popularizer of science celebrated in his day but little noticed in ours: Noël Antoine Pluche (1688-1761), the author of a highly popular work, *Le Spectacle de la Nature* (1732). Pluche, almost as much as Pangloss, presents a caricature of more thorough contemporary reasoning about the character and plausible extent of scientific and metaphysical knowledge. That reasoning, the distortion presented by Pluche, and the magnified distortion of Pangloss will each be considered in this presentation. What was fantastically popular was at least as important to the public philosophe as what was most carefully and systematically reasoned. A regard for cultural context and for the historical era of composition of *Candide* is of value if we are to gain a measured grasp of the breadth and the focus of Voltaire's criticism, as well as a sense of the spread of philosophical ideas in European culture. [Philosophy, Allegheny College, 520 N. Main Street, Meadville, PA 16335, USA. Email: epalmer@pellns.alleg.edu]

Jean-Louis TRUDEL, *Verging on the vernacular: Technological dreams in popular science and technology magazines of the 19th century* — Popular science and technology magazines occupy an uneasy borderland between the sphere of professional practices and discourses, and the sphere of vernacular culture. Katherine Pandora (2001) has identified vernacular culture as that of the everyday forms of communication and activity that mark the public discourse. It is an "intellectual commons" where social and theoretical commentary can circulate without regard for scientific propriety. Unauthorized by nature, the vernacular sphere is open to speculations at odds with the norms of academic science or professional technology. Often unstructured and ephemeral, the vernacular discourse can nevertheless shape public opinion and communicate beliefs through the use of strong images and narratives. The clearest overlap between the subject matter of popular science and technology magazines and the topics of interest to the broader culture arguably results from the presentation of technological dreams by such publications. Though didactic reportage may cover items with more immediate impact, such dreams include collective projects with the allure of unbounded promise. While today's possi-

bilities still manage to mine the “progress narrative,” popular science and technology magazines first articulated this narrative in the nineteenth century. Therefore, it is the presence and framing of such technological dreams in such magazines as *Cosmos*, *La Nature*, and *Popular Science Monthly* before 1900 that will be characterized here, as well as their support of and reliance on the progress narrative. [CIRST, Université du Québec à Montréal, CP 8888, Succ. Centre-Ville, Montréal (Québec) H3C 3P8, Canada. Email: d364034@er.uqam.ca]

Jean-François CHASSAY, *Regards croisés: l'écrivain et le physicien face au réel dans Atlas occidental de Daniele Del Giudice* — Cette communication vise à suggérer quelques pistes de réflexion sur les rapports entre science et littérature. Il existe des textes qu'on peut qualifier de “fictions scientifiques” : ils cherchent à rendre compte des

modifications de la conscience, des perceptions produites par ce que les sciences révèlent du monde; ils interrogent les possibilités et les effets de la connaissance scientifique à travers les états du langage, sa logique, ses contraintes, ses limites; ils utilisent les développements de la recherche scientifique pour les besoins de de l'intrigue romanesque. Dans ce contexte, la communication s'intéressera plus particulièrement au roman *Atlas occidental* de l'Italien Daniele Del Giudice, qui raconte la rencontre entre un jeune physicien nommé Brahé qui travaille sur les particules subatomiques au CERN à Genève, et un vieil écrivain, Epstein, connu, adulé, “nobélisable.” Ce roman propose le développement de leur amitié, mais aussi, subtilement, le regard commun qu'ils posent sur le réel et sur la complexité qui le fonde. [Département d'études littéraires, Université du Québec à Montréal, CP 8888, Succ. Centre-Ville, Montréal (Québec) H3C 3P8, Canada. Email: chassay.jean-francois@uqam.ca]

19th Century Philosophy of Science / *La philosophie des sciences au XIX^e siècle* (14:00-15:15 Victoria College 323)

MARGARET SCHABAS (University of British Columbia)
chairman / *président*

Jean Leroux, *De la vérité en sciences. Considérations sur la tradition épistémologique du 19^e siècle* — Nous voulons indiquer comment les conceptions de la physique élaborées par la tradition épistémologique des « savants-philosophes » tels Hermann von Helmholtz, Heinrich Hertz et Henri Poincaré, ont fait l'économie de tout concept de vérité en science qui ne soit pas de teneur pragmatique. En particulier, la notion de vérité-correspondance était expressément absente de ces conceptions. Nous soulignerons d'abord l'influence du criticisme kantien en ce qui concerne l'attitude fondamentale de ces auteurs face à la notion de vérité. Nous ferons ensuite allusion à trois développements qui ont orienté l'épistémologie vers une image de la science qui n'offre aucune prise à la notion de vérité-correspondance: a) l'adoption par Helmholtz d'une conception des sensations en tant que signes, et sa généralisation à la théorie physique entière accomplie par Hertz, b) les études sur les fondements de la géométrie dans les dernières décennies du siècle et les leçons conventionnalistes qu'en a tiré Poincaré, et c) les développements de la théorie physique même durant la dernière moitié du 19^e siècle qui ont incité à concevoir celles-ci comme de simples descriptions (et non comme des explications) des phénomènes naturels. Nous suggérons enfin que ces développements ont marqué un processus de désontologisation de l'image scientifique du monde, à tout le moins dans la mesure où il est considéré que la notion (d'allégeance réaliste) de vérité-correspondance n'est aucunement prédictible de cette image. [Département de philosophie, Université d'Ottawa, Ottawa (Ontario) Canada. Courriel : jleroux@uottawa.ca]

Frederic BOUCHARD, *An Examination of Peirce's Intuition about the Statistical Character of Evolutionary Theory* — In *Fixation of Belief*, C.S. Peirce (1877) alludes to a possible analogy between the statistical character of evolutionary theory and thermodynamics. The general consensus today is that some evolutionary processes are ontologically probabilistic. The disagreement is about the origin of probabilities in evolutionary theory. In this paper I will examine current alternatives and discard them in favour of Peirce's original intuition. The alternatives currently considered are partly right: the probabilities are function of our epistemic limitations (Graves, Horan and Rosenberg, 1999) or the probabilities are the result of some sort of propensities (Brandon and Carson, 1996 and Stamos, 2000). Each contemporary account highlights an important aspect of the theory, but a complete explanation of the statistical character of evolutionary theory hasn't been provided. Although Peirce's understanding of Darwin's theory and motives were flawed, his insight can still be vindicated. [Philosophy Department, Duke University, 201 West Duke Building, East Campus, Durham NC USA 27708. Email : fb3@duke.edu]

Mary Pickard WINSOR, *Whewell's Method of Types, the Very Antithesis of Typology* — In 1840 William Whewell, the 19th century's greatest philosopher of science (according to Ian Hacking), studied the impressive progress botanists and zoologists were making and judged that they had created a new method, more appropriate for their subject than the method of definition formulated by logicians. Although Whewell labelled this the Method of Types, it stands in direct

opposition to essentialist types. Historical examples of taxonomic practice support Whewell's analysis and directly challenge the picture about the dominance of essentialism before Darwin that is now uncritically accepted. [IHPST,

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Break / *Pause* (15:15-15:30)

Digging Science / *Creuser la science*

(15:30-17:00 Victoria College 206)

Eric MILLS (Dalhousie University)
chairman / *président*

David MCGEE, *A Second Battle of the Pyramids: An Early Episode in the Development of Scientific Archaeology* — In late 1867 and early 1868 a brutal battle took place at the Royal Society of the Edinburgh over the meaning of the Great Pyramid of Giza. On one side of the fight was Astronomer Royal of Scotland Charles Piazzi Smyth, who believed science proved that the Great Pyramid was of Divine inspiration, and intended to be used as a metrological monument. On the other side of the battle was James Young Simpson, one of the most famous medical men of his time, but also an ardent antiquarian and master of the latest developments in new field of prehistoric archaeology. This paper examines the battle in two ways, firstly as a fight between conflicting mid-century visions of prehistory, and secondly as a battle over proper methods in the continuing 19th century to make archaeology into a science. My argument is that this battle was important in defining the border between what was and what was not acceptable by way of argument in scientific archaeology. [Burndy Library, Dibner Institute, Cambridge MA, USA. Email: dmcgee@dibinst.mit.edu]

J. CONOR BURNS, *Moundbuilders and Material Culture, 1884-1894: An Alternative Approach for Understanding the History of Prehistoric Archaeology* — Thousands of large-scale earthwork structures were once distributed throughout the Ohio River watershed, and these mounds became controversial objects for archaeological inquiry in the 1880s and 90s. Archaeologists wanted to understand the historical relationship between living Indians, who no longer lived in direct association with mounds, and the archaeological record of the mounds' creators. Historians have described the institutional and cognitive dimensions of this period in the history of archaeology. They have emphasized the role of the Smithsonian's Bureau of Ethnology in exerting institutional authority over a vast, heterogeneous network of amateurs in order to establish a "correct" scientific belief regarding mounds, bringing the "moundbuilder controversy" to a close. My research on mound archaeology has focused on the practical management of archaeological data-on details such

as site excavation techniques and museum curatorship of artifacts-within the considerably complex social structures of nineteenth century American science. In this paper, I will discuss investigations of Serpent Mound, Ohio with special attention to the physical and social management of archaeological data via Harvard's Peabody Museum of American Archaeology and Ethnology. I will argue that this level of analysis might improve our understanding of the development of prehistoric archaeology-an important branch of anthropology-and of the conditions under which excavation, documentation, and interpretation of archaeological sites occurs. [IHPST, University of Toronto, Victoria College, Room 316, 91 Charles Street West, Toronto (Ontario) M5S 1K7, Canada. Email: jburns@chass.utoronto.ca]

Rob-Roy DOUGLAS, *Hiding Dirty Linen: The Geological Survey of Canada, Alexander Galt and the Bow and Belly River Coal Deposits* — 19th Century Canadian politics is generally strewn with incidents of graft, political patronage and corruption. The Geological Survey of Canada, however, was supposed to be above all that. Corruption was kept at arms length by the strong arm of the directors, especially the first three, Sir William Logan, ARC Selwyn and GM Dawson. Under their control the Survey was for the benefit of all Canadians, not just a favoured few. This is the received view of the Survey, one that has endured for over a century and a half. It is also incorrect. In at least one instance, the morals of the Survey fell well short of their standards. After surveying the geology of the Bow and Belly River region. GM Dawson informed Alexander Galt, a land developer and Canadian High Commissioner in London, of his discovery of a major coal deposit there. In order to allow Galt time to establish his claim, Dawson delayed publication of his report on the region for two years. Galt went on to become immensely wealthy from his interests in the coal deposits. Dawson received an unspecified payment for his services. Whether Dawson's superiors knew anything of his dealings with Galt, is unclear. Certainly it never became public knowledge, leaving his reputation, and that of the Survey, untarnished. [University of Toronto]

Themes in the Philosophy of Science / *Thèmes en philosophie des sciences* (15:30-17:00 Victoria College 323)

Anjan CHAKRAVARTY (Cambridge University)
chairman / *président*

Zvi BIENER & Chris SMEENK, *Is Gravity at the Heart of the Matter? Mathematics and Philosophy in the Newton-Cotes Correspondence* — At the heart of Newton's achievement in the *Principia* lies an innovative conception of matter and matter's relation to gravitation. Modern readers are tempted to see this conception as the familiar idea of mass. In fact, Newton uses "quantity of matter" throughout the *Principia* as a measure of a body's response to impressed forces. However, this *dynamical* conception of matter exists alongside a *geometrical* conception that is often ignored due to its apparently less important role in the *Principia* itself and its neglect by subsequent developers of Newtonian theory. On this conception, introduced in *De Gravitatione*, the quantity of matter is to be measured by the *amount of space* filled by a body. We argue that this forgotten conception is essential for understanding Newton's broad theory of matter and its philosophical foundations. The relation of these two conceptions of matter to the philosophical foundations of Newtonian theory will be articulated through an analysis of the correspondence between Newton and Roger Cotes, editor of the *Principia*'s second edition. Cotes shows that Newton's use of the two conceptions in the Argument for Universal Gravitation is inappropriate since the two conceptions are at odds with one another both *philosophically* and *mathematically*. Mathematically, the two conceptions lead to conflicting ways of quantifying the proportionality between matter and gravitation and thus Newton must make strong and unjustified assumptions regarding their equivalence in order to salvage the main argument of Book III. Philosophically, the two conceptions reveal the precarious methodological role the Third Rule of Philosophizing plays in Newton's experimental philosophy, particularly in ascertaining the essential properties of matter. [Dept. of History and Philosophy of Science, 1017 Cathedral of Learning, University of Pittsburgh, Pittsburgh, PA 15217, USA. Email: zvb1@pitt.edu]

Ravi GOMATAM, *What is Neils Bohr's Interpretation?* — A central tenet, common to the (various strands of the) so-called Copenhagen interpretation, that the ψ function provides a description of the *state of the individual system* is *incompatible* with Bohr's own interpretation. Bohr instead mooted an idea, call it the inseparability hypothesis, according to which the ψ function represents the state of the individual quantum system and the *totality* of the experimental arrangement conceived as a single *epistemic* whole. The ψ function is not to be interpreted as a "state" in any physical sense, much less a complete physical state, of the individual system. In this sense, Bohr was much more in agreement with Einstein than is commonly supposed. Bohr differed from Einstein only over whether a conception of the independent state of the individual system can be at all provided by *any*

theory. We shall trace in detail how, using the idea of inseparability, Bohr strove to avoid all contradictions and paradoxes, and at the same time demonstrate the predictive completeness of QT by attributing to the quantum formalism, a new *epistemic* mode of description. In so far as inseparability itself is unexplained, Bohr's interpretation remains an unsatisfactory rationalization. However, situating his inseparability hypothesis within a logically prior idea that I shall call *proto-relationality*, we shall show that Bohr's interpretive move contains a clue for going past the limitations of present use of ordinary language, thus opening the possibility for an ontological interpretation. If so, besides clarifying historically the interpretive content of Bohr's ideas and differentiating it from the so-called Copenhagen interpretation, our reading will point out the use that his ideas may yet have for the ongoing project of developing an ontological interpretation of QT. [Bhaktivedanta Institute, 2334 Stuart Street, Berkeley CA 94705, USA. Email: rgomatam@bvinst.edu]

Gualtiero PICCININI, *Computing Mechanism* — I offer a novel explication of what it means to be a computing mechanism. The main tool I employ is functional analysis understood as in engineering and physiology, where a functional analysis of a mechanism is a partition of a mechanism into component parts and an assignment of functions to the system's parts. The present account analyzes calculators and computers in terms of their components parts (processor, memory, input and output devices) and their functions. Those components are also analyzed in terms of their component parts (registers and circuits) and their functions. Those, in turn, are analyzed in terms of primitive computing components (logic gates) and their functions. Primitive computing components can be further analyzed but their analysis does not illuminate the notion of computing mechanism. In contrast to extant philosophical explications of computing mechanisms, the present account applies very naturally to stored-program digital computers and matches the language and practices of computer designers. It also satisfies four desiderata better than the competition: (1) it is observer-independent, i.e. it does not allow different observers with different goals to attribute different computations to the same mechanism; (2) it explains why calculators and computers compute while planetary systems and hurricanes don't; (3) it explains the difference between computers properly so called (i.e. all-purpose, stored-program computers) and less powerful computing mechanisms; and (4) it explains what it means for a computing mechanism to make a mistake in a computation. This makes the present account ideal for grounding discussions of computational theories of mind and brain. [Department of History and Philosophy of Science, University of Pittsburgh, 1017 Cathedral of Learning, Pittsburgh, PA 15208, USA. Email: gupst1@pitt.edu]

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