

**Abounader, Brooke, “Mathematical and Visuospatial Models in Early Quantum Chemistry”**

Session III.2B Tue May 31 14:00–14:40 MIDSX 110

In the late 1920s and 1930s, attempts to understand the hydrogen ( $H_2$ ) molecule led to the emergence of two simultaneously developed distinct approaches to chemical bonding. In the molecular-orbital approach, advocated by Robert Mulliken, the atomic nuclei are considered as a set. In the atomic-orbital (or valence-bond) approach, supported by Linus Pauling, each nucleus is a distinct entity. The molecular-orbital approach includes detailed mathematical models, while the atomic-orbital approach is associated with Pauling’s visuospatial models. Using primary source documents authored by Pauling and Mulliken, this paper addresses the nature and role of these models with respect to the relevant work of philosophers Nancy Cartwright, Mary Morgan, and Margaret Morrison. I examine and characterize the relationships between broad quantum theory, Mulliken’s and Pauling’s particular views of chemical bonding, their mathematical and visuospatial models of the atom and chemical bond, and their target system(s) in the physical world. I maintain a clear distinction between model and theory while illustrating the mutually influential interactions between these two entities.

**Anderson, Katherine, “A Curious Alienation: Meteorology and Physical Science”**

Session 1.4A Sun May 29 16:10–16:50 MIDSX 15A

In 1926, Napier Shaw, a Cavendish-trained scientist who became the director of the Meteorological Office, spoke of a “curious alienation of the experimental and theoretical physicist” from meteorology that – in Shaw’s view at least – had existed for several decades. Shaw’s comment points to a puzzle about disciplines and their identity, both for contemporaries and for historians of science. Was meteorology a physical science, a nascent physical science, or something else altogether? Who asked and who answered this question, and why? This paper considers the evolution of the identity of meteorology as a discipline through the nineteenth century and then investigates when and how prominent ‘experimental and theoretical physicists’ of Victorian Britain, like John Tyndall and William Thomson, did indeed involve themselves with meteorological research. These encounters illuminate the mixture of methodological and social concerns in Victorian science, but, more broadly, they also raise questions about the influence of disciplines and their categories in historical studies of science.

**Bennett, Bryce, “Time Waits for Gnomon: What the Simple Sundial Tells Us About Science”**

Session I.1A Sun May 29 9:40–10:20 MIDSX 15A

Since ancient astronomers began to employ simple sundials, it has been known that they do not provide a consistent measure of the passage of time. This results from ‘inequalities’ in the motion of the Sun. From the time of the Greek astronomer Hipparchos, the problem of the Sun’s motion resisted a satisfactory explanation.

Of course, it is actually the Earth that is in non-uniform motion about the Sun. However, the fact of the Earth’s motion, now accepted by any rational observer, is not as obvious as we might suppose from our modern perspective.

The person who solved the mystery of the motion of the Sun was Johannes Kepler (1571-1630). Kepler's contribution to modern science is often overlooked, regarded as an inevitable consequence of Copernicus' sun-centred theory of the orbs, or seen as 'preliminary' to the great achievements of Newton. But Kepler's explanation of the solar inequalities was the first in history to employ the physical concept of 'force' in the modern sense, a new and radical departure from previous astronomy. Kepler knew he was on to the truth, when, with the same conceptual tools, he could also explain the motions of the (other) planets.

This talk will show how the simple sundial can tell us much about science.

**Bennett, Bryce, "The Philosophy of Heinrich Hertz: Consistency, Correspondence and Convention"**

Session II.3B Mon May 30 14:00–14:40 MIDSX 105B

Heinrich Hertz (1857-94) is best known for his experimental studies of the phenomena associated with the propagation of electromagnetic displacements, the results of which were published in a series of papers that appeared in *Weidemann's Annalen* from 1887 to 1890. Both in terms of the advance made over earlier science, and in terms of their influence on later science, Hertz's electromagnetic experiments were of unquestioned significance. The philosophical component of Hertz's work, however, is less often acknowledged. In addition to his many experimental researches, Hertz also pursued strictly theoretical studies. His final, and greatest theoretical work, *Principien der Mechanik*, published in 1894, presents an axiomatic deductive theory of mechanics that presumes only three logical elements, from which all the laws of mechanics are deducible.

This paper will present an account of the philosophy of Hertz with the aim of establishing that Hertz's accomplishments in both electromagnetic and mechanical physics developed out of the epistemological considerations that he took as underwriting all scientific theories. This account will emphasize the view that Hertz succeeded in achieving his own fundamental scientific objective: that is, his writings in both electromagnetic and mechanical physics provide accounts of the phenomena that are superlative in respect to their logical clarity and simplicity.

**Bluhm, Robyn, "Mechanisms and Medicine"**

Session III.1A Tue May 31 11:10–11:50 MIDSX 15A

In this paper, I consider the importance of the concept of a mechanism for the philosophy of biomedical science and of medicine. Recent work in philosophy of science has investigated the importance of the elucidation of mechanisms in biological research in physiology and molecular biology. Only rarely, however, has this research acknowledged the implications of the concept of a mechanism for our understanding of disease; the focus tends to be on the normal, rather than pathological, functioning of mechanisms. Using research on diabetes during the early and mid-twentieth century as a case study, I will show how the search for an underlying disease mechanism informed research progress during this time. To show the importance of the idea of a mechanism for biomedical research, I will begin with the definition of a mechanism given by Machamer, Craver and Darden (2000) and then elaborate on their account by drawing on the work of Schaffner, Thagard, and Bechtel and Richardson, which emphasizes the importance of examining physiological mechanisms at different levels of analysis (e.g. anatomy, physiology, molecular biology). In the case of diabetes research, the search for disease mechanisms has had implications not only for our understanding of the

disease's etiology (i.e. by providing a scientific explanation of diabetes) but also for diagnosis and treatment, and so for the practice of medicine. Thus, a better understanding of how mechanisms are understood by medical researchers has implications for both philosophy of science and philosophy of medicine.

**Boulos, Pierre, and Marcello Guarini, “Can ECHO support a Theory of Explanatory Coherence?”**

Session I.3B Sun May 29 14:40–15:20 MIDSX 105B

Paul Thagard has put forward a theory of coherence as constraint satisfaction, and he has applied that theory to a number of different philosophical and scientific problems. The purpose of this paper will be to point out concerns with some applications of his connectionist coherence theory to scientific reasoning. While he has applied connectionist coherence theory to practical reasoning as well, a proper examination of that topic is beyond the scope this paper.

Part two of this paper contains (a) a general, abstract account of Thagard's connectionist theory of coherence as constraint satisfaction, and (b) a general account of his Theory of Explanatory Coherence (TEC). Thagard makes use of the computer program ECHO (Explanatory Coherence by Harmony Optimization), to implement TEC, and he uses that program to model various episodes in the history of scientific reasoning. Part three will put forward underdetermination arguments that undercut the support that ECHO simulations are said to provide for TEC. Part four concludes with discussion of critical responses to Thagard's work.

We focus here on limitations of connectionist coherence theory. However, this is not to say that it does not have its strengths. For example, Eliasmith and Thagard argue that connectionist coherence theory has some interesting advantages over probabilistic approaches to theory assessment (1997, 12-15). So while the general tone of this paper is critical, we do not mean to suggest any of the following: (a) that there is no value to connectionist coherence theory; (b) that there is no value to connectionist cognitive modeling, and (c) that naturalized approaches to epistemology are of no use. If fact, we reject (a), (b), and (c): what we offer is an internal critique of a specific type of computational epistemology/ philosophy of science.

**Burns, Elizabeth A., “The *Somnium* as a Scientific Treatise”**

Session I.2A Sun May 29 10:30–11:10 MIDSX 15A

Johannes Kepler's fictional work, *Somnium*, in which he describes a cosmic voyage to the moon, is generally not counted amongst his astronomical treatises. While the *Somnium* is meant to be a piece of fiction, Kepler clearly had a scientific agenda. In describing the astronomy of the moon, Kepler relies on his already advanced astronomical theory. Although the *Somnium* is less rigorous than Kepler's other works, it, nevertheless, discusses lunar astronomy in both quantitative and quantitative terms, thus supporting his cosmology. By showing that the astronomy in the *Somnium* adheres to Kepler's other scientific works, I will argue that Kepler's *Somnium* should not be viewed as a purely fictional work, but instead, it should be analysed as a scientific treatise on lunar theory. Furthermore, while the *Somnium* complies with the Copernican system, I will show that Kepler's conceptions about the moon are also largely influenced by his belief that the Earth has a privileged place in the universe. Accordingly, the lunar theory of the *Somnium* is placed in the context of Kepler's larger cosmological beliefs.

**Butler, Alison, “Nineteenth-Century Science and Sorcery, or Louis Pasteur Must Die”**  
Session I.3A Sun May 29 14:00–14:40 MIDSX 15A

In 1887, the famous French scientist, Louis Pasteur, was recovering from an unknown illness on the French Riviera. Hundreds of miles away in London, Anna Kingsford was celebrating the partial success of her occult attempts to murder the scientist. Had Pasteur succumbed, he would have been Kingsford’s third victim in her magical efforts to rid the world of those “demons of vivisection”, fellow researchers she encountered during her medical studies in Paris.

Kingsford was an extraordinary scientist and mystic who experienced visions she believed were divinely sent in order for her to recover an ancient theological system that would mend the growing rift between science and religion. In recording this system, Kingsford conflated science with sorcery and vilified vivisectors as black magicians, claiming that the realm of sorcery had invaded that of science.

Through an analysis of her work, this paper will examine the curious intellectual blend of scientific naturalism and the belief in magic that characterized a marginalized population in Victorian England. While naturalist thinkers strove to establish a scientifically-directed culture and to eradicate “superstitious” thought, magical practice and belief were gaining popularity in certain circles in Victorian England. Groups such as the Theosophical Society and the Hermetic Society sought to maintain the mystery and spirituality associated with religious and magical thinking, while at the same time acknowledging the giant strides undertaken by science. Kingsford was a prominent supporter of this reconciliation and her ability to merge major doctrines and tenets from both Victorian science and magic demonstrates their unexpected compatibility.

**Chakravartty, Anjan, “Representing and Describing”**  
Session I.1B Sun May 29 9:00–9:40 MIDSX 105B

The ontological nature of scientific theories has been a matter of controversy since the demise of logical empiricism. Rejecting the idea that theories are axiomatic systems of statements, expressed in a formal language and closed under deduction, many have proposed variations on the theme that theories are rather collections of models. One of the primary aims of this approach is to replace an essentially linguistic conception of theories with a non-linguistic conception, thereby avoiding the challenge of having to make sense of the relation between pieces of language and parts of world. In this paper I consider whether this aim is reasonable by examining the ways in which theories and models are used to represent and describe objects of scientific investigation. I will argue that the flight from language is not well advised after all. The moral is not, however, that there is no alternative to logical empiricist conceptions of theories, nor that models are unimportant in the context of scientific theorizing. Rather, it is that the ontological nature of theories is largely a matter of convention, and that however one chooses, one must face up to certain epistemic challenges inherent in the nature of representation and description.

**Contessa, Gabriele, “Scientific Representation, Similarity and Prediction”**  
Session I.2B Sun May 29 10:30–11:10 MIDSX 105B

In this paper, I will argue that Ronald Giere's similarity account of scientific representation is unable to account for the predictive use of models. According to Giere, a model represents a target system in virtue of the fact that a user uses the model to represent the system for certain purposes and the model is similar to the system in certain respects and to a certain degree. I will argue that the similarity account is unable to account for cases in which we use a model to predict certain aspects of the behaviour of a system. When we use a model to predict an aspect of the behaviour of the system, we often do not know in advance that the model is similar to the system in the very aspect we want the model to predict (the output). Similarity of the inputs is not sufficient to warrant similarity of the output. In some cases, we could assume that the model is similar to the system in all those aspects that are causally relevant to the output. However, in other cases, such a requirement would be too strong. Often, we do not believe our predictively successful models to encompass all the causally relevant factors. Similarity of the inputs is not even sufficient to warrant similarity of the outputs. As far as prediction is concerned, the similarity account fails to account for how model represent their target system. In the paper I illustrate my claim by means of a model from classical mechanics and one from nuclear physics.

**Dea, Shannon, “Our Hypotheses are Elephants’: Thomas Reid’s Rigourized Anti-Hypotheticalism”**

Session III.1B Tue May 31 10:40–11:20 MIDSX 110

Hypothesis was one of Thomas Reid’s favourite bugbears. As early as 1751 (and conceivably much earlier), in his lectures on natural philosophy, Reid cautioned his students about the use of hypotheses. For the duration of his career, his lectures, correspondence and published works were replete with attacks on the use of hypothesis, and with assertions that the proper method for philosophy resided not in conjecture, but in “chaste” and “patient” induction, after the model of Bacon and Newton. In my paper, I closely consider Reid’s account of hypothesis, in light of Newton’s own. I argue that Reid rigourized what was, for Newton, a vague term, *via* his “true causes” reinterpretation of Newton’s first rule of philosophizing, and *via* his extension of Newton’s prohibition against hypothesizing beyond experimental philosophy to philosophy *tout court*. Finally, I draw upon Reid’s 16 December 1780 letter to Lord Kames to show the heavy toll that was exacted by his rigourization of the term “hypothesis”. Forced by Kames to admit that hypothesis has some utility in research, Reid was obliged to sharply distinguish between natural philosophy and metaphysics, and to admit the study of non-empirically-testable propositions about the world into philosophy as a whole.

**DeVidi, David, “Constructive Logics without Constructivism?”**

Session II.2B Mon May 30 10:30–11:10 MIDSX 105B

Constructive (aka Intuitionistic) logics originate in a particular approach to foundations of mathematics, but they also figures in many debates in contemporary metaphysics. This is largely due to the influence of Michael Dummett. He suggested that *anti-realists* of various stripes needed to develop *theories of meaning* for discussions of the sorts of things they were claiming not to be real, and that the only viable anti-realist meaning theory to date is the one worked out by constructivists. Hence the first approach for anti-realists in areas other than mathematics should be to pattern their meaning theories on the constructivist one, and so we might expect them to wind up, as the constructivists did, endorsing a logic quite different

from the standard classical one. Increasingly, though, constructive logics are being recommended as important parts of solutions to problems in a variety of areas in philosophy.

In this paper I will briefly sketch a few cases of this. I will then argue that these solutions are incompatible with anything akin to constructivism. This raises the question of whether other grounds can be offered for the suggestion that constructive logic is correct than those offered by constructivists. If not, the proposed solutions are painfully ad hoc. The paper concludes with an assessment of the prospects for non-constructivist endorsement of constructive logics.

**Douglas, Rob-Roy, “Imaginary Mountains and National Frontiers: George Mercer Dawson on the Geography of British Columbia”**

Session II.2A Mon May 30 11:10–11:50 MIDSX 15A

On June 20 1877, George Mercer Dawson, the Geological Survey of Canada field officer in British Columbia, had a paper read before the Geological Society of London on the glacial geology of the province. His paper outlined the physiographic geology of British Columbia, noting the existence of the Rocky Mountains, the Selkirk Range, the Coast Range and the Vancouver Island Range. He also described, however, the existence of a transverse mountain range running along the 49<sup>th</sup> parallel, something that had eluded the notice of all previous scientific visitors to the region, including geologists from the Palliser Expedition and the British Boundary Commission.

There were compelling scientific and political reasons why these mountains would appear attractive to Canadians in the early years of the Dominion. They provided a physiographic justification for the international boundary in British Columbia, while also demarcating the Canadian Cordillera from the American Cordillera. Of course they also did not exist, as Dawson himself concluded upon visiting the region himself in 1877. That he did postulate their existence, despite the lack of evidence from his predecessors, is indicative of the political circumstances of the 1870’s and their influence on course of science and the interpretation of evidence from the field.

**Edgar, Scott, “Paul Natorp’s Argument Against Psychologism”**

Session II.1A Mon May 30 9:00–9:40 MIDSX 15A

Arguments against psychologism are well-known in both 19<sup>th</sup> Century philosophy of mathematics and in Husserl’s phenomenology. Less familiar, though, are arguments against psychologism in 19<sup>th</sup> Century philosophy of empirical science. Nevertheless, Paul Natorp, a 19<sup>th</sup> Century Neo-Kantian from the Marburg School offers a distinctive argument against psychologistic approaches to the epistemology of natural science. In fact, his argument can be seen as an instance of a kind of argument that was not uncommon in the second half of the 19<sup>th</sup> Century: an argument that turns on the Kantian idea of “conditions of possible experience”. However, other figures who use the idea of conditions of possible experience, such as Hermann von Helmholtz and F.A. Lange, do not use it to object to psychologism. On the contrary, both offer what can be seen as thoroughly psychological accounts of the conditions of possible experience.

I want to make two points about Natorp’s argument. First, I will argue that he turns it into an objection to psychologism by modifying it in a way suggested by comments made by Hermann Lotze. Second, I will argue that the innovation implicit in Natorp’s argument—the

innovation that does the argument's work—is a distinctive conception of the relation between epistemology and the empirical sciences.

**England, Richard, “The Scientific Accuracy of the Bible: Authority, Honesty and**

**Deference in debates over Kinns’ Moses and Geology(1882)”**

Session II.2A Mon May 30 10:30–11:10 MIDSX 15A

Samuel Kinns' *Moses and Geology* (1882) was prefaced by a most unusual list of subscribers that proved more controversial than the content of his work. Kinns decorated his popular science book with the names of bishops who supported him, but buttressed it with the names of men of science who, he claimed, testified to the accuracy of his facts. Kinns' claims about the harmony of science and the Bible hinged on minute scientific details, and his readers were reassured by his parade of scientific experts. However, when his claims to scientific authority were noticed in a report in the *Times* there was a flurry of letters from critics and supporters, each claiming scientific authority. Kinns was accused of misappropriating the names of scientists without their consent. While some minor men of science did back Kinns, more prominent scientists attacked them for getting facts wrong and propping up Kinns' “false science”. His closest supporters stood by him, even as leading astronomers and botanists called for them to withdraw their support. As in many apparent “science vs. religion” debates, both Kinns supporters and critics made scientific claims in the interest of defending religion. While elite scientists tried to destroy his influence, he managed to prosper, though at some personal cost. We see here the diverse impacts of public controversy on sales and credibility, and the power of newspapers like the *Times* to shape popular readings of the relationship between science and religion.

**Feke, Jacqueline, “Copernican Fiction”**

Session I.2A Sun May 29 11:10–11:50 MIDSX 15A

The expanding influence of Copernicanism through the seventeenth century transformed not only the natural philosophic leanings of astronomers but also the store of conceptual material accessible to writers of fiction. During this period of scientific revolution, a new literary genre arose, namely that of the scientific cosmic voyage. Scientists and writers alike constructed fantastical tales in which fictional characters journey to the moon, sun, and planets. In so doing, they discover that these once remote worlds are themselves earth-like in character. Descriptions of these planetary bodies as terrestrial in kind demonstrate the seventeenth-century intellectual shift from the Aristotelian to the Copernican framework. By positing the tripartite motion of the earth, the Copernican paradigm broke down the boundary between the terrestrial and celestial realms championed by Aristotelian physics. Because the earth is planetary, the planets, in turn, must be earth-like. While astronomers argued the former, fictionists imagined the latter. My paper will take Johannes Kepler's *Somnium*, Francis Godwin's *The Man in the Moon*, and Cyrano de Bergerac's *Voyages to the Moon and Sun* as case studies exhibiting the influence of Copernicanism on seventeenth-century works of fiction.

**Frappier, Mélanie, “The EPR argument under the lens of Heisenberg's microscope”**

Session III.2B Tue May 31 14:40–15:20 MIDSX 110

In 1927, Heisenberg introduced the uncertainty relations using the microscope thought experiment. The traditional rendering of the experiment, suggested in part by Heisenberg's description of it, is that the simultaneous position and momentum of small objects, like electrons, can never be known precisely since the light used in their observation through a microscope will always disturb their motion in an intractable way. This usual disturbance description of the thought experiment has led many to believe that Heisenberg's analysis of the thought experiment makes use of the same problematic locality assumptions as Einstein's analysis of the EPR experiment and that it is therefore subject to same criticisms.

Given this reading of Heisenberg's view on quantum phenomena, his rebuttal of Einstein's analysis of the EPR experiment as not defining correctly what constitutes the experimental set-ups of the possible measurements would be inconsistent with the rest of his views on quantum mechanics. I will show, however, that Heisenberg did not believe that the uncertainties in the indeterminacy relation were simple epistemological limits due a semi-classical disturbance of the electron's motion by photons and that, therefore, he did not share Einstein's locality assumptions. I will then show how this superior understanding of the presuppositions underlying quantum mechanics enabled Heisenberg to realize – years before the EPR thought experiment – both that entanglement arises between certain quantum systems, notably the photon-electron pair of the microscope experiment, and that this first analysis of the strange consequences of this quantum phenomenon allows for his dismissal of the EPR argument.

**Fraser, Doreen, “Haag’s Theorem and the Treatment of Interactions in Quantum Field Theory”**

Session III.2B Tue May 31 15:20–16:00 MIDSX 110

To this point, most of the interpretative work on quantum field theory (QFT) has focused on free field theories, even though any realistic theory of the world must describe interactions. This is probably due in part to the notorious complications that arise in the presence of interactions. Haag's theorem pinpoints some of the foundational issues that are peculiar to interacting QFT's. In spite of the fact that Rudolph Haag first stated a version of the theorem in 1955 and a generalization was rigorously proven by Hall and Wightman in 1957, the implications of Haag's theorem are not widely appreciated. I will sketch Haag's theorem and discuss a few of the foundational issues that it raises. In brief, the result is that if a purported representation for an interacting field satisfies a set of *prima facie* plausible assumptions, then it necessarily describes a free field. In particular, Haag's theorem entails that the interaction picture employed in canonical QFT necessarily describes a free field. In this sense, the interaction picture does not represent interactions. And yet, the interaction picture yields fantastically accurate predictions. One foundational issue is how this is possible. A second foundational issue is how Haag's theorem bears on the question of how to choose among presumably physically inequivalent representations. This is the choice presented by the uncountable set of unitarily inequivalent Hilbert space representations of the canonical commutation relations.

**Gauthier, Yvon, “L’intuitionnisme comme semi- constructivisme”**

Séance II.2B Lun 30 Mai 11:10–11:50 MIDSX 105B

De toutes les variétés du constructivisme, l'intuitionnisme brouwerien et sa succession logique, c'est-à-dire la logique intuitionniste de Heyting à Troelstra et van Dalen, ne paraissent pas avoir défendu une posture fondationnelle suffisamment radicale pour se rapprocher du constructivisme radical – que j'appellerai arithmétisme plutôt que finitisme pour évoquer Kronecker avant Hilbert. Ceux que l'on a appelés les semi-intuitionnistes français, Poincaré, Lebesgue ou Borel semblent plus près en réalité de Kronecker que de Brouwer. Le prédictivisme de Poincaré et à cet égard celui de Hermann Weyl jusqu'à Edward Nelson ou encore la mathématique numérique de Bishop répondent plus nettement aux exigences constructivistes.

Si le rejet du tiers exclu pour les suites infinies caractérise l'intuitionnisme brouwerien, il n'est pas toujours clair que l'on s'en tienne aux suites infiniment processives. Certains principes intuitionnistes transgressent les préceptes constructivistes. Je veux le montrer sur l'exemple de l'induction complète que l'on voudrait identifier à la méthode de la descente infinie; une telle identification n'a de sens qu'en logique classique(booléenne), puisque l'on opère une double négation sur un ensemble infini (de nombres naturels). Le tiers exclu est ainsi réintroduit subrepticement. Je citerai à l'appui de ma thèse et de sa démonstration les idées du pionnier du constructivisme, Kronecker, mais aussi celles de Kolmogorov, Poincaré et Hilbert.

### Sources

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**Gingras, Yves, “What can Historical reconstruction of experiments teach the sociology of scientific knowledge?”**

Session III.2A Tue May 31 15:20–16:00 MIDSX 15A

In the course of its development, the sociology of scientific knowledge(SSK) has often insisted on the marginal role of "nature" in the formation of consensus in science. SSK does not seem to have taken seriously the challenge of the practice of Historical reconstructions to its model of scientific practice. In this talk we will first recall the view of science according to SSK, explain how reconstructions presuppose a certain stability and resistance of "objects" to action that is incompatible with some implicit tenets of SSK and propose a modified model of the dynamic of science based on the notions of "constraint" and "argumentation" that looks less "radical" than SSK but is more in tune with the reality of scientific practice.

**Hamm, Ernst, “The ‘Temple of Serapis’ as icon in the history of science”**

Session I.4B Sun May 29 15:30–16:10 MIDSX 105B

The so-called “Temple of Serapis” at Pozzuoli, north of Naples, is one of the most celebrated images in the history of science, thanks to it serving as the frontispiece for Charles Lyell's *Principles of Geology*. The temple quickly became (at least for Lyellians) an emblem of

uniformitarianism; more than that, it has attained something like iconic status for geologists. A very recent (2004), and excellent, popular work by a distinguished palaeontologist claims that the temple and the area around it “is the nearest thing there to holy ground that there is” for geology. It is curious that a human built monument less than 2000 years old, barely an instant in geological time, should stand for gradual changes in the very long history of the earth championed by Lyell, who was by no means the last or the first observer to comment on the natural historical significance of this historical structure. This illustrated paper will consider the way geologists before and after Lyell have regarded the “Temple of Serapis” and argue that this structure and the way it has been interpreted sheds light on the character of geology as it established itself as a scientific discipline.

**Knaapen, Anne-Loes, “The Premenstrual Syndrome in the USA and France: Variation vs. standardization in biomedicine”**

Session III.1A Tue May 31 10:00–10:40 MIDSX 15A

The first medical description of the premenstrual syndrome (PMS) dates from 1931, and the term itself wasn’t coined until 1953. To trace the history of this syndrome in comparative perspective I did a content analysis of the medical literature devoted to it from 1950 to 2004, focusing on French and American articles. Explaining the convergence and divergence of these histories, I hope to contribute to the expanding literature on variation and standardization within western biomedicine.

Until the 1980’s, PMS was in both countries a fairly common problem treated by general practitioners and gynecologists. Somatic symptoms like bodily swelling and headaches were treated with diuretics, analgesics and various hormones. After 1980 variation between French and American research began to develop. In the USA, in response to public controversy, researchers sought to quantify and objectify PMS symptoms. American psychiatrists joined gynecologists in pursuing PMS research; they focused on emotional symptoms like irritability and depression, designing strict diagnostic criteria that are included in the psychiatric Diagnostic and Statistical Manual of Mental Disorders (DSM). Somatic symptoms become increasingly unmentioned and unmeasured. Consequently, treatments, notably the SSRI antidepressants, have only targeted psychological symptoms. This psychiatric approach now dominates American as well as French PMS research, despite the French’s strong initial emphasis on somatic approaches.

We suggest that the increasing use of the DSM-IV provided an international standard for diagnosis and terminology, making research comparable across countries and disciplines. This power to standardize perhaps explains the success of the psychiatric approach to PMS research.

**Koenig, Tricia, “Jumping Neutrons! The implementation of a nuclear particle in astrophysics”**

Session I.1A Sun May 29 9:00–9:40 MIDSX 15A

In February 1932, James Chadwick announced the discovery of a new particle at the Cavendish Laboratory in Cambridge, England. Following an old suggestion of Rutherford, the new neutral particle was identified as a “proton and electron in close combination”, thus giving the neutron a metaphysical status similar to that of alpha particles. In the following three years, though, experimental and theoretical developments in nuclear physics forced the acceptance of the neutron as a new elementary particle, “something like a proton or an

electron”. While discussions on the nature of the new particle were going on among radioactivists and nuclear physicists, two astronomers, Walter Baade and Fritz Zwicky, used neutrons in formulating their supernovae theory in Pasadena, California. Their theory, first presented in November 1933, explained supernovae as a mechanism that produced neutron stars and cosmic rays, while concurrently coining the terms supernova and neutron star. Despite the hazy status and definition of a neutron at the time, it was implemented into astrophysics rather quickly. How did the neutron jump from nuclear physics circles to astrophysics? What metaphysical status did Baade and Zwicky attribute to their “neutron”? In this paper I want to discuss the way in which a particular scientific entity, the neutron, traveled between disciplines.

**Landry, Elaine, “The Notion and Uses of Shared Structure”**  
Session I.2B Sun May 29 11:50–12:30 MIDSX 105B

Recent approaches to scientific structuralism (Da Costa, French [1990]; Da Costa, Bueno, French, [1997]; and French [1999], [2000]), aiming to characterize the concept of shared structure between models, formally frame a model as a type of set-structure. In this paper, I challenge the idea that the notion of a model, and so the concept of shared structure between models, must be framed in set-theoretic terms.

I first investigate the Bourbaki-inspired assumption that models are types of set-structures and next consider the extent to which this problematic assumption underpins both Suppes’ and current semantic views of a scientific theory. I point out that, to account for the fact that two models share structure, we do not have to specify what models *qua* types of set-structures are. It is enough to say that, in the context under consideration, there is a morphism between the two systems (mathematical or physical) which makes precise the claim that they share the appropriate kind of structure.

I then use this investigation to show that there is no need to agree with French that “without a formal framework for explicating this concept of ‘structure-similarity’ it remains vague, just as Giere’s notion of similarity between models does...” (French [2000], p. 114). Neither notion is vague; both can be made precise by appealing to the notion of a morphism, but it is the context (and not the set-theoretic frame) that acts as the conceptual framework, i.e., that determines the appropriate kind of morphism. I make use of French’s [1999] own example from the development of quantum theory to show that it was the context of considering the “relevant symmetries” that determined that the appropriate kind of morphism was the one that preserved the shared Lie Group structure of both the mathematical and phenomenological models.

**LeBlanc, André, “The Thermodynamics of Freedom: Joseph Delboeuf on the Problem of Free Will”**  
Session III.1B Tue May 31 10:00–10:40 MIDSX 110

The work of the great Belgian philosopher and psychologist, Joseph Delboeuf (1831-1896), has undergone a major revival in recent years among scholars interested in the history and philosophy of hypnosis. But Delboeuf’s writings on hypnosis represent but a small fraction of his life’s work. In this paper, I propose to examine what Delboeuf believed to be his most important achievement: his investigations of the relationship between free will and the first law of thermodynamics. The first law of thermodynamics presents a special problem for belief in the existence of free will. If energy can neither be created nor destroyed, how then

could an organism influence the movement of matter within itself without violating this principle of energy conservation? This difficulty could be overcome, Delboeuf proposed, if the organism were capable of controlling the *duration* or the *timing* of physical events within its nervous system since time plays no part in the equations of thermodynamics. To my knowledge, no one has ever considered the possibility that the key to solving the problem of free will might lay in a deeper understanding of the nature of time. The purpose of my paper, therefore, will be to outline Delboeuf's proposed solution to the problem of free will and to relate it to some of the burgeoning literature on the topic. In addition to letters between Delboeuf and William James during the 1880s, I will also take into consideration recent work by Noam Chomsky, Daniel Dennett, Benjamin Libet and John Searle.

**Bernard Lightman, “Defining a New Audience: Women as Scientific Authors”**  
Session I.4A Sun May 29 15:30–16:10 MIDSX 15A

The second half of the nineteenth century was a golden age for female scientific authors. They wrote about virtually every aspect of the natural sciences. In opportunistic fashion they seized upon elements of previous traditions of female scientific authorship to forge a powerful, new tradition that reached out to a broad audience. Whereas female scientific authors of the first half of the century had self-consciously addressed an audience composed of women and children, their successors, like Mary Somerville, addressed adult males, as well as women and children. As they navigated around the barriers erected by the professionalization and masculinization of science, they retained the role of religious and ethical guides established by their predecessors in the “maternal” tradition. Defining a new audience involved careful attention to the use of scientific terminology and systems of classification, as well as experimentation with innovative genres in science writing.

**Marchand, Nicolas, “Défense de la nation contre prospérité de la discipline”**  
Séance II.1A Lun 30 Mai 9:40–10:20 MIDSX 15A

En marge de leurs ravages, les grands conflits militaires du XX<sup>e</sup> siècle ont profité à l'expertise scientifique et à l'essor des disciplines. Ils ont contribué à l'intensification du soutien étatique des institutions scientifiques (universités, instituts, centres de recherche, etc.) et de leurs acteurs (professeurs, chercheurs, étudiants, administrateurs, etc.). L'expertise scientifique s'est dans ce cadre développée comme une ressource nationale majeure et un facteur de puissance militaire.

La psychologie est l'une de ces disciplines à avoir grandement bénéficié de l'appareil militaire pour prospérer et se faire valoir, par exemple en sol américain (cf. Capshe, Herman, Napoli, Samelson). Pour résumer, l'on peut dire que les conflits internationaux permirent à la jeune discipline de se voir attribuer une pertinence sociale et d'accéder en une très courte période de temps à d'importantes et nouvelles ressources publiques (matérielles et symboliques). Aux États-Unis, la demande et les usages militaires de la psychologie ont contribué grandement à la diffusion de l'expertise psychologique et des psychotechnologies, ainsi qu'à leur appropriation rapide par les organismes publics.

Notre étude, fondée sur des sources primaires et des archives inédites, indique que ce fut aussi le cas au Canada. Notre communication traitera plus spécifiquement des usages de la psychologie au sein de l'appareil militaire durant la Deuxième guerre mondiale et la Guerre froide, notamment dans les domaines de la gestion des ressources humaines (sélection,

classification et formation du personnel) et du contrôle des facteurs humains. Elle discutera aussi des effets bénéfiques de ces usages militaires sur la montée de l'expertise psychologique au Canada.

**Margocsy, Daniel, “The Court Goes to Pont Neuf: Popular Magic and Courtly Culture in Mid-17th-Century Paris”**

Session I.3A Sun May 29 14:40-15:20 MIDSX 15A

Recent historical studies have greatly developed our understanding of the connections between courtly culture and science in the early modern period. This paper shows how mid-17th-century French courtly culture elevated the status of popular juggling to the rank of aristocratic pastime. *La magie du Pont-Neuf*, an anonymous manuscript preserved at the Houghton Library, narrates how aristocrats from early modern Paris perform sleights of hand at their countryside resorts. The manuscript can probably be dated to the period between 1643 and 1659 and, to my knowledge, has not been discussed in literature previously. Unlike the previous tradition of cheap conjuring pamphlets or erudite books of secrets, *La magie du Pont-Neuf* addresses the emerging French aristocracy of salons in the form of a courtly dialogue. Members of the conversation discuss how the art of juggling, which is usually performed by swindlers on the Pont Neuf, might become an important tool for the survival of *honnêtes hommes* in the courtly culture of simulation and dissimulation. They argue that the manual dexterity present in illusionism requires the same *je-ne-sais-quoi* adroitness that is prescribed for gentlemen in early modern books of conduct from Castiglione to the Chevalier du Méré. The manuscript thus illuminates how civilized courtly culture could broker its relations with the lower strata of sciences and society to its own advantage in the early modern period.

**Martens, Rhonda, “Unity and Confirmation”**

Session I.3B Sun May 29 14:00–14:40 MIDSX 105B

Morrison, Kitcher, Friedman, Forster and Sober, Myrvold, Harper and Smith have addressed the question of whether the unifying power of a theory contributes to its confirmation. Following Harper and Myrvold, I will argue that it does. Morrison, one of my primary targets, holds that one of the reasons unifying power is not linked to confirmation in the way a realist would like is because a theory is either more unifying or less unifying relative to its competitors. Theories are not unifying *per se*. This means that unity is a context-dependent virtue and thus cannot tell us whether our preferred theory is true or how far it is from the true theory. I argue that if we follow up on the work of Harper and Myrvold on unity, their account supplies the resources for claiming that even if Morrison is right about unity being a context-dependent virtue, we can still hold that the theory with greater unifying power is closer to the true one than the less unifying theory (*ceterus paribus*). This, it turns out, is sufficient to ground the link between confirmation and unity.

**McArthur, Daniel, “Contra Cartwright: Structural Realism, Ontological Pluralism and Fundamentalism about Laws”**

Session I.2B Sun May 29 11:10–11:50 MIDSX 105B

In this paper I argue against Nancy Cartwright's claim that we ought to abandon what she calls "fundamentalism" about the laws of nature and adopt instead her "dappled world" hypothesis. According to Cartwright we ought to abandon the notion that fundamental laws (even potentially) apply universally, instead we should consider the law-like statements of science to apply in highly qualified ways within narrow, non-overlapping and ontologically diverse domains, including the laws of fundamental physics. For Cartwright, "laws" are just locally applicable refinements of a more open-ended concept of capacities. By providing a critique of the dappled world approach's central notion of open ended capacities and substituting this concept with an account of properties drawn from recent writing on the subject of structural realism I show that a form of fundamentalism is viable.

**McDermid, Kirk, "Anti-Realism and Empirically Significant Theoretical Entities"**  
Session I.1B Sun May 29 9:40–10:20 MIDSX 105B

A typical argument against realism involves the observation that, when one compares empirical support between theory X taken *with* a commitment to the realism of its theoretical entities, and then *without*, one will inevitably find that X *sans* commitment is the better supported. Theoretical elements, taken realistically, are seen as liabilities: they require, but are incapable of contributing, empirical support. An allegedly crucial element in this argument is the distinction between the observational and the theoretical: if we show that a certain "phenomenon" or entity is theoretical, and therefore "as-if-able", we (goes the argument) show that it plays no positive evidential role. But this argument assumes that theoretical "phenomena" are simply incapable of empirical support – the empirical support generated via appeal to theory is completely exhausted by the observable predictions of the theory.

In this paper, it is this evidence-based distinction (that theoretical "phenomena" are empirically inaccessible, and observable phenomena accessible) that I argue requires revision. The most evidentially-significant predictions generated by theoretical elements cannot be brought to bear on anything other than those theoretical elements. That evidence solely has import for the *theoretical* mechanisms responsible for that *theoretical* phenomenon, inasmuch as those supported relations are *constitutive* of the theoretical entities. I will try to show that this position can defend a very conservative realism of those theoretical elements, entities or phenomena that obtain the requisite empirical support through empirical confirmation of the constraints or dependencies they place on observable entities of the theory.

**McOuat, Gordon, "Collecting Kinds of Things: How the British Museum went from collecting patronage to collecting commodities and what that might say about Natural Kinds"**

Session I.4A Sun May 29 16:50–17:30 MIDSX 15A

In the midst of a raucous nineteenth century debate over the social and scientific place of the premier imperial collections, the British Museum altered its collections policy (at least with respect to natural objects). This paper will track the debates between radicals and conservatives/centres and periphery, over the meaning of natural objects (vs. artefacts) in the Museum, the importance of changing models of political economy attached to natural history, and what this might mean for "natural kinds" and centres of calculation.

**Okruhlik, Kathleen, “Sinking Neurath’s Boat”**

Session II.3A Mon May 30 14:00–14:40 MIDSX 15A

Otto Neurath's recent rehabilitation as a leading figure of the Vienna Circle has come about (at least in part) because he seems to have anticipated so many of the theses we associate with so-called “post-positivist” accounts of science. It is not surprising, therefore, that some authors now treat Neurath as the prefiguration of Kuhn – or Kuhn as the second coming of Neurath. So, for example, in his introduction to Carl Hempel's *Selected Philosophical Essays*, Richard Jeffrey says that Hempel's later essays record a growing sense of Carnap's strategy as a dead end, from which the way out might prove to be Neurath's empirical sociology of science “in something like its Kuhnian avatar.” (p.3) Although other authors don't use such colorful phrases, they do stress rather heavily the similarities between Neurath and Kuhn, often treating *The Structure of Scientific Revolutions* as a continuation of Neurath's program. There is something to be said for such approaches, but in this paper I shall stress instead important dissimilarities between Neurath and Kuhn, dissimilarities too often obscured in the recent literature.

**Padovani, Flavia, “The Principle of Probability and the Principle of Causality in Hans Reichenbach's Works from 1915 to 1925”**

Session II.3A Mon May 30 14:40–15:20 MIDSX 15A

According to autobiographical notes composed by Hans Reichenbach in 1927, the results of his dissertation of 1915 (*Der Begriff der Wahrscheinlichkeit für die mathematische Darstellung der Wirklichkeit*) are: 1) the demonstration that the presupposition of the equations of science can be traced back to a continuity assumption; 2) the demonstration that this assumption is not only presupposed by probability proofs but also by any assertion of physics, the principle of causality being otherwise empty; 3) the attempt to reduce probability to an assertion of certainty; 4) the attempt to show that the probability assumption is a synthetic a priori condition of experience. If he abandons his position on 4) after the publication of his *Relativitätstheorie und Erkenntnis a priori* (1920), at that time he still considers the principle of causality beside (or completed by) the principle of probability. But, during the same year, after an interesting and, up to now, never reported correspondence with Paul Hertz on these topics, he will change in his perspective also on 3), what will have a significant impact on his epistemology. The aim of my paper is to illustrate the role of these two principles and their shift in Reichenbach's works from 1915 to 1925.

**Pearce, Trevor, “Science and the *a priori*: what Positivists, Pragmatists, and Foucauldians have in common”**

Session II.1B Mon May 30 9:00–9:40 MIDSX 105B

There are so many conceptions of the *a priori* that it is hard for the historian of the philosophy of science to make sense of them. A short list will suffice to show that Kant's idea of an *a priori* that opens the space for scientific knowledge has proven quite attractive to philosophers over the years:

- 1) the constitutive *a priori* (Hans Reichenbach)
- 2) the pragmatic *a priori* (C.I. Lewis)

- 3) the operational *a priori* (John Dewey)
- 4) the historical *a priori* (Michel Foucault, Ian Hacking)
- 5) the relativized *a priori* (Michael Friedman, Thomas Kuhn)

Of the three distinct lineages of the *a priori*, only those stemming from logical positivism and Foucault have found articulation in recent philosophy of science.

My paper will compare and contrast the conceptions of the *a priori* native to both logical positivism and Foucault, exploring how these accounts have been deployed by contemporary philosophers of science like Michael Friedman and Ian Hacking. I will argue, fleshing out Arnold Davidson's distinction between internal and external conditions of possibility, that a full picture of scientific development can be given neither by the formal and relativized *a priori* of the logical positivists nor by the informal and historicized *a priori* of Foucault. Finally, I will claim that these two ideas can be fruitfully combined in a revised pragmatic *a priori*. Science, as Dewey argued, is a tool whose space is constrained by the relation between our needs and the world – a relation common to both internal and external conditions of possibility.

**Reynolds, Andrew, “Ernst Haeckel’s Discovery of the *Magosphaera planula*; a Vestige of Metazoan Origins?”**

Session I.4B Sun May 29 16:10–16:50 MIDSX 105B

In September of 1869, while studying sponges off the Norwegian island of Gisoe, Ernst Haeckel (1834-1919) discovered a tiny, flagellated ball-shaped organism swimming about in his samples. Further observation revealed that this curious little creature was a colonial organism with a polymorphic life cycle including unicellular undulipodiated (flagellated) and amoeboid forms in addition to the many-celled colony. Haeckel named the organism the *Magosphaera planula* (the “magic ball”) and upon its existence devised a whole new taxon of Protista, the Catallacta (from Latin for “mediator”), which he proposed represented the bridge between the Protophyta and Protozoa. This living example of development (*Entwicklung*) from a single-celled amoeboid form to a multicellular ball-shaped colony (essentially a permanent blastula, said Haeckel) was of obvious significance for Haeckel’s later gastrea theory (1874) of the origin of the metazoa (multicellular animals) from a unicellular Protist ancestor. In this paper I offer a history of Haeckel’s discovery of the *Magosphaera*, the role it played in the development of his gastrea theory, and its ultimate disappearance from later biological discussion.

**Rocke, Alan J., “The Heuristics of Historical Reconstructions Across the Disciplines”**

Session III.2A Tue May 31 14:40–15:20 MIDSX 15A

In recent years, carefully informed reconstructions of historical objects and processes appear to have become more generally accepted as a legitimate adjunct to more traditional methods of historical investigation. Included in this category are reproductions or “re-stagings” of famous scientific experiments, the reconstruction and demonstration of important technological objects (catapults, triremes, the Wright Flyer), historical cookery, the making and playing of early musical instruments, and even the production of historically-sensitive motion pictures. Historians can learn much that is new and valuable from such endeavors, but there are also challenges, difficulties, and pitfalls regarding which historical restagers must be ever vigilant. Starting from the collaborative experience of reproducing and

performing analyses with an important chemical apparatus of ca. 1830, we will consider the broader opportunities, problems, commonalities, and distinctions connected with historical re-stagings across many different fields.

**Saint-Ours, Alexis de, ‘Herbert Dingle and the Twin Pseudo-Paradox’**

Session II.3B Mon May 30 14:40–15:20 MIDSX 105B

In an April 1956 article, published in *Nature*, Herbert Dingle caused a major controversy about the thought experiment of the Langevin's traveller, by writing that: "[Einstein's 1905] paper contains a most regrettable error, in a statement that a clock moving in a closed curve will be found, on returning to its starting point, to be behind a stationary clock." Dingle's posture as a fierce opponent to special relativity (because of the Langevin's traveller) is particularly interesting because previously he was not only a very gifted physicist but also a very enthusiastic relativist.

The object of this talk is to reconstruct Dingle's various arguments and compare them to those proposed by Bergson in his unfortunate opposition to special relativity (in his book *Duration and Simultaneity*). In the light of this historical perspective, we will endeavour to understand the philosophical roots of Dingle's opposition (in particular his views of mathematics in physics and his conception of intuition).

**Schlipp, Dirk, “Analogies in Science Revisited”**

Session III.1A Tue May 31 11:20–12:00 MIDSX 15A

The importance of the use of models and analogies in science has received considerable attention in philosophy of science ever since the publication of Hesse's "Models and Analogies in Science" (1966). In the wake of the general acceptance of Gentner's "structural theory of analogies" in cognitive science, a characterization of analogies in terms of structure-preserving mappings between two domains seems to have become standard also in the philosophy of science literature.

In this paper I challenge what appears to be the current paradigm in the study of analogies, namely that they are best characterized by structure-preserving mappings between two domains. In my critique I point out certain theoretical limitations of this approach and argue that it does not reflect well the practice of theory construction in science.

In search for an alternative account, I revisit a construal of scientific analogies that is based on a common linguistic description of the analogous domains, and I discuss various advantages of this account of analogies with regard to scientific practice. In particular, I argue that such an understanding of analogies is better suited for illuminating certain aspects of scientific methodology. For example, the formulation of laws can be understood as the explication of conditions that are satisfied by different structures or systems, i.e., as the characterization of the analogies between these systems.

**Sugar, Gabrielle, “‘The Centre Cannot Hold’: The Plural Worlds of Johannes Kepler, Francis Godwin and Cyrano de Bergerac”**

Session I.2A Sun May 29 11:50–12:30 MIDSX 15A

In the 17<sup>th</sup> century, Copernicus's theory that the earth was not the centre of the universe was slowly changing the entire vision of the cosmos. Yet, Copernicus may have had an even greater effect as a result of what he did not explicitly say. Although he still believed that the universe had a centre, that of the sun, he at the same time inadvertently destabilized this notion by presenting a universe with two centres: the earth, a centre around which the moon travels, and the sun, a centre around which all heavenly bodies travel. Thus, Copernicus's heliocentrism actually leads to a loss of a universal centre, generating a new concept of space. This paper argues that this new astronomy has philosophical impact on the literature of the time: Johannes Kepler's *Somnium* (pub.1634), Francis Godwin's *The Man in the Moon* (pub.1638), and Cyrano de Bergerac's *The States and Empires of the Moon* (pub. 1657), each respond to the loss of the centre by imagining the existence of life on another world, the moon. This shift out of the centre leads these three authors to consider the moon as one possible alternative centre from which to view the cosmos, and thus allows them to consider the universe from a new frame of reference. However, as the authors each envision an alien life to emphasize this new perspective, their own ontology is put into question. For all but Cyrano, their absolutist notions return to reinforce their limited perspective of the nature of humanity.

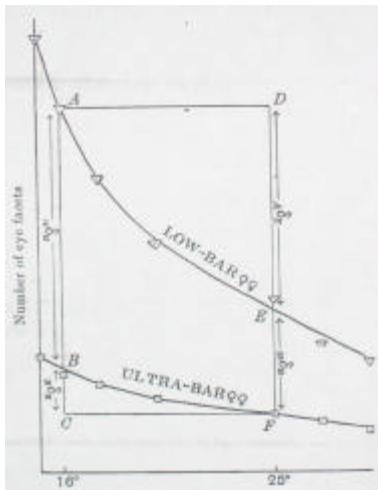
**Tabery, James, “R.A. Fisher, Lancelot Hogben, and the Origin(s) of Genotype-Environment Interaction”**

Session I.4B Sun May 29 16:50–17:30 MIDSX 105B

The concept of genotype-environment interaction (GxE) refers to the fact that different genotypes can react in quite different manners to the same array of environments (see Figure 1). Empirical cases of GxE have significant implications for statistical methodologies that attempt to partition sources of phenotypic variance in a population (such as the analysis of variance), and for the assessment of genotypic group differences.

This historical essay will explicate the origins of the concept of GxE in the work of British biologists R.A. Fisher and Lancelot Hogben in the 1920s and 1930s. "Origins" and not "*the origin*" will be explicated because, it will be argued, Fisher and Hogben actually came to consider the concept of GxE by quite distinct routes, which led them, in turn, to quite distinct interpretations of the concept: Fisher introduced the *biometric* sense of genotype-environment interaction ( $GxE_B$ ), while Hogben developed the *reaction norm* sense of genotype-environment interaction ( $GxE_{RN}$ ). With these two senses of GxE explicated, I will draw on previously unpublished correspondence to detail the debate that emerged between Fisher and Hogben concerning how significant the concept of GxE actually was for the eugenic studies of population variance and group differences.

I will also claim that this explication of the distinction and relationship between  $GxE_B$  and  $GxE_{RN}$  can facilitate a clearer understanding of more recent debates concerning the significance of GxE for studies of population variance and group differences, such as in the "IQ controversy" of the 1970s and the "*Bell Curve* wars" of the 1990s.



**Figure 1.** Empirical example of GxE for two genotypic strains of *Drosophila*. Note the different reactions of LOW-BAR and ULTRA-BAR to the array of temperatures in which the strains developed (From Hogben 1933).

**Tsou, Jonathan Y., “Putnam’s View on Analyticity and Scientific Change: Its Historical and Contemporary Interest”**

Session II.1B Mon May 30 9:40–10:20 MIDSX 105B

In a number of papers in the 1960s and 1970s (e.g., “The Analytic and the Synthetic”, “It Ain’t Necessarily So”, “Two Dogmas Revisited”), Hilary Putnam articulated a way of drawing an analytic-synthetic distinction that was motivated to address the problem of scientific change. Putnam maintains that in the context of any scientific theory, there are certain statements (e.g.,  $f = ma$ ,  $e = \frac{1}{2} mv^2$ , and the laws of Euclidean geometry) that function as “analytic definitions” (or “framework principles”) insofar as they are *necessary relative* to that body of knowledge. In this paper, I examine Putnam’s position on analyticity (and apriority) in the context of the Carnap-Quine debates on analyticity. After situating Putnam’s view of analyticity in relation to Quine and Carnap’s views, I explore the relation between Putnam’s views on analyticity and scientific change. Putnam’s perspective on these issues is of contemporary interest for its connection with recent discussions on apriority in scientific knowledge (e.g., Michael Friedman and David Stump) and structural realism (e.g., John Worrall, James Ladyman, and Stathis Psillos). I conclude by attempting to locate Putnam’s perspective in relation to these contemporary perspectives.

**Usselman, Mel, “Science and Surprise in Reconstruction Experiments”**

Session III.2A Tue May 31 14:00–14:40 MIDSX 15A

Reconstruction of important experiments in the history of science can, in appropriate context, yield insights into the accuracy of the experiments, their replicability, persuasiveness and scientific and historical value. Occasionally, reconstruction experiments can even reveal characteristics of the original experimenter that are not in accord with prevailing historical judgement. But to be useful assessment tools, reconstruction experiments need to fulfill demanding scientific and historical criteria - they must be technically accurate, contextually appropriate, scientifically informed and untainted by presentism. These criteria become more difficult to fulfill as the experiment lies further in the past. In this presentation I will

summarize the scientific and experimental challenges that accompanied reconstructions of Wollaston's platinum purification process, Chenevix's synthesis of artificial palladium, Thomson's (erroneous) discovery of multiple combining proportions in strontium salts, Dalton's nitrous air experiments and Liebig's combustion analysis of organic compounds. Each reconstruction project yielded some historical information that was at odds with conventional historical and scientific wisdom. I will argue that there are many historical questions that are best answered by experimental reconstruction, and some that cannot be answered any other way.

**Vanderburgh, William, “The Forgotten Tradition of Non-Mathematical Probability, and Why HPS Needs to Remember”**

Session III.1B Tue May 31 11:20–12:00 MIDSX 110

The origin of mathematical probability theory is normally attributed to Pascal. The early developers of mathematical probability theory focused on games of chance, but the framework was soon extended to other kinds of situations, e.g., deciding whether or not the risk associated with being inoculated against small pox was worth the benefit, how to structure annuities, and even the probability of scientific hypotheses in light of the available evidence. Today mathematical probability theory is so dominant that historians and philosophers have largely forgotten the long and noble tradition of probable reasoning that existed before Pascal. Some commentators (e.g., Hacking) go so far as to claim that there was little notion of evidential probability before Pascal. But in fact the pre-Pascalian tradition of probability goes back to ancient Roman law, and perhaps farther. This tradition guided the development of evidential reasoning in the law and all other fields from the middle ages through the early modern period, and it was entirely non-mathematical.

Historians and philosophers of science need to be more aware of this ancient tradition of probable reasoning, for two reasons. First, because philosophers and scientists before and even after Pascal had the ancient tradition in mind when discussing the probability of scientific hypotheses, reading them through the lens of mathematical probability seriously distorts what they meant. Second, by neglecting the ancient tradition of probability, we lose an opportunity to think about evidence in a different and potentially fruitful way.

**Yoshida, Kei, “Rationality and relativism: The case of Sahlins vs. Obeyesekere”**

Session III.1A Tue May 31 11:20–12:00 MIDSX 15A

The purpose of this presentation is to reconsider the problem of rationality and its relation to other cultures by scrutinizing the recent controversy between two anthropologists: Marshall Sahlins and Gananath Obeyesekere.

In his works, Sahlins claims that eighteenth century Hawaiians regarded Captain James Cook as their god Lono. This shows, according to Sahlins, that people in different cultures have different conceptual schemes and criteria of rationality. But Obeyesekere criticizes Sahlins as trapped in European mythmaking and treating the Hawaiians as irrational. In Obeyesekere's view, human beings share a common practical rationality based in biological nature. Obeyesekere tries to refute Sahlins's relativist view and claim that the Hawaiians regarded Cook as a chief. Sahlins counter-attacks Obeyesekere for his acceptance of the Western view and for treating the Hawaiians as modern Europeans.

This controversy invites us to reconsider the problem of rationality and its relation to other cultures, which was originally triggered by the so-called “rationality debate.” Following

Jarvie and Agassi, I shall argue that, although we need to refute Sahlins's relativist view, we do not have to accept Obeyesekere's biological view. In my view, both of them ignore the possibility of degrees of rationality. If we admit that there are degrees of rationality, then both Westerners and indigenes can be more rational in some cases and less rational in other cases. Hence we do not need to draw a line between Westerners and indigenes, as Sahlins does. But, at the same time, we do not have to appeal to a common biological nature, as Obeyesekere does.