HALIFAX

JUNE 21-24

HOPOS 2012

THE INTERNATIONAL SOCIETY FOR THE HISTORY OF PHILOSOPHY OF SCIENCE

PROGRAMME

Pre-Conference: Wednesday, 20 June 2012

Reception 7:00-9:00pm Fourth Floor, The Sobey Building, **Saint Mary's University**

Day 1: Thursday, June 21

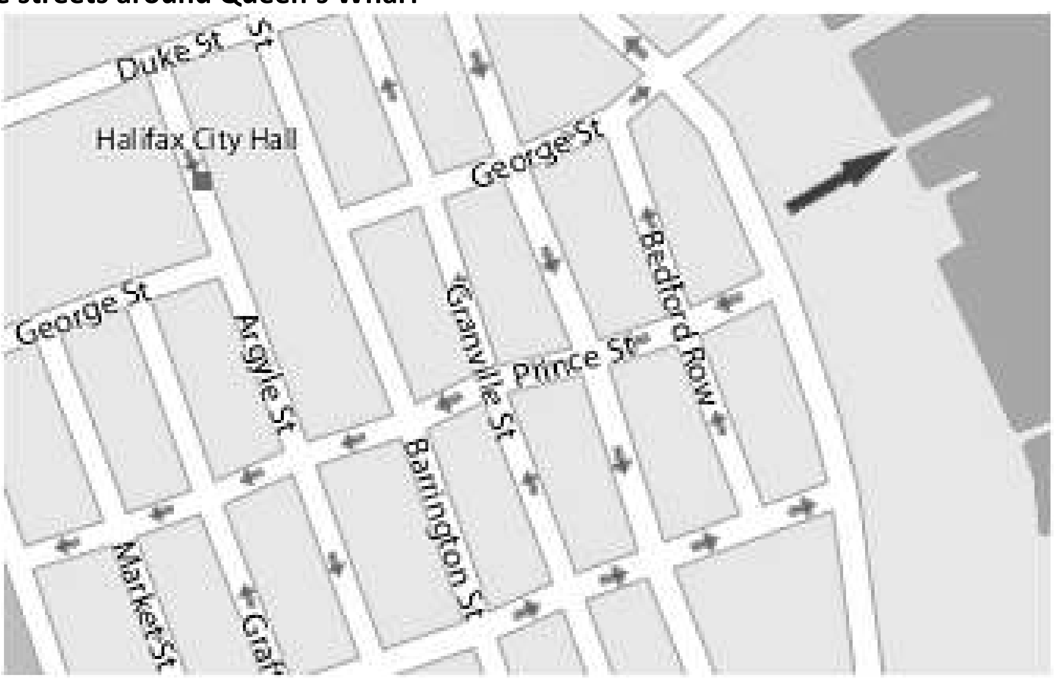
Parallel Session I 8:30-10:30am			
I.1 KTS Lecture Hall ***Perspectives on Carnap and Kuhn*** **Chair: Gary Hardcastle** **Jonathan Y. Tsou:** *Reconsidering the Carnap-Kuhn connection* **Thomas Meier:** *From Carnap via Kuhn to Stegmüller: The development of structuralist philosophy of science*	I.2 Archibald Room ***19th- and 20th-century sciences/philosophies of persons and bodies*** **Chair: Morgan Tunzelman** **Morgan Tunzelmann:** *Taxonomic depths and the haptic in early nineteenth century medical theory* **Philip Honenberger:** *Re-evaluating classical philosophical anthropology (1927-1940)* **Brandon Konoval:** *'The Philosopher of Power'?: Nietzsche, Foucault and the genealogy of sexuality*	I.3 Scotiabank Room ***Sir Francis Bacon, Lord Verulam*** **Chair: Lisa Mullins** **Daniel Schwartz:** *Francis Bacon on the unity of discovery and justification* **Ian Stewart:** *Francis Bacon and the history of the philosophy of 'observation'* **Karen Zwier:** *Experiment as test of causal claims: A history*	I.4 Haliburton Room ***Scientific Methods and Explanation*** **Chair: Eric Palmer** **David Marshall Miller:** *Pluribus ergo existentibus centris: Explanations, descriptions, and Copernicus* **Petter Sandstad:** *Philodemus on the joint method of agreement and difference*
Coffee Break 10:30-10:45 am 1st floor – New Academic Building			
Parallel Session II 10:45am-12:45 pm			
II.1 KTS Lecture Hall ***Logical empiricists in context*** **Chair: Thomas Oberdan** **Daniel Kuby:** *A 'bottom-up epistemology': Viktor Kraft on discovery, justification and the tasks of philosophy of science* **Nikolay Milkov:** *On Walter Dubislav* **Alan Richardson:** *Making waves: Hans Reichenbach, radio philosopher*	II.2 Archibald Room ***Philosophical theories in the age of Weltanschauungen*** **Chair: Kristian Camilleri** **Tony Mills:** *Meyerson's épistémologie* **Kristian Camilleri:** *The physicist as philosopher: Philosophical ambitions in cultural context*	II.3 Scotiabank Room ***Thomas Hobbes*** **Chair: Ian Stewart** **Marcus P. Adams:** *Maker's knowledge and underdetermination in Hobbesian natural philosophy* **Geoffrey Gorham:** *Hobbes on motion, time and conatus: A realist account* **Edward Slowik:** *Hobbes and the "phantasm" of space*	II.4 Haliburton Room ***Wallis and Kant*** **Chair: Tad Schmaltz** **Adam Richter:** *The Trinity and the cube: Nescience in the epistemology of John Wallis* **Michael J. Olson:** *Metaphysics and science in Kant's Copernican revolution*
Lunch 12:45-2:15pm Prince Hall			

Day 1: Thursday, June 21st (continued)

HOPOS Journal Editorial Board Meeting 12:45-2:15pm Senior Common Room			
Parallel Session III 2:15-5:15pm			
III.1 KTS Lecture Hall ***Symposium: Dedekind, mathematical methodology, and the notion of function***	III.2 Archibald Room ***Symposium: What the philosophy of biology was: Neglected figures in early 20th-century philosophical and theoretical biology***	III.3 Scotiabank Room ***Symposium: Normative naturalism in Comte's positive philosophy***	III.4 Haliburton Room ***Aristotle***
Chair: Georg Schiemer **Erich Reck:** *Dedekind's methodology and the infinite in mathematics* **Emmylou Haffner:** *Generality of definition and arithmetical methodology inDedekind* **Ansten Klev:** *Mappings in Dedekind* **Dirk Schlimm:** *The early development of Dedekind's notion of mapping*	**Chair: Trevor Pearce** **Erik L. Peterson:** *Joseph Neeedham's new and improved organicism in the midst of a growing reductionist consensus, 1925-1938* **Jon Umerez:** *Paul Weiss and organicist roots of hierarchical thinking* **Daniel J. Nicholson:** *The enduring relevance of Ludwig von Bertalanffy's organicist conception of the organism* **Richard Gawne:** *J. H. Woodger, logical empiricism, and the unity of science*	**Chair: Daniela Barberis** **Warren Schmaus:** *Comte's revolution in epistemology* **Laurent Clauzade:** *In defense of 'historical epistemology': Comte and Whewell on metaphysics* **Michel Bourdeau:** *Two conflicting ideas upon the nature and the goals of man's action upon social phenomena* **Vincent Guillin:** *The sociological rule: Positive polity and its epistemological foundations*	**Chair: Helen Hattab** **Phil Corkum:** *Aristotle on quantification* **Richard Dewitt:** *Does Aristotle say an object that weighs twice as much falls twice as fast? (Hint: No.)* **Janine Gühler:** *Aristotle's way of abstracting* **Mark Sentesy:** *The compatibility of dunamis and energeia*
Coffee Break 5:15-5:30pm 1st floor: New Academic Building			
Keynote I: Heinrich von Staden **"Experimentation in ancient science? Concepts, theory and practice"** 5:30-7:00pm Alumni Hall Chair: Eric Schliesser			
Cruise and Ceilidh on the Tall Ship *Silva* 8:00-10:30pm **Boarding at 8:00, departing 8:30 sharp** Queen's Wharf, Halifax Harbour (see map) (Optional: tickets required--cash bar)			

Map from University of King's College to Queen's Wharf, where the Silva is moored

Map of the streets around Queen's Wharf



Dress warmly and be sure to arrive in good time (comfortably *before* 8:30 pm).

No refunds if you miss the boat!

Day 2: Friday, June 22nd

Parallel Session IV 8:30-10:30am			
IV.1 KTS Lecture Hall ***Symposium: Robert Merton and the philosophy of science*** **Chair: Saul Fischer** **Saul Fisher:** *Merton and Nagel on functional explanation* **Stephen Turner:** *Robert Merton and Dorothy Emmet: Deflated functionalism and structuralism* **Gary Hardcastle:** *Merton, ethos, and sentiment*	IV.2 Archibald Room ***Nineteenth-century German scientific epistemology*** **Chair: Greg Moynihan** **Liesbet de Kock:** *Im Anfang war die Tat: Helmholtz and the problem of externality in perception* **Scott Edgar:** *Continuity and the constitution of individuals in Hermann Cohen's* <u>*Prinzip der Infinitesimal-Methode*</u> **Christian Damböck:** *Critical remarks on neo-Kantian interpretations of Carnap and Kuhn*	IV.3 Scotiabank Room ***Philosophy of Experiment*** **Chair: Alex Klein** **Peter Anstey:** *D'Alembert, the 'Preliminary Discourse', and the experimental philosophy* **Madalina Giurgea:** *On the creative role of experimentation in Descartes' study of colours*	IV.4 Haliburton Room ***Gottfried Wilhelm Leibniz*** **Chair: Ed Slowik** **Erik C. Banks:** *The problem of extension in the philosophy of science (1700-1860)* **Douglas Bertrand Marshall**: *Leibniz: Geometry, physics, and idealism* **Ken Pearce:** *Leibniz on phenomenalism, mechanism, and the great chain of being*

Coffee Break 1st floor New Academic Building 10:30-10:45am

Parallel Session V 10:45am-12:45pm				
V.1 KTS Lecture Hall ***Symposium: Transforming methods: Late Aristotelian roots of modern approaches to medicine, natural philosophy and civil science*** **Chair: Peter Anstey** **Peter Distelzweig:** *William Harvey's Aristotelian experimentalism* **Nathan Smith:** *Simple natures and scientific explanation in Bacon and Descartes* **Helen Hattab:** *Method and mathematical order from Zabarella to Hobbes*	V.2 Archibald Rm. ***Twentieth-century neo-Kantianism and the exact sciences*** **Chair: Scott Edgar** **Thomas Oberdan:** *Cassirer's response to Russell's* Principles of Mathematics **Nabeel Hamid:** *The 'Duhem thesis' in Ernst Cassirer's philosophy of science* **Dan McArthur:** *Exploring neo-Kantianism in Bohr and logical empiricism*	V.3 Scotiabank Rm. ***Historical methods in HPS*** **Chair: Eve Roberts** **Xavi Lanao *et al.*:** *The evolution of case studies in philosophy of science: A path towards integrated HPS?* **Aaron D. Cobb**: *Exploratory experimentation and securing understanding* **Philipp Haueis:** *Logical and experimental underdetermination*	*V.4 Haliburton Rm.* ***Newton and Huyghens*** **Chair: Rose-Mary Sargent** **Ari Belenkiy:** *The master at the Royal Mint: How much money did Newton save Britain?* **Alistair Isaac:** *Newtonian answers to Baconian questions: 'Proof by experiment' in Newton's optical research* **Maarten van Dyck:** *Mechanics and natural philosophy in the work of Christiaan Huygens*	V.5 Frazee Rm. ***Perspectives on post-positivism*** **Chair: Lisa Gannett** **Peter Olen:** *Pure pragmatics and logical empiricism: Contextualizing Wilfrid Sellars's early publications* **Vasso Kindi:** *The influence of Wittgen-stein's philosophy on historical philosophy of science* **Matteo Collodel:** *Between logic and history: The development of Feyerabend's idea of incommensurability*

Day 2: Friday, June 22nd continued

Lunch and Open Business Meeting 12:45-2:15pm
Prince Hall

Parallel Session VI 2:15-5:15pm

VI.1 KTS Lecture Hall	VI.2 Archibald Room	VI.3 Scotiabank Room	VI.4 Haliburton Room
Symposium: Descartes' Metaphysical Physics: Twenty years young	***Symposium: Poincaré in perspective: Conventionalism one hundred years later***	***History of philosophy of biology***	***Historical methods in philosophy of science and mathematics***
Chair: Gideon Manning **Dennis Des Chene:** *Descartes' revision of the relations of metaphysics to natural philosophy* **Dana Jalobeanu:** *Descartes' mathematical physics and Descartes' Metaphysical Physics* **Tad M. Schmaltz:** *The mechanical philosophy and occasionalism: Reflections on Descartes' Metaphysical Physics* **Daniel Garber:** *Response*	**Chair: Janet Folina** **Robert DiSalle:** *Poincaré on the construction of space-time* **Janet Folina:** *Poincaré's conventions: Between intuition, empiricism, and stipulation* **Maria de Paz:** *The third way in epistemology: A recharacterization of Poincaré's conventionalism* **David J. Stump**: *From Poincaré to pragmatic a priori: Lenzen and Pap on the conventionality of principles*	**Chair: Andrew Reynolds** **Marij van Strien:** *Vital instability: How Maxwell, Kelvin and others created a domain for life through physics* **Charles H. Pence:** *The early history of chance in evolution: Causal and statistical in the 1890s* **Olivier Sartenaer:** *Neither metaphysical dichotomy nor pure identity: Clarifying the emergentist's creed* **Jan Baedke: "***The epigenetic landscape in the course of time": A transdisciplinary survey of Conrad Hal Waddington's landscape images*	**Chair: Thomas Staley** **Jacobo Asse Davan:** *Incorporating history into the philosophy of mathematics* **Mark Dietrich Tschaepe:** *John Dewey's conception of scientific explanation: Moving philosophers of science past the realism-antirealism debate* **Matthias Neuber:** *Is logical empiricism consistent with scientific realism?* **Charles T. Wolfe:** *Materialism before physicalism: Cultured brains and reductive materialism from Diderot to J.J.C. Smart*

Coffee Break 5:15-5:30pm
1st floor New Academic Building

Keynote II: Ian Hacking
"On rhetoric, and especially paradigms"
5:30-7:00pm Alumni Hall
Chair: Warren Schmaus

"Science Inaction"
Theatre Presentation 9:00 – 9:45pm
8:30 (reception, cash bar) The Pit
Free for Conference Delegates

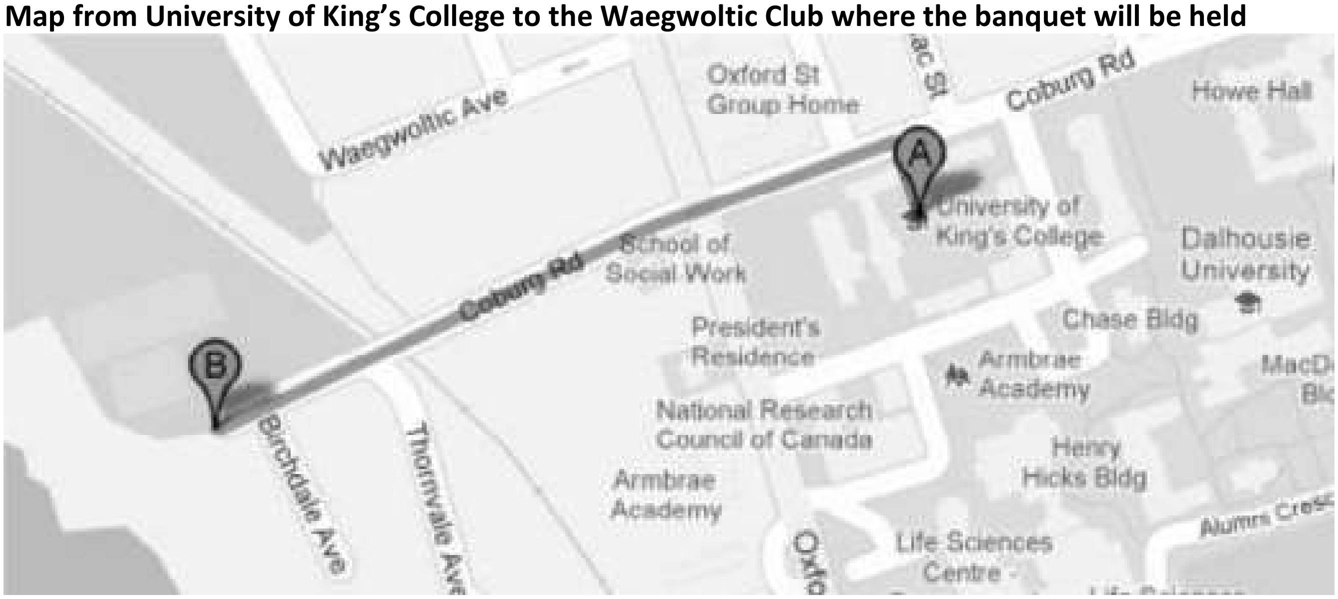
Day 3: Saturday, June 23rd

Parallel Session VII 8:30-10:30am			
VII.1 KTS Lecture Hall ***Symposium: Newton's place in the rationalist tradition*** **Chair: Eric Schliesser** **Mary Domski:** *Newton and Proclus on the geometry of absolute space* **Eric Schliesser:** *Spinoza and the Newtonians on motion and matter (and God, of course)* **Janet Folina:** *Hamilton's Newtonian defense of truth in algebra*	VII.2 Archibald Room ***Carnap, Carnap, Carnap*** **Chair: Jonathan Tsou** **Christopher F. French:** *Reconstructing rational reconstructions in Carnap's Aufbau* **Georg Schiemer:** *Carnap's mathematical structuralism* **Matteo Collodel:** *The Neurath-Carnap disputes: Carnap's final attempt at their dissolution*	VII.3 Scotiabank Room ***Anglo-American HOPOS*** **Chair: David Stump** **Trevor Pearce:** *Evolution in the Metaphysical Club: Wright and Fiske on Darwin and Spencer* **Thomas W. Staley:** *The 'Scratch Eight', Aristotelians, Metaphysicals, Mind, and more: An exploration of late Victorian philosophical institutions and their context(s)* **Alexander Klein:** *Russell's external world program and the psychology of spatial perception: The significance of James*	VII.4 Haliburton Room **Varia** **Chair: Brandon Konoval** **Eric Palmer:** *'A wise disposition of nature': Finding purpose in early modern explanation* **John Barresi:** *British psychology as an empirical science in the eighteenth century: Pneumatological lectures of Grove, Doddridge, Reid and Belsham* **Ina Goy:** *Kant on formative power*
Coffee Break 10:30-10:45am 1st floor New Academic Building			
Parallel Session VIII 10:45am-12:45pm			
VIII.1 KTS Lecture Hall ***Symposium: Kant, Leibniz, and the foundations of the exact sciences*** **Chair: Clinton Tolley** **Clinton Tolley:** *Kant, Leibniz, and the metaphysical foundations of logic* **Jeremy Heis:** *Leibniz versus Kant on Euclid's axiom of parallels* **Marius Stan:** *Leibniz and Kant on the relativity of motion and the law of inertia*	VIII.2 Archibald Room ***Fleck, Neurath, and social philosophies of science*** **Chair: Don Howard** **Artur Koterski:** *Fleck's anti-relativism in his polemics with Bilikiewicz* **Katherine Arens:** *The science debate comes to the US:* The International Encyclopedia of Unified Science **Elisabeth Nemeth:** *Scientific knowledge, democratic decision-making and philosophy of science: Harry Collins' 'normative theory of expertise' in historical perspective*	VIII.3 Scotiabank Room ***History of philosophy of mathematics*** **Chair: Dirk Schlimm** **Jean-Paul Cauvin:** *Leon Brunschvicg's critical idealism and the epistemology of mathematical reason* **Yvon Gauthier:** *Finitism from Kronecker to Gödel via Hilbert* **Oran Magal:** *The logical in mathematics and the mathematical in logic*	VIII.4 Haliburton Room ***Descartes*** **Chair: Geoff Gorham** **Barnaby Hutchins:** *The non-mechanical foundation of Descartes' mechanical physiology* **Bret J. Saunders:** *Descartes's scientific poetics: Analysis, analogy and rhetoric in* Optics I **Monica Solomon:** *Descartes and Newton: The influence of mathematics in conceptualizing motion*

Day 3: Saturday, June 23rd continued

Lunch 12:45-2:15pm Prince Hall
Special Plenary Symposium: Reflections on Michael Friedman's Kant and the Exact Sciences 2:15-5:15pm Alumni Hall Chair: Mary Domski Emily Carson: "Kant, quantity, and figurative synthesis" Marius Stan" "Physics in Kant and the Exact Sciences: Twenty years later" Robert DiSalle: "Transcendental philosophy from a Newtonian perspective" Michael Friedman: "Reconsidering Kant and the Exact Sciences"
Coffee Break 5:15-5:30pm 1st floor, New Academic Building
Keynote III Kathleen Okruhlik **"'Bridled irrationality': Historical antecedents of Bas van Fraassen's epistemic voluntarism"** 5:30-7:00pm Alumni Hall Chair: Alan Richardson
Banquet 7:30-10:00pm Waegwoltic Club (see map) (6549 Coburg Road; Phone (902) 429-2822)

Map from University of King's College to the Waegwoltic Club where the banquet will be held



Day 4: Sunday, June 24th

Parallel Session IX 9:30am-12:30pm			
IX.1 KTS Lecture Hall ***Symposium: Life before the man-machine: Conceptualizing life and mechanism in early modern natural philosophy***	IX.2 Archibald Room ***Fin-de-Siècle European philosophies of science***	IX.3 Scotiabank Room ***Varia***	IX.4 Haliburton Room ***A11: History of philosophy of mathematics***
Chair: Charles T. Wolfe **Peter Distelzweig:** *Function, use and teleology in Descartes and early modern medicine* **Barnaby Hutchins:** *Descartes and the dissolution of life* **Dagmar Provvijn:** *Harvey's mechanisms* **Charles T. Wolfe:** *Automata, man-machines and the challenge of life*	**Chair: Mélanie Frappier** **Daniela Barberis:** *History and contingency in the work of Émile Boutroux* **Klodian Coko:** *Epistemology of a believing historian: Making sense of Duhem's anti-atomism* **Milena Ivanova:** *Poincaré's acceptance of the atom: against fundamentalism* **Pablo Ruiz de Olano:** *Blas Cabrera's defense of relativity: Duhem's role in the debate on the foundations of relativity*	**Chair: Gordon McOuat** **Matteo Vagelli:** *Some remarks on the role of conceptual flaws, errors and mistakes in the historiography of science* **Alessandro Zir:** *The Indians who came from Ophyr: Prophecy and natural history in early-modern Brazil* **Silvia Di Marco:** *From Hunter's Gravid Uterus to the Visible Human Project: Have 'interpreted images' really displaced 'metaphysical images'?* **Jason Jordan:** *Causal necessity and the agreement between the ancients and the moderns*	**Chair: Daniel D. Campos** **Mario Santos-Sousa:** *Berkeley on the mind-dependence of numbers* **Barbara Sattler:** *The labours of Zeno: A supertask?* **Tobias Schöttler:** *New adventures in old mathematics: The shift from causes to relations at the root of early modernity* **Daniel G. Campos:** *The role of analogy in mathematical reasoning: The case of* *Archimedes' De Circuli Dimensione* *and Bernoulli's* *Ars Conjectandi*

Abstracts for parallel sessions

Thursday, 21 June

Parallel Session I

Session I.1 Perspectives on Carnap and Kuhn

Jonathan Y. Tsou, Iowa State University
"Reconsidering the Carnap-Kuhn connection"

Rudolf Carnap and Thomas Kuhn are undoubtedly two of the most influential twentieth century philosophers of science. According to the 'received view,' Carnap's and Kuhn's views represent diametrically opposed approaches to philosophy of science and Kuhn's (1962/ 1996) *Structure of Scientific Revolutions* is one of the main philosophical works—along with Quine's (1951/1980) "Two Dogmas of Empiricism"—that (rightfully) contributed to demise of logical empiricism in the 1960s and 1970s. While the received view has been commonplace among post-positivist philosophers of science (e.g., see Suppe, 1974/1977; Giere 1988, ch. 2; McGuire 1992), this narrative about the history of twentieth century philosophy of science has been increasingly called into question in the past two decades.

Some historians of philosophy of science (Reisch 1991; Earman 1993; Irzik and Grünberg 1995; Friedman 2001, 2003; Irzik 2002, 2003; Richardson 2007) have argued that the received view on Carnap and Kuhn is mistaken, suggesting that there is a close affinity between the philosophical views of Carnap and Kuhn. The basis for this revised understanding stems from some fundamental similarities between the philosophical systems of Carnap and Kuhn, especially on issues concerning the theory ladenness of observation, incommensurability, the pragmatic nature of theory choice, and scientific change (cf. Uebel 2011, 131). The upshot of this revisionist picture is that the "two styles of doing philosophy of science epitomized by Carnap and Kuhn should be seen as complementary rather than mutually exclusive" (Irzik and Grünberg 1995, 304-305).

In this paper, I offer some reasons for resisting the revisionist conclusion that Carnap's and Kuhn's philosophical views are closely aligned. While there are similarities between Carnap's and Kuhn's philosophical systems, I argue that a consideration of the broader philosophical projects of Carnap and Kuhn renders these similarities superficial in comparison to their fundamental differences. On a general level, revisionist analyses fail to sufficiently acknowledge that Carnap's linguistic frameworks are logical reconstructions *intended to clarify answerable (i.e., meaningful) and unanswerable (i.e., meaningless) questions via logical analysis*, whereas Kuhn's theory of scientific revolutions is motivated *to provide a naturalistic description of scientific change*. This difference reflects two vastly different styles of doing philosophy of science (i.e., logical versus historical reconstruction). On a more specific level, I argue that the 'incommensurability' that can justifiably be attributed to Carnap is far less robust than Kuhnian incommensurability and that Kuhn's insistence that proponents of competing paradigms (or 'lexicons') cannot fully communicate with one another is entirely antithetical to the motivations of Carnap's logic of science project (viz., to resolve philosophical and scientific debates). From this perspective, I suggest that the philosophical methodologies of Carnap and Kuhn are correctly regarded as two contrasting philosophical styles that mark a significant division between positivist and post-positivist philosophy of science.

Thomas Meier, Ludwig-Maximilians-Universität
"From Carnap via Kuhn to Stegmüller: The development of structuralist philosophy of science"

The aim of this work is to provide a historical reconstruction of the development of the structuralist view of scientific theories. This meta-theoretical view understands scientific theories as *model-theoretic* entities. One of the principal aims of the structuralist view is to explain theory change in a precise formal sense.

I will argue that the issue of theoretical change and the development of scientific theories through time has become a central motivation for the development of the structuralist view of scientific theories. In this work I will show how the development of structuralism is strongly motivated by Kuhn´s conception of radical theory change. I will argue that one of the principal aims of Stegmüller and Sneed for the development of the structuralist conception of scientific theories was to provide a clear exposure of Kuhn´s threatening proposals of radical, "irrational" theory change.

At the beginning, structuralism took its formal tools from Suppes' method of defining set-theoretic predicates in order to axiomatize empirical theories. Originally, this method aimed to provide a clearer mathematical insight into the logical structure of scientific theories than the statement-view was providing. In the latter, theories are just understood as sets of sentences closed under deduction. This conception, as adherents of the structuralist view argue, cannot provide an exhaustive insight into the logical structure of empirical theories. For the precise explanation of theory change in science, the statement-view turned out to be unable to argue against Kuhn's attacks on a harmonious view of theory change. In response to this, Stegmüller and Sneed started developing the structuralist approach.

In this work I will show how the structuralist view also takes its motivations by Carnap's work in the philosophy of science. I will recur to Carnap's *Aufbau*. I aim to show that Carnap's idea of structural descriptions of all our knowledge exposed in the *Aufbau* can be seen analogous with the structuralist view of representing our knowledge about scientific theories. Carnap claims (mainly in §16 of the *Aufbau*) that only structural descriptions of our knowledge guarantee the objectivity and intersubjectivity of our claims. I will state that this can be seen analogous with the contemporary structuralist view of scientific theories, which provides to give us complete formal descriptions of the logical structure of our empirical theories in order to explain their relation with our experience. My interpretation of the development of structuralist philosophy of science also aims to partially integrate work of the historical reconstruction that has already been done by Michael Friedman (1999). I will show that the structuralist program can be understood to be continuous with Carnap's and also with some of Kuhn's original ideas about scientific theories.

Session I.2 19th-and 20th-century sciences/philosophies of persons and bodies

Morgan Tunzelmann, University of Waterloo
"Taxonomic depths and the haptic in early nineteenth-century medical theory"

In his medical encyclopedia entitled *A series of engravings...which are intended to illustrate the morbid anatomy of some of the most important parts of the human body* (1803), the physician Matthew Baillie includes a number of engraved plates that depict the "chief diseased appearances" of several abdominal and sexual organs, as well as the brain, noting that the diseases of these important parts of the human body will be "distinctly impressed upon the mind by figures...exhibited to the eye" (5). Despite the sensory immediacy of the figures, Baillie nonetheless laments the impossibility of representing visually the "finer changes of structure" that indicate the underlying nature and effects of disease (4). Baillie supplements this shortcoming not only by intensifying the degree of detail in his anatomical illustrations, but also by relying on the verisimilitude of his figures to support the taxonomy of diseases that he develops and uses to organize the images. In these ways, Baillie resembles his contemporary Robert Willan, whose encyclopedia *On cutaneous diseases* (1808) includes coloured plates that assist in developing a classification system for lesions: "cutaneous" is by definition superficial, but "diseases" implies a deeper, abstract system. The visual delineation of cutaneous diseases thus relieves the need to probe or touch this abject rupture of the skin's surface.

Using Baillie's and Willan's texts as representatives of early nineteenth-century medical theory, my paper will discuss how the aesthetic "surfaces" of the illustrations and the structural "depths" of their taxonomies correspond with the visual and haptic (or tactile) senses. Creating a taxonomy to substitute for a sense of "depth" in the representation of a diseased body ultimately removes the need for investigative touch in medicine, I will argue, precisely because the tendency of touch to discern specific textures and details (similar to the Kantian condition of individual aesthetic appreciation) may in fact cause a sublime confrontation with the unknown recesses of the body. The medical illustration thus not only mediates between vision and touch, subsuming the latter beneath the former, but also gives rise to a theory of medicine that exalts anatomical dissection (the process of turning the body inside out through the probing of instruments) over mindful, empathetic palpation.

Philip Honenberger, Temple University
"Re-evaluating classical philosophical anthropology (1927-1940)"

In Weimar-era German philosophy, the topic of *philosophische Anthropologie* was in the air, occupying the speeches and texts of such philosophical notables as Martin Heidegger and Ernst Cassirer. But Heidegger and Cassirer's responses to philosophical anthropology have tended to push down contemporary estimates of its continuing value.

In *Sein und Zeit* (1927) and *Kant und das Problem der Metaphysik* (1929), Heidegger rejected philosophical anthropology as a misguided organization of the problems of philosophy around an anthropological center, a rejection followed by his advocacy of a not specifically-anthropological "fundamental ontology." Cassirer favorably discussed "philosophical anthropology" in an unpublished essay (1928) intended for a fourth volume of the *Philosophie des Symbolischen Formen* (vols. 1-3, 1923-1929), and temporarily incorporated the name into the many titles he employed for his later philosophical project, briefly subtitling the *Essay on Man* (1944) a "philosophical anthropology." (Before publication he changed the subtitle to "An Introduction to a Philosophy of Human Culture.")

In this paper I offer an alternative estimate of Weimar-era philosophical anthropology's importance. Regarding the relevance of broadly biological sciences (for instance, as represented by Darwin, Driesch, Uexküll, and Köhler) to the interpretation and understanding of human beings, the methodological and substantive commitments of (what I call) the "classical philosophical anthropologists" Max Scheler, Helmuth Plessner, and Arnold Gehlen offer marked advantages over Heideggerian and Cassirean approaches. While Heidegger was correct that ontological concerns are not solely anthropological ones, he himself proposed, in *Sein und Zeit* (1927), human beings under his own peculiar description – that is, as *Dasein* – as the central and initial topic of fundamental-ontological inquiry, while problematically stripping this topic of its empirical content. And while Cassirer's later philosophy was promisingly empirically- and historically-attuned and expansive in its scope, incorporating scientific knowledge in physics and biology – both as defining a mode of existence of a "knowing" subject (the scientist) and a "known" object (the domain of concern of the science itself) – alongside other modes of subject-object relation (such as those characteristic of fine art, myth, language, history, and ethics and state politics), Cassirer's own construal of the relation between the "natural-scientific" and the "cultural" sciences or symbolic forms remained ambiguous and unresolved: sometimes dualistic in a manner reminiscent of Kant, sometimes relativistic in a manner closer to Hegel or Lévi-Strauss.

Classical philosophical anthropology's founding insight elides both the Heideggerian and the Cassirean errors just mentioned. By insisting on the relevance of empirical sciences to philosophical reflection, and by (relatedly) construing the human being as an animal, and thus a sometime object of biological science, that has nonetheless been transformed by a special kind of relationship to its environment – namely, one that is mediated by historically contingent artifactual transformations of the environment itself in the form of language, material culture, and socially-recognized and transmitted practices – Scheler, Plessner, and Gehlen provided a methodological and doctrinal foundation for philosophy that still promises a path forward in regards to contemporary problems.

Brandon Konoval, University of British Columbia
"'The Philosopher of Power'?: Nietzsche, Foucault and the genealogy of sexuality"

In 1975, Michel Foucault celebrated Nietzsche as "the philosopher of power, a philosopher who managed to think of power without having to confine himself within a political theory in order to do so." In 1976, staking his own claim to the mantle of "philosopher of power," Foucault published the *History of Sexuality, Vol. I: The Will to Knowledge*, in which his genealogy of a *scientia sexualis* provided the framework for a re-evaluation of the techniques, ambitions and general conceptualization of power that came to be associated with the term "bio-power" (Foucault, 1976). Instead of marking a constructive relationship between the two philosophers of power, certain commentators have argued rather that the *History of Sexuality* signaled a decisive rupture between the genealogical projects of Nietzsche and Foucault: thus, Hans Sluga has remarked that "we can read Foucault's genealogical work, particularly in the first volume of the *History of Sexuality*, as a redoing of Nietzsche's genealogical project—one that seeks to bypass its problems"—problems that Sluga, amongst others, has located primarily in Nietzsche's "distinctive understanding of the nature of power" (Sluga, 2003). Given the polemical orientation of the *History of Sexuality* toward a received conceptualization of power, what can have happened by 1976 to the Nietzsche who was still championed by Foucault in 1975? Did Foucault's innovative formulation of biopower render Nietzsche irrelevant both as 'philosopher of power' and as a critical resource for Foucault's analysis of the scientific discourse of sexuality?

Through Foucault's own record of commentary on the works of Nietzsche, dating from the 1960s through his lectures at the Collège de France, this study assesses the relationship between the *History of Sexuality, Vol. I* and Nietzsche's *Genealogy of Morality* (1887), a work of recurrent and pronounced interest to Foucault. Where Sluga et al. have characterized Nietzsche's account of power relations in terms that invoke the very model of power challenged by Foucault in the *History of Sexuality*, this study contends that Nietzsche's *Genealogy* offered the very foundation for key aspects of Foucault's alternative to the "repressive hypothesis" and his critical appraisal of the *scientia sexualis*. Furthermore, in his account of the ascendance of the ascetic priest, Nietzsche identified actors and components of power relations that Foucault was to carefully exploit in his critique of psychoanalysis and its relationship to bio-power. The role of Nietzsche's thought in the French anti-psychiatric movement of the 1960s and 1970s is addressed in light of Foucault's close intellectual association with the prominent French Nietzsche scholar, Gilles Deleuze, as well as Deleuze and Guattari's *Anti-Oedipus* (1972), in which the use of the *Genealogy of Morality* informs the critique of Freud later pursued in the *History of Sexuality*. Nonetheless, Foucault did indeed seek to 'redo Nietzsche's genealogical project' through his anatomy of bio-power, a scientific discourse by which the *Genealogy of Morality* found itself beguiled, despite the distinctive critical instruments it contributed to a genealogy of sexuality.

Session I.3 Sir Francis Bacon, Lord Verulam

Daniel Schwartz, University of California, San Diego
"Francis Bacon on the unity of discovery and justification"

Recent work by Friedrich Steinle, Theodore Arabatzis, and others has questioned the sharpness of the distinction between the context of discovery and the context of justification. This paper adds to those arguments by looking to Francis Bacon. When he addresses "proofs and demonstrations" by induction in the *De augmentis*, he observes that in deductive logic, discovery and demonstration are separate, whereas in inductive logic "the same action of the mind which discovers the thing in question judges it." He observes that a sharp distinction between demonstration and discovery makes sense in syllogistic logic. This is because the middle term in a syllogism must be causally explanatory of the conclusion, as in this example:

All dogs are carnivores.
All carnivores have incisors.
Therefore, all dogs have incisors.

The middle term, being a carnivore, explains why dogs have incisors; they have incisors because they need them to eat meat. Notice in this example that one discovers that all dogs have incisors before discovering that all carnivores have incisors, even though the order of justification goes in the other direction.

Induction is different, according to Bacon. In this paper, I first examine Bacon's account of the origins in scholastic logic of the discovery-justification distinction. Then I explain both in theory and by reference to examples in his natural histories why Bacon regards discovery and justification as inseparable. I also consider some problematic examples where the two seem to come apart; based on these examples, I argue that Bacon holds that discovery, like justification, comes in degrees. That is, instead of thinking of discovery as binary—one moment something is not on one's radar, and the next moment it is—Bacon thinks of discovery as a gradual process of becoming acquainted with the world more deeply over time. Finally, I discuss some of the implications that Bacon draws from the unity of discovery and justification, especially his view that the best way to communicate the results of any inductive investigation to students is to lead them down the steps that led to the discovery in the first place.

Ian Stewart, University of King's College
"Francis Bacon and the history of the philosophy of 'observation'"

In *Representing and Intervening*, Ian Hacking heralded Bacon as the "first philosopher of experimental science." Naming him as such was intended, in part, to draw attention to the ways Bacon thought about the relation of observation, theory, experiment etc. *before* positivism, and before the distinctions between inductivism and deductivism had become hooks (or gallows) by which historians of the philosophy of science could hang figures on. Moreover, for Hacking, Bacon offered insights to experiment that could have saved us from some disputes in the philosophy of science in the 19th and 20th centuries.

In particular, Hacking astutely took note of a feature of Bacon's *Novum Organum* long overlooked by historians of the philosophy of science, the so called 'Prerogative Instances' comprising well over half of book II, remarking that Bacon, as a philosopher of experiment, was not interested in the problem of 'observation' as such, that is, the distinction between 'seeing' and 'inference' in the way positivism (and phenomenology) would later demand. For Hacking, Bacon's approach to observation was "more like the way in which modern physics speaks of observation, than the concept...found in positivist philosophy." (*Representing and Intervening*, 249)

Whatever the truth of the first half of that provocative comparison, this paper draws on recent scholarship on Bacon's matter theory to revisit an important aspect of Hacking's assessment of Bacon: that he was aware of but (prophetically) indifferent to "the difference between what is directly perceptible and those invisible events which can only be 'evoked' [by experiment]." What recent scholarship is making clear is the degree to which Bacon's matter theory, particularly as laid out in his 'prerogative instances' and elsewhere, shaped his whole conception of his 'experimental' natural histories, as well as the inductive logic intended to 'process' the data of such histories. It was this matter theory that informed his keen interest in the problem of the difference between sensation and what lay 'beneath' sensation, and how that difference could be *carefully managed.* Far from being uninterested in 'observation', Bacon was determined through his prescriptive and descriptive writings to condition, to discipline, indeed to redefine what 'observation' meant, in light of his intense commitments to a metaphysics of matter, commitments which have remained, until recently, largely hidden from his reading public.

Far from offering a critique of Hacking, however, I hope to encourage his salubrious efforts to read early modern philosophy of science without the lenses of later philosophies of science, positivist, inductivist, or otherwise. I will conclude by turning to examples from the later 17th century, where Bacon's approach to observation was already misunderstood, and only through distortion allied to the empiricist projects of the mechanical philosophy and its distinctions, for example, between primary and secondary qualities. How Bacon from this point of view fits and does not fit into standard histories of the philosophy of science (from Kant onward) will likely be a subject for discussion that the paper raises, thus returning us, at least in spirit, to Hacking's commendatory (if somewhat cheeky) use of him.

Karen Zwier, University of Pittsburgh
"Experiment as test of causal claims: A history"

It appears to be a widely accepted view that there is something about experiment as a method of investigation that grants a special kind of access to knowledge about causes and effects. This view is central to contemporary manipulationist accounts of causation. However, while some may trace the history of manipulationist accounts only back to Gasking (1955), or perhaps even to Collingwood (1940), there is a much deeper and richer history. The idea of the link between experiment and causation is not a novel invention of contemporary philosophy of science; it has always been central to modern science, with roots stretching back to the beginnings of the scientific revolution. The historical background of the manipulationist view is underappreciated (if even recognized at all) by critics of the manipulationist account, and even among proponents of manipulationist causation, it has not been explored in detail.

This paper explores the history of the idea that experiment constitutes a privileged method for gaining knowledge of causes. Part of my aim in calling attention to that history is to argue against any temptation one might have to view manipulationist accounts of causation just as a current philosophical fad, as marginalizable, or as just one of several equally valid candidates for fleshing out the meaning of causal claims. Manipulationist accounts are, on the contrary, part of a long tradition of thinking about causation as empirically testable and intimately tied to experiment. More importantly, I show that the tradition itself—i.e., that of thinking about causation as tied to experiment—was by no means a sideline in the history of science; it is wrapped up in the fundamental ideas of the scientific revolution.

It is during a revolution that major thinkers articulate that which in later times becomes tacit. Thus, I begin this history by examining the thought of two of the main advocates for experiment during the early stages of the scientific revolution: Galileo and Bacon. I explore their explicit statements about the connection between experiment and knowledge of causes, and I also examine examples of experiments that they carried out and discussed in their writings. My examination will show that the turn toward experiment during the scientific revolution was marked by a sharp change in what was considered to be a valid cause. Galileo and Bacon labored to decrease emphasis on certain senses of causation prevalent in the academic Aristotelian philosophy of nature, while elevating a different sense of cause that was directly linked with the very experimental methodology that they were advocating. I then move forward in history and show that the narrowed experimental sense of cause that Galileo and Bacon promoted and characterized is strongly present John Stuart Mill's famous list of experimental methods two centuries later.

Ultimately, my examination of these three figures in this historical thread of thought reveals a great deal of constancy in the idea of the connection between experiment and causal knowledge. In addition, I highlight the progress and increasing sophistication of experimental methods for testing causal relationships.

Session I. 4 Scientific Methods and Explanation

Anastasia Guidi Itokazu, Universidade Federal do ABC

"Johannes Kepler's history of hypotheses: In defence of realism"

The present paper proposes an investigation on Kepler's account of the history of astronomical hypotheses. I argue that the passages concerned comprise some powerful arguments in support of the author's realist stance. The theme is directly addressed in the *Defence of Tycho against Ursus*, while it permeates the argument of the *New astronomy*. In both works, it is clear that Kepler believes in the human ability of acquiring true knowledge of the world, which includes its overall cosmic arrangement as well as the motions of heavenly bodies. Therefore, when he turns to the history of astronomical hypotheses, Kepler's goal is set right from the start: to show that astronomy, throughout its history, has always been a science concerned with the discovery of the *true* motions of celestial bodies. Copernicus and Ptolemy are thus approximated, insofar as Ptolemy is depicted as a realist. Accordingly, Copernicus is depicted as a genuine member of the Ptolemaic tradition of mathematical astronomers. It is well known that Kepler defied this very tradition, for having introduced non-circular orbits, because of his treatment of varying velocities as fundamental and, more generally, because of the physical character of his heliocentrism. However, unlike Galileo, Kepler was not interested in dismissing pre-Copernican astronomy as the work of fools. His rhetoric is somehow opposite. The relevance, to Ancient astronomy, of physical (or metaphysical) assumptions based on solid spheres constitute a precedent to his own introduction of physical hypotheses. In the *New astronomy*, instead of presenting a straightforward account of the two first planetary laws that bear his name, Kepler leads his reader through an intricate journey following the idealized path that would have led him from the data relative to Mars' apparent positions, previously collected by Tycho Brahe and his team, to the description of the planet's actual path around the Sun. A journey that was meant to respond to some important objections raised by his contemporaries, such as the astronomer David Fabricius (as James Voelkel has argued), but that in a broader sense was meant to show that the new physical astronomy was the legitimate heir to Ptolemaic tradition. The passage from apparent motions to the underlying reality, which constitutes the argument of the book, echoes the very history of astronomy, the passage from bare appearances to geocentric astronomy, and from there to the Copernican system and to Kepler's own physical heliocentrism. A gradual departure from direct sensation, which began with Ancient astronomy's project of representing apparent varying velocities by underlying uniform rotations, and culminated with Copernicus' admission of the Earth's motions.

David Marshall Miller, Duke University

"Pluribus ergo existentibus centris: Explanations, descriptions and Copernicus"

What motivated Copernicus to propose an alternative to the Ptolemaic geocentric theory? Ptolemy's system had been accepted for over a millennium, unaltered in essence and successively refined in application since first set down in the second century. Yet, at the beginning of the sixteenth century, Copernicus saw Ptolemaic astronomy as somehow inadequate. What was the problem he took it upon himself to solve? Scientific theories can fail at two distinct interfaces: that between phenomena and description and that between description and explanation. In the first case, the descriptions of phenomena derivable from the theory—e.g., the predictions generated by the theory—fail to correspond to experience. This is an empirical failure. In the second case, the phenomena are described in such a way that they cannot be satisfactorily explained. The physical principles, as explanantia, do not lead to the descriptions, as explananda. This is an explanatory failure. Copernicus cannot have been motivated by empirical failure. There was no outstanding, extensive set of problematic observations, either newly-discovered or slowly accumulated, that demanded novel explanation, and Copernicus's mature system did not appreciably improve predictive accuracy over its predecessors. Instead, Copernicus's motivation was the explanatory failure of Ptolemaic astronomy.

Even in antiquity, astronomers and philosophers alike noticed that physical explanations depended on the stipulation of a single, universal center, and could not be made to account for a descriptive system—like Ptolemy's—that posited a multiplicity of motions around a multiplicity of centers. Nevertheless, the empirical success of Ptolemaic astronomy proved attractive, and the problem of multiple centers was explicitly set aside in late antiquity. In the middle ages, however, this "Ptolemaic Compromise" was rejected by Arabic philosophers, especially Averroës, who insisted that Ptolemaic astronomy could not be reconciled with physical explanations and therefore had to be rejected. The problem of multiple centers, they said, could not be surmounted.

A resurgent Averroism in the Renaissance universities where Copernicus studied renewed interest in the gaps between Ptolemaic descriptions and physical explanations, especially the problem of multiple of centers. Thus, Ptolemaic astronomy's explanatory failure led Copernicus (and others) to attempt new reconciliations between observational astronomy and physics. In fact, Copernicus was a somewhat conservative Averroist. He did not reject Ptolemy outright, as did some of his peers (e.g., Fracastoro and Amico). Instead, he tried to preserve as much as possible of the Ptolemaic system, rejecting only equants, which he thought too egregiously departed from the physical "first principles" of astronomy. In the end, Copernicus did not solve the problem Averroës had raised. Like Ptolemy, he posited a multiplicity of centers, contrary to the demands of Aristotelian physics. Subsequent authors continued to struggle with the explanatory problem of multiple centers in Copernicus's heliocentric astronomy, and were thus led to seek novel and ultimately non-Aristotelian explanations of the heavens and the natural world—ones that did not depend on the stipulation of a center at all. A change of descriptions to save explanations led to changes of explanations to save descriptions.

Petter Sandstad, University of Oslo
"Philodemus on the joint method of agreement and difference"

John Stuart Mill's joint method of agreement and difference is known by any novice of philosophy of science. Yet it is almost equally well known that the method has several significant problems. It is a difficult method to apply, as it is highly difficult to formalize the phenomena so that Mill's method is applicable (pace William Whewell). Indeed, there are few cases where one can say that his method has been applied. Another serious problem is the number of methods that he presents, in all five methods, where the joint method of agreement and difference is a combination of two of these. But why is there no combination of the other methods, viz. the joint method of agreement and residues, etc.? Even though some have argued that Mill in practice only has one method (e.g. H. W. B. Joseph), his formulation of these methods lacks integration.

Variations of these methods can be found in earlier thinkers. E.g. Whewell thinks the four methods, though not the joint method, can be found among Francis Bacon's "prerogatives of instances". However, in a work by Philodemus called *De Signis*, one can find this joint method of agreement and difference.

The work is a defence of Epicurean method of inference, mainly against Stoic criticism. The Stoics thought the method of similarity (*homoiotē s*) invalid, in contrast to the method of removal (*anaskeuē*). Philodemus defends the method of similarity by arguing that the method of removal is a form of the method of similarity. He argues that they are not two (or some places three) forms of inference, but just one.

The two methods of similarity and removal are not far different from Mill's two methods of agreement and difference, with the difference that they are considerably broader inapplicability. However, Philodemus unites these two methods in a distinct manner, thereby differing considerably from Mill. The interrelation between the two is according to Philodemus so strong that they are basically two variants of the same method. As Elizabeth Asmis paraphrases, "the method of similarity "extends through" (*diē chei*) the method of removal, and that the latter is "confirmed" (*bebaioutai*) by the former." (Asmis 1984: 208-9) In this aspect the method of inference that Philodemus presents are superior to that of Mill, by circumventing some of the problems that faces Mill's methods.

Parallel Session II

Session II.1 Logical empiricists in context

Daniel Kuby, University of Vienna
"A 'bottom-up epistemology': Victor Kraft on discovery, justification and the tasks of the philosophy of science"

The distinction between "context of discovery" and "context of justification" has been introduced, as is well known, by Reichenbach (1938). Since the 1960s the DJ distinction has come under attack numerous times and has been criticized with a vast range of arguments. Only in recent times, however, the debate itself became a subject of historical investigation. With respect to the development of logical empiricism within the Vienna Circle, I present the neglected position of Viktor Kraft on the issue. His contribution, which he called a "bottom up epistemology" ("eine Erkenntnistheorie von unten") is worth to be looked at, as it reveals an interpretation of the DJ distinction in relation to different tasks of epistemology, which in turn may clarify some later arguments, especially those due to Paul Feyerabend. It is well known that Paul Feyerabend became one of the foremost critics of the distinction, but it is often forgotten that he was also Kraft's "best student" (as Kraft himself called him). First, I reconstruct Kraft's introduction of a DJ distinction as early as 1925 in his *Die Grundformen der wissenschaftlichen Methoden* – Kraft was a member of the Vienna Circle since its inception – and compare Kraft's and Reichenbach's notions of the DJ distinction. Also, both philosophers introduced an ulterior distinction, that between a descriptive task and a normative task of epistemology (DN distinction). In this respect I argue that it is possible to criticize the DJ distinction while maintaining the DN distinction and that many philosophers who later attacked the DJ distinction actually did so.

Secondly, I outline the descriptive and the normative task of epistemology in relation to the DJ distinction. I argue that Kraft's take on the matter allows him to develop a requirement of "empirical adequacy" of philosophy of science with respect to scientific practice. This requirement seems to be the cause of Krafts's idiosyncratic account of the descriptive task, which differs from the later notion of "rational reconstruction". This element is also mirrored in his conception of the normative tasks, but has another source: a fully developed empiristic theory of values

(*Grundlagen einer wissenschaftliche Wertlehre*, 1937). In this context Kraft defends a quasi-pragmatistic requirement, i.e. that norms, understood as conditions of realization of valued goals, should be constructed by means of their "empirical adequacy". Interestingly, both requirements are later weakened in Kraft's later account of epistemology (cf. his *Erkenntnislehre*, 1960).

Conclusively, I argue that this understanding of norms as it surfaces within Kraft's conception of the normative task of epistemology can account for Karl Popper's and Paul Feyerabend's idiosyncratic understanding of the normativity of philosophy of science. To make my case, I correlate Feyerabend's early normative understanding of philosophy, his later dismissal of the normative task and his "historical turn" with the contrasting tendencies in Kraft's conception of philosophy.

Nikolay Milkov, University of Paderborn
"On Walter Dubislav"

Walter Dubislav (1895–1937) was a leading member of the Berlin Group of scientific philosophy, the "sister group" of the Vienna Circle. The Group emerged around Hans Reichenbach's seminar as early as in 1927/8. Later Reichenbach was conducting joint seminars with Dubislav. In May 1929, Reichenbach and Dubislav were elected to the Board of the Society for Empirical Philosophy: Reichenbach as a President (*Vorsitzender*), Dubislav as a Manager (*Greschäftsführer*).

Collaboration with Dubislav on logic proved especially valuable for Reichenbach. Dubislav's work on definitions helped Reichenbach to clarify his position on "coordinative definitions". Another product of this collaboration was Dubislav's 1929 paper "Elementarer Nachweis der Widerspruchslosigkeit des Logik-Kalküls." Appearing in the *Crelles Journal*, this essay features Dubislav's "quasi truth-tables". Reichenbach himself pursued work along the same lines: three years later he employed Dubislav's table in his paper "Wahrscheinlichkeitslogik" (1932). It supported Reichenbach's theory of probability according to which propositions have three predicates: true, false, and their prediction-value or weight.

Dubislav and Reichenbach also shared a joint position in ethics, one that opposed the Vienna Circle's doctrine on the subject. Although both schools took anti-cognitivist stands in ethics, the Vienna philosophers championed a form of emotivism: they maintained that value judgments are expressions of emotions. This position distinguished two forms of understanding, knowledge and emotions, the problem with it being its reliance on elements of the German "life-philosophy" (G. Gabriel), with all of the complications that this doctrine held for the scientific philosopher that the Vienna Circle otherwise radically opposed.

In contrast, Reichenbach and Dubislav regarded all ethical propositions as implicit commands. Thus as with scientific propositions, which are posits, the propositions of ethics are, according to Reichenbach and Dubislav, products of the free will: the two philosophers saw this position as a triumph of the radical empiricism.

Unfortunately, the political changes in Germany in 1933 marked a break in Dubislav's career (and in his life): after Hitler came to power, he published scarcely anything. Apparently, the reason was that Dubislav, who unlike Reichenbach and Grelling was not Jewish but "Aryan," "believed that his connection with [the journal *Erkenntnis*; but also with the Society of which he took the helm upon Reichenbach's departure] would be harmful for his career." In 1937, Dubislav committed suicide under tragic circumstances.

Alan Richardson, University of British Columbia
"Making waves: Hans Reichenbach, radio philosopher"

This talk aims to illuminate Hans Reichenbach's early philosophical views by considering them in light of his interests in radio. Reichenbach was both a radio engineer, working as a consultant with the German army and then with Erich Huth's Gesellschaft für Funkentelegraphie, and a pioneer in popular science and philosophy on radio. The talk begins with an overview of Reichenbach's engineering and on-air activities. It then considers in more detail two issues. First, it asks what role Reichenbach's radio-engineering work played in his evolving conception of scientific philosophy in the 1920s. To this end, it considers in some detail Reichenbach's concerns with the measurement of amplification as presented in his 1919 manuscript, "Aktennotiz zur Verstärkungs-Messung" (which was concerned with the logical well-formedness and empirical tractability of three proposed quantitative definitions of degree of amplification; manuscript HR 0180-014-01 in the Reichenbach Collection at the University of Pittsburgh Archive for Scientific Philosophy) and his conceptual and engineering work in an interdisciplinary wartime experiment to determine which soldiers would make good battlefield wireless telegraphers. This scientific work is important in assessing two aspects of Reichenbach's philosophical interests. First, radio engineering involves detailed investigations into how the content of a message can be retained across changes in the physical processes transporting, condensing, and amplifying that message. Thus, Reichenbach's professional engineering work illuminates his specific interest in the information-carrying features of causal processes. Second, Reichenbach's part in an interdisciplinary team measuring psychophysiological features of soldiers in the field undergirds his understanding of the nature of scientific work, the collaborative aspect of which he consistently urges upon his philosophical colleagues. The second larger question of the talk is: what do we learn about Reichenbach's vision of the social importance of scientific philosophy by attending to his diligent efforts to bring science and scientific philosophy as a mass audience through the use of new communication technologies? I argue that here as elsewhere the popular efforts of the logical empiricists have been undervalued in our accounts of the trajectory of their philosophical project.

Session II.2 Philosophical theories in the age of Weltanschauungen

Tony Mills, University of Notre Dame
"Meyerson's épistémologie"

To most contemporary scholars, Émile Meyerson is a footnote in an obscure history: early 20th century French philosophy. While the rich traditions of French *épistémologie* are beginning to enjoy the scrutiny they deserve in contemporary scholarship, Meyerson's own legacy remains stubbornly elusive. Any attempt to give an account of his philosophical project thus confronts two obstacles. The first is that despite recent efforts (e.g., Brenner, Gale, Chimisso, Laugier), there remains no unified scholarly framework into which Meyerson's work can be placed. The second obstacle is Meyerson's own complicated place in the history of philosophy. This essay provides the groundwork for an attempt to confront these obstacles: by situating Meyerson's work historically, it seeks to elucidate the nature of his critique of positivism, the distinctive character of his epistemological method, and the significance his project has for the philosophy of science more generally.

I argue that, in Meyerson's writings, "*positivisme*" refers not only to the doctrine of Auguste Comte and its legacy, but also to a philosophical tendency, one that Meyerson takes to be ubiquitous in the understanding of science of his time. Inseparable from Meyerson's critique of positivism, however, is the implementation of a new methodology, which was preached, Meyerson claims, but not practiced, by Comte himself. This methodology is what Meyerson calls *épistémologie*, and it is predicated on the assumption that scientific rationality can only be understood through its historical development. *Épistémologie* reveals, Meyerson claims, an ineliminable tendency of scientific reason to *explicate*. Positivism can thus be characterized as the suppression of this "causal tendency," reducing scientific theories to *descriptions* rather than *explanations* of nature. This, however, is inseparable from a normative claim about the limits of scientific knowledge. Whether it be motivated by a desire to leave space for a metaphysics that grasps the reality underneath scientific description, or a Kantian commitment to the world of phenomena, positivism manifests itself in an delimitation of what science can and cannot say about the real. According to Meyerson, therefore, positivism is incomplete descriptively – since it misconstrues the nature of scientific reason – *and* deleterious to the progress of scientific knowledge, by making normative pronouncements from a vantage point divorced from scientific practice, which is the exclusive privilege of philosophy.

Meyerson's goal is therefore twofold: to reassert the right of science to function according to its own immanent principles without proscriptions from philosophy; and to provide a prolegomenon to any future metaphysics, which must respect the metaphysical right of scientific knowledge. But this "prolegomenon" consists in a determination of the principles of reason, which does not take place *a priori* through a transcendental deduction, but through an historical investigation into the products of thought. Meyerson can therefore be understood as taking Comte's program for a positive philosophy as a point of departure, but his *épistémologie* secures a place for a historically oriented philosophy that does not reduce all knowledge to observable facts, while forbidding metaphysics from inhibiting the development of scientific thought, by recognizing that science is itself metaphysical.

Kristian Camilleri, University of Melbourne
"The physicist as philosopher: Philosophical ambitions in cultural context"

Between 1880 and 1930, a new figure emerged on the European intellectual landscape – the philosopher-physicist. During this period many leading physicists, predominantly in the German-speaking world, were cast in the role of philosophers as they sought to come to grips with the transformation in the foundations of their discipline. Einstein, Weyl, Schrödinger, and Bohr were among the more prominent physicists of this era who saw their work as physicists as deeply interconnected with epistemological questions. Recent historical scholarship has shed important light on the emergence of this intellectual tradition, which has its origins in the work of Helmholtz and Mach. By the 1920s, the rise of neo-Kantianism and positivism, the new developments in physics, notably relativity and quantum mechanics, and the formation of societies and journals and the rise of a new style of theoretical physics provided a new institutional and intellectual setting in which a physicist could take the mantle of philosopher in the debates over the nature of space, time and causality.

Although the problem of knowledge constituted an important, and perhaps even the central, task for many philosopher-physicists, by the 1930s a number of German-speaking physicists turned their attention away from the epistemological problems of quantum mechanics and relativity, and instead became increasingly preoccupied with broader questions, which reflect wider cultural concerns of their time. Schrödinger, Pauli and Heisenberg in particular devoted Considerable effort, albeit in quite different ways, to the task of a attempting to articulate a new worldview, which included, but was not limited to, the conception of reality presented by the natural sciences. This philosophical task was by no means new, but it appears to have taken on a new urgency in the later years of the Weimar Republic. Indeed the work of all three physicists reflected themes that can only be fully appreciated once we situate them in the context of the pervasive sense of crisis that dominated German intellectual life in the decades that followed the First World War.

In this paper I explore the philosophical ambitions of this later generation of philosopher-physicists by focusing on the way Schrödinger, Pauli and Heisenberg took up philosophy later in their careers. Schrödinger's efforts to situate science within humanistic culture, Heisenberg's historicist approach in attempting to grasp the ordering of reality, and Pauli's foray into metaphysics reflect quite different responses to what they perceived as the intellectual challenge of their time. Here I am less concerned with the substance of their philosophical views, than in what they reflect about the physicist's understanding of the task of science and philosophy and the extent to which they were deeply and personally committed to this task. The intellectual projects pursued by Schrödinger, Heisenberg and Pauli, while idiosyncratic, provide an intriguing insight into the ways in which physicists responded to the perceived sense of fragmentation of intellectual life and the different intellectual resources they drew on in their private and popular writings during this period.

Session II.3 Thomas Hobbes

Marcus P. Adams, University of Pittsburgh

"Maker's knowledge and underdetermination in Hobbesian natural philosophy"

Despite his numerous criticisms of Aristotelian philosophy, Thomas Hobbes agreed with Aristotle that to have scientific knowledge (epistêmê) one must have causal knowledge. Hobbes often used the term scientia or its cognates to designate scientific knowledge and distinguish it from cognitio. However, Hobbes's account of how one acquired causal knowledge differed greatly from the Aristotelian account, since he held that causal knowledge about a phenomenon was available only to those who had acted as makers for that phenomenon, i.e., those who had maker's knowledge.

Hobbes thought that individuals had such causal knowledge when they constructed geometrical figures or commonwealths, and in Six Lessons he argued that the natural philosopher did not have access to such causes when attempting to explain natural phenomena:

> Geometry therefore is demonstrable for the lines and figures from which we reason are drawn and described by ourselves and civil philosophy is demonstrable because we make the commonwealth ourselves. But because of natural bodies we know not the construction but seek it from the effects there lies no demonstration of what the causes be we seek for but only of what they may be (EW VII.184).

If Hobbesian maker's knowledge were limited only to geometry and civil philosophy, as many in the literature have argued, then the scope of scientia would be incredibly narrow, so narrow that it would fail to solve the problem which I argue Hobbes meant it to solve (discussed below). As a result, in this paper I argue that Hobbes never confined his account of maker's knowledge in this way but instead held that those who constructed conceptions in first philosophy, which served as part of the foundation for his natural philosophy, had maker's knowledge as well.

First, I argue that Hobbes appealed to maker's knowledge to buttress his natural philosophy against the threat of skepticism about the possibility of scientia. Maker's knowledge was Hobbes's (mechanical) response to a worry that the actual causes of any given natural phenomenon were vastly underdetermined, a worry which Hobbes discussed frequently. Second, I explore how makers have causal knowledge when constructing geometrical figures and argue that they also have it when constructing conceptions in first philosophy. In these contexts, I examine the two steps that for Hobbes were involved in acquiring scientific knowledge: first, knowledge of particular causes from a construction; and second, knowledge of general causes from making a definition on the basis of the construction in the first step.

Finally, I show how explanations in Hobbes's natural philosophy made use of this maker's knowledge from geometry and first philosophy, examining an explanation from Hobbes's optics in *De homine*. I argue that Hobbes placed maker's knowledge at the foundation of his natural philosophy to ward off the threat of skepticism about scientia that might have resulted from recognizing the underdetermination of the actual causes of natural phenomena. Although the natural philosopher cannot demonstrate the actual causes of natural phenomena (as Hobbes argued in the Six Lessons quotation above), by using maker's knowledge the number of causes that "may be" would be greatly reduced and thus the threat of underdetermination would no longer plague the natural philosopher.

Geoffrey Gorham

"Hobbes on motion, time and conatus: A realist account"

Although Hobbes is famous (and was infamous) for his materialism, motion is as fundamental as matter to his metaphysics and natural philosophy. Hobbes maintains that all "all mutation is motion" including the mutations in our sense organs and brains which constitute perception, imagination and memory. Indeed, all causation, power and activity are nothing but motion. So a body at rest cannot act upon or resist a moving body. Hobbes's basic physical principles are laws of motion: all physical interaction is between contiguous moving bodies; whatever is in rest or motion remains in that state unless acted on mechanically by another body. And his core dynamical notion, the *conatus* or endeavor carried by moving bodies and transferred in collisions, is essentially kinetic. He defines *conatus* as "motion made in less space and time than can be given", while cautioning he does not mean motion in a spatial or temporal point: "for there is no such thing in nature (*in rerum natura*)". As Brandt observed many years ago, "Hobbes should more correctly be called a motionalist . . . he is the philosopher of motion as Descartes is the philosopher of extension".

As one might expect, Hobbes accepts that motion presupposes time: "It cannot be conceived that anything can be moved without time". Consequently, time plays an irreducible role in his key physical concepts and laws, and in his motionalist theories of sensation and memory. The problem is that Hobbes seems to advance a reductionist, or even idealist, conception of time itself. Against Aristotle, he denies that time itself can measure motion; rather motion measures time. Moreover, he argues that both time and space are in a certain sense mind-dependent or 'imaginary'. Finally, he sometimes asserts that strictly speaking only the instantaneous present exists: the past and future are merely phenomenal. In various ways, these views of time threaten to undermine the motionalist foundations of Hobbes's mechanical philosophy. Most importantly, they seem to imply that motion itself, and therefore Hobbes's entire system of natural philosophy, is subjective or ideal. But although the idealist picture of Hobbes has been vigorously defended, there is strong evidence against it in his scientific writings. Temporal idealism is especially problematic for Hobbes since his own mechanistic account of human thought requires objective or real motion and time.

In this paper I offer a reconstruction of Hobbesian time that relies on the analogy with space, which is given greater attention than time by Hobbes and his commentators. I argue that just as there is for Hobbes a real space (the magnitude of bodies) corresponding to 'imaginary space', there is a real time corresponding to 'imaginary time': the successive duration of motion. This conception of real time, I argue, is consistent with Hobbes's thoroughgoing materialism and nominalism, but avoids the idealist and phenomenalist implications of 'imaginary time'. I conclude by briefly considering how Hobbes's mature views on time and motion bear on the young Leibniz's reconsideration of strict Hobbesian mechanism, particularly his appropriation of *conatus*.

Edward Slowik, Winona State University
"Hobbes and the 'phantasm' of space"

This presentation will explore Hobbes' theory of space as presented in his major work on natural philosophy, *De corpore* (1655), as well as from other sources from the 1640s and later, with the main emphasis placed on the problems associated with the concept of imaginary space, and the manner by which Hobbes reckons that imaginary space is obtained from our experience of the world. This particular aspect of Hobbes' theory of space poses the greatest challenge to commentators, and has elicited many divergent interpretations. As will be argued, the best philosophical interpretation that gathers together both the strong subjectivist, or empiricist, features of Hobbes' theory of space, alongside the role apparently accorded to abstraction, is the anti-universals thesis, nominalism. Despite the recent, and excellent, investigations of many aspects of Hobbes' theory of space (e.g., Leijenhorst's study of the Scholastic background), the nominalist component in Hobbes' thought seems under appreciated in these contemporary studies. What is important about a nominalist interpretation of Hobbes' theory of space is that it straightforwardly incorporates all of the separate functions of his cognitive theory, e.g., sense, memory, abstraction—all of which are either implicitly or explicitly involved in his conception of imaginary space—with his theory of language, as names or marks for these perception, memories, and abstractions (and which stand for universals). In fact, as a final verdict and summary, the problem with Hobbes' imaginary space conception lies in the conjunction of his cognitive theory and his nominalist theory of language. By merely grouping together all of our different cognitive functions that involve space under a single name or sign, the many different approaches to the problem of space, in particular, the epistemological versus the ontological, are often conflated. One of the unfortunate side effects of this conflation is the number of widely divergent interpretations that his spatial theory has elicited among later commentators, as will be demonstrated. Nevertheless, not only does Hobbes' spatial theory foreshadow the work on space perception of the later British Empiricists, but his treatment of many of the problems and issues that bedeviled the more ontologically and theologically oriented investigations of space in the seventeenth century are quite unique and forward looking, since Hobbes' does not accept the ontological grounding of space that comprised the century's default view (i.e., God). In this respect, Hobbes' theory provides a much better instance of the sort of non-metaphysical, definitional or constitutive formulation of space that is often (and erroneously) attributed to Newton by many modern day Positivist-inclined philosophers.

Session II.4 Wallis and Kant

Adam Richter, University of Toronto
"The Trinity and the cube: Nescience in the epistemology of John Wallis"

John Wallis (1616-1703) is best known as one of the leading mathematicians of the Royal Society of London in the seventeenth century, whose work contributed to Newton's development of infinitesimal calculus. Yet Wallis was a man of many talents: as well as mathematics he was accomplished in linguistics, education of the deaf, physics and theology. Wallis's theology has received little attention from historians, and those who have examined it have generally viewed it in isolation from his other intellectual pursuits. Yet Wallis does not make a sharp distinction between his fields of study; in fact, he draws on the language of mathematics and physics to convey theological concepts. For example, he compares the Holy Trinity to a cube. The length, width and breadth of a cube, Wallis argues, are equal and are all Necessary for the cube's existence, but none of them constitutes a cube on its own. Such, in Wallis's conception, is the nature of the Father, Son and Holy Spirit: they are equal and equally necessary parts of God.

I argue that the mathematical and physical metaphors in Wallis's theology do not reflect merely superficial similarities in his thought. Rather, they represent an epistemology that Wallis applies to both his theology and natural philosophy. Wallis acknowledges the limits of human reason and, accordingly, tolerates a degree of nescience in all fields of inquiry. In his theology, he admits that his metaphors will never correspond exactly to the nature of divine mysteries like the Trinity, as these mysteries are beyond human understanding. Since the nature of the Trinity is inscrutable to everyone except God, Wallis argues, the best a person can do is create metaphors that reflect the simplified understanding of the Trinity that God has made available through Scripture.

In his physics, Wallis takes a similar approach to the ultimate causes of natural phenomena. In his theory of tides, for instance, he argues that earth and moon share a centre of gravity, thus creating a small epicycle around which they both revolve as they orbit the sun. Wallis argues that this motion accounts for the observed monthly tidal cycle that other theories have failed to explain. Wallis's peers, not yet having encountered to the notion of universal gravitation, objected that he had not explained how the earth and the moon could have a common centre of gravity without being physically connected. Wallis replies that this is beyond his concern: his task as a physicist is to recognize natural phenomena, not to explain their ultimate causes. For him such causes are beyond the scope of human understanding, much like the nature of the Trinity. Such a toleration of nescience is apparent in the empiricist natural philosophy of contemporaries of Wallis like Newton and Boyle. An understanding of the role of nescience in Wallis's theology and natural philosophy may therefore shed light on the epistemological links between theology and empiricism in Restoration-era philosophy.

Michael J. Olson, Villanova University
"Metaphysics and science in Kant's Copernican revolution"

Despite the fact that Kant himself never employs the phrase 'Copernican revolution' to describe his own reorganization of metaphysics, the general familiarity of the phrase has come to overshadow the details of Kant's own invocation of the Copernican project in the Preface to the 1787 edition of the first *Critique*. In this paper I will offer an analysis of the meaning of the Copernican revolution in critical idealism based on Kant's understanding of Copernicus's place in the history of the natural sciences. This analysis will proceed by situating Kant's adoption of the Copernican legacy within two contexts: first, I will situate the Copernican revolution in relation to two marginal notes in Kant's copy of Baumgarten's *Metaphysics* written in the late 1770s; second, I will recapitulate Kant's analysis of the intellectual revolutions in mathematics and experimental science in the B Preface in order to indicate the relation between Copernicus and the shared structure of these earlier scientific revolutions. When read together, these passages invite a different understanding of the way in which the Copernican revolution is Copernican. Kant's late pre-critical marginal notes contrast Copernicus with Tycho Brahe and the Pythagorean Philolaus of Croton. Together they indicate that Kant takes the importance of Copernicus in the development of the science of astronomy to be inseparable from the role that suitable empirical evidence plays in rendering scientific speculation properly scientific.

When Kant's well-known discussion of the importance of the Copernican hypothesis in the B Preface is read in relation to these other references to Copernicus in Kant's writings, the significance of the Copernican character of his Copernican revolution is considerably altered. Rather than understanding the Copernican revolution effected by transcendental idealism to be primarily focused on a hypothetical or speculative change in perspective which includes the activity of the spectator within the analysis of the appearance of phenomena, we can see that the importance of the figure of Copernicus in Kant's eyes is more crucially connected to the necessity of empirical or experiential proof in grounding conceptual speculation. The Copernican revolution is importantly Copernican, rather than, for example, Philolaic, insofar as it distinguishes itself from mere speculation. The reversal of the epistemological priority of subject and object, which is more generally recognized to be the heart of the Copernican revolution, is, then, of secondary importance; this reversal is the means by which Kant attempts to secure, for his own metaphysical intervention, the proof demanded by what he understands to be the distinguishing scientific feature of Copernicus's astronomical revolution. I will develop this claim by reviewing Kant's analyses of the intellectual revolutions in mathematics and physics in the B Preface. In both cases, Kant claims, these disciplines became sciences by adopting new conceptions of their own objects: geometry became a science when it thought of its objects as constructions rather than either definitions or figures and physics became a science when it embraced the experimental manipulation of natural phenomena rather than nature itself as its object of study. The result of reading the totality of Kant's remarks on Copernicus together, then, is an understanding of the interconnection of what metaphysics takes its objects to be and the means by which it can establish the epistemic validity of its claims concerning those objects. Moreover, Kant's articulation of his own project in terms of the Copernican revolution itself relies on a specific understanding of the methods and history of the sciences and so indicates the early modern interconnection between research in metaphysics and the history of science.

Parallel Session III

Session III.1 Symposium: Dedekind, mathematical methodology and the notion of function

It is widely acknowledged that in the nineteenth century there occurred an important shift, or even a revolution, in mathematical methodology. This shift is connected with the transformation of mathematics from the study of quantity, as it was traditionally seen, to a more abstract conception of mathematics, as the study of relational structures quite generally; and the latter involved the acknowledgement of various new kinds of mathematical entities, as well as the articulation of corresponding methodological principles and basic laws. The mathematician Richard Dedekind (1831-1916) played a central role in this development. In the four talks in this symposium, Dedekind's contributions and their significance are considered from several different angles. The overall goal is to illustrate how reflections on the development of mathematical methodology, and on other aspects of mathematical practice, can be philosophically profitable.

For that purpose, historical and philosophical considerations are intertwined in a number of ways.

In the first two talks Dedekind's methodology is discussed with respect to two general themes. To begin with, there are direct connections between methodological matters and Dedekind's acceptance, indeed his central use, of infinite sets in mathematics. In this context, Dedekind's contributions can be compared profitably with Bolzano's, Cantor's, and Frege's. Arguably, it is in Dedekind's works that the actual infinite came to be built centrally into mainstream mathematics. His writings are also shaped by the desire to find apt and fruitful definitions more generally, as has been noted before with respect to number theory. Equally illuminating, but relatively neglected so far, is his joint work with Heinrich Weber on algebraic functions, as is argued in the second talk. When that work is taken into account, what becomes evident is the role a combination of methodological values and norms played for Dedekind. What also comes to the fore are some striking connections between his views about mathematical methodology and the human mind.

In the remaining two talks, the focus is somewhat narrower, although there are connections to several themes from the first two as well. A crucial part of the transformation of mathematics in the nineteenth century was not just the introduction of infinite sets, but also a related broadening of the notion of function, including the treatment of functions as entities in their own right. In this connection one can distinguish several different notions of function, all operative in Dedekind's writings and in the nineteenth century more generally, as is established in the third talk. It also leads us back to Dedekind's views about the human intellect, to be clarified further in Kantian and Husserlian terms. Close attention to the step-by-step emergence of a general notion of function in Dedekind's works helps, furthermore, with respect to understanding better the axiomatic method and the structuralism he adopted, as the final talk illustrates. And these are intimately tied to his acceptance of the actual infinite and related methodological desiderata.

Erich Reck, University of California at Riverside
"Dedekind's methodology and the infinite in mathematics"

Several of Dedekind's contributions to the investigation, as well as the acceptance, of the actual infinite in mathematics are well known. This includes ideas and techniques from his 1888 essay, *Was sind und was sollen die Zahlen*?, such as: his definition of being (Dedekind-) infinite for sets (1-1 mappable onto a proper subset); his characterization of the natural numbers in terms of the notion of simple infinity (thus as finite ordinal numbers); his related analysis of mathematical induction and recursion (later generalized by Zermelo and von Neumann to the transfinite case); and his use of initial segments of the natural numbers series to measure the cardinality of finite sets (as tallies). In addition, there is his definition of the notion of continuity, and the related construction of the real numbers via Dedekind cuts, in his 1872 essay, *Stetigkeit und irrationale Zahlen*. Beyond both essays, Dedekind's correspondence with Cantor contains a proof of the countability of the set of algebraic numbers. Moreover, one can find all the ingredients for a proof of the Cantor-Bernstein theorem (that two sets that are 1-1 mappable into each other are isomorphic) in Dedekind's writings, as he was well aware.

In this talk, I will start by providing a chronology and comparative discussion of these contributions. Building on them, I will then argue that Dedekind's role concerning the infinite should be seen as even more important than is common in the literature. Thus, Cantor is typically credited with having introduced successful considerations of the actual infinite into mathematics, by means of his work in analysis and the theory of transfinite sets and numbers that grew out of it. But Dedekind did not only use the infinite seriously earlier than Cantor, in his work on algebra from the 1850s and 60s, he also tied acceptance of the infinite more intimately to various parts of mathematical practice than Cantor. My argument for the latter will be based on a discussion of Dedekind's novel and very influential methodology, as illustrated both by his foundational and by his other, non-foundational writings. One might speak of a combined infinitary, set-theoretic, and structuralist methodology in this connection, in a sense to be spelled out further in the talk.

My ultimate point is not to elevate Dedekind over Cantor in a priority dispute. Rather, it is to illustrate how discussions of the actual infinite shifted from being tied to metaphysical debates, as had been usual from Aristotle until the nineteenth century, to being connected with mathematical practice later on. Insofar as Cantor remained more interested in the older metaphysical debates (as did, e.g., Bolzano), Dedekind's case is a better illustration for this shift. His case is also illuminating insofar as in his writings foundational and more mainstream mathematical concerns blend seamlessly into each other (more than, e.g., in Frege's case). By exploring the latter aspect, the talk is meant to be a contribution to the recent turn towards "mathematical practice" in the philosophy of mathematics.

Emmylou Haffner, Université Paris Diderot - Paris 7
"Generality of definition and arithmetical methodology in Dedekind"

From Dedekind's point of view, there is no mathematics without mathematicians doing it. The mathematicians need to constantly search for (general) definitions from which whole theories can be derived, without loss of generality and rigor, and which hold the promise of further developments. In his own pursuit of this goal, Dedekind goes back and forth between epistemological values, and mathematical notions and methods, which he often rethinks.

In this talk I will go back and forth between Dedekind's practice and my exegesis. As is well known, methodological concerns play a leading role in his mathematics; they guide his quest for the "right" definition of central notions. I will focus on some specific epistemological norms and values found in Dedekind's works. Among Dedekind's methodological requisites, three have already been discussed widely: rigor, simplicity, and purity. Another one has been neglected so far: efficiency. Finally, the demand for generality is of the upmost importance for him — the leading virtue, so to speak. These self-imposed dicta are intricately related to each other and form the cornerstone of what I will call Dedekind's "foundational project".

In the first half of the talk, I will discuss how Dedekind is led to introduce new concepts and corresponding methods, such as those of field and ideal, as providing a "higher point of view" for the subject of number theory. I will consider the role of basic arithmetical operations (the so-called "Spezies") for Dedekind, and suggest that they have a normative value and may have been considered as forming part of the structure of human understanding, providing then an epistemic tool for the development of this "higher" level with which Dedekind wishes to work. Then, and as systematically as possible, Dedekind strives for definitions and methods that make use of solely "the simplest principles of arithmetic", reaching this "higher" level, in which "algebra and the theory of numbers interconnect in the most intimate manner". As will become evident, the "higher" level, in which Dedekind's theory of algebraic numbers is grounded, emphasizes the primacy of laws over objects and allows to see how arithmetical operations are used as a means for generalization.

In the second half of the talk, I will consider Dedekind's *Theorie der algebraischen Funktionen einer Veränderlichen* (1882), co-written with Heinrich Weber. In this work, Dedekind's ideal theory is applied to algebraic functions and used so as to algebraically re-define Riemann surfaces. Focusing on that text allows us to view Dedekind's mathematical practice from a particularly interesting point of view. Since the theory does not deal with the very notion of number, and is indeed presented as an algebraic version of Riemann's invention, it illustrates clearly the role arithmetic played in the shaping of Dedekind's general methodology. Moreover, it embodies a striking convergence of his methodological requisites. This sheds further light on his "definitional project," here by showing how new concepts are mobilized to elaborate his re-definition of the notion of Riemann surface.

Ansten Klev, Leiden University

"Mappings in Dedekind"

Any reader of Dedekind's *Was sind und was sollen die Zahlen*? will recognize the important role played in that work by the notion of mapping. Indeed, Dedekind claims that it is on the mind's capacity for mappings—its capacity for letting one thing correspond to another—that arithmetic rests.

A mapping is a function in one sense of that term. There are, however, other essentially different notions of function, and the first part of our paper will be devoted to contrasting mappings with other kinds of function. It will be argued that we find at least three different notions of function in Dedekind's work. According to the now standard definition of a function, it is a special kind of set, namely a set of ordered pairs satisfying the condition of functionality. This notion of function is not found in Dedekind, for he treats both mappings and sets as primitive, and hence defines neither notion in terms of the other. A definition of function still current in the second half of the nineteenth century was that given by Euler, as an "analytical" expression involving variables. That notion of function Dedekind did recognize. Indeed, in § 11 of *Was sind und was sollen die Zahlen*, they are explicitly distinguished from functions as mappings. Here it seems that Dedekind followed Cauchy and Galois, who distinguished functions as analytical expressions from what they called substitutions. In Dedekind's algebraic and lattice-theoretic work one finds yet another notion of function, namely what Dedekind calls an operation; examples are addition, lowest common divisor, union. An operation may naturally be viewed as a binary mapping, but it cannot replace the notion of mapping in Dedekind's system, for it is conceptually posterior to that notion—the definition of operation rests on the notion of cardinality, which in turn rests on the notion of mapping.

Dedekind's idea of a capacity for mappings is closely related to his logicism, more specifically to the view that the origin of arithmetic is to be found in the understanding and not in sensibility. Dedekind seems to have considered the notion of set as unproblematically logical, and given that the notion of mapping is the other key primitive in his grounding of arithmetic, he sought to establish that the latter is likewise purely logical. It is in this light one must view his repeated claim that the capacity for mappings is one without which "no thinking at all would be possible". Sadly, Dedekind never expanded on this claim, so one is left to speculate on what it implies. In the second part of our talk we will do so in the light of both Husserl's notion of categorial intuition and Kant's pure concepts of the understanding. This will suggest ways in which the notion of mapping can be said to be logical as well as how the capacity for mappings may be considered a "condition for the possibility" of thinking.

Dirk Sclimm, McGill University

"The early development of Dedekind's notion of mapping"

The notion of mapping (*Abbildung*) presented in Dedekind's *Was sind und was sollen die Zahlen*? (1888), and central to Dedekind's mature mathematical and philosophical outlook, is carefully analyzed in Ansten Klev's contribution to this symposium. This notion did not suddenly appear fully formed in 1888, but is the result of a continuous development that can be traced back to the earliest writings of Dedekind, namely his *Habilitationsrede* (1854) and the lecture notes on group theory and algebra (1855–58). The present contribution (based on joint work with Wilfried Sieg) aims at presenting and discussing this development with particular attention to Dedekind's work on the real numbers, *Stetigkeit und irrationale Zahlen* (1872), algebraic number theory (from 1863, 1871, 1877, and 1879), various drafts for the booklet on the natural numbers (1872–78), and his correspondence with Cantor.

To distinguish the different conceptions of mappings that can be identified in Dedekind's writings it is useful to look at the elements that can be used as domain and range of functions and mappings. A careful look at his writings reveals that Dedekind gradually arrived at a rigorous concept of mapping that allows for different kinds of objects to be mapped to each other. Moving away from considering only numbers as possible domains and ranges for functions, Dedekind mentions correspondences between different kinds of objects in 1872; but the first time Dedekind speaks of a mapping between different kinds of objects is only in 1888.

There is also a change in how Dedekind treats functions and mappings as genuine objects of investigation. Despite using homomorphisms implicitly in his early algebraic notes, it was only in 1877—when *Stetigkeit und irrationale Zahlen* (1872) was already written but before the publication of the axiomatic presentation of the natural numbers (1888)—that Dedekind discussed for the first time in print the properties of mappings and explicitly formulated those that are now called 'injectivity' and 'surjectivity'. Thus, Dedekind's 1872-78 draft of *Was sind und was sollen die Zahlen*? is the first evidence for the development of the conceptual apparatus needed in order to formulate the idea that two models of an axiom system that belong to different domains of objects have the same structure (i.e., that they are isomorphic). This is crucial for an interpretation of Dedekind's work on the real numbers as being 'axiomatic' (as is his later work on the natural numbers), since we can now explain the lack of a categoricity theorem for the real numbers in Dedekind's 1872 publication. Finally, it shows that the mathematical background of his structuralist philosophy of mathematics emerged only gradually in his writings.

Session III.2 Symposium: What the philosophy of biology was: Neglected figures in early twentieth-century philosophical and theoretical biology

Among contemporary philosophers of biology, it is widely believed that: (a) philosophers had little interest in the life sciences prior to the downfall of logical empiricism, and (b) the few philosophical excursions into the biological realm that did take place prior to the late 1960s and 70s were entirely fruitless. The papers in this symposium will show that both of these theses are mistaken. The aim of the symposium is to lay the groundwork for a long overdue reappraisal of the history of twentieth-century philosophy of biology by examining the oeuvre of four neglected organicist thinkers who are nonetheless archetypes of what the philosophy of biology once was: Joseph Needham, Paul Weiss, Ludwig von Bertalanffy, and Joseph Henry Woodger. The participants in this symposium will survey the key ideas held by these individuals in an attempt to demonstrate their importance, not only as objects of historical study, but also to contemporary debates in the philosophy of biology. The philosophy of biology did not arise ex nihilo in the last third of the twentieth-century—the prevalence of this misguided view has caused contemporary philosophers of biology to neglect decadesworth of thoughtful and stimulating philosophical and theoretical work in the life sciences. By restoring the intellectual legacy of Needham, Weiss, Bertalanffy, and Woodger, this symposium hopes to convince philosophers of biology to rethink and look back at their discipline's history so that the philosophical and theoretical writings of these and other early twentieth-century authors may be brought into fruitful interaction with the modern literature.

Erik L. Peterson, University of Wisconsin, Madison

"Joseph Needham's new and improved organicism in the midst of growing reductionist consensus, 1925-1938"

In the late 1920s, a young British biochemist named Joseph Needham developed a philosophy of biology he termed "neo-mechanism." Neo-mechanism supposedly carved a "middle way" between physico-chemical reductionism supported by his peers in biochemistry and the "vitalism" of Hans Driesch. Needham derived inspiration for neomechanism from four sources: Popular accounts of dialectical materialism; The Christian socialist movement; The pre-WWI organicism of J. S. Haldane, E. S. Russell, and L. J. Henderson; British Emergentism associated with C. Lloyd Morgan and C. D. Broad. Needham's neo-mechanism served as a bridge concept between older "organicism" and the systems-oriented philosophy of biology often associated with Ludwig von Bertalanffy and J. H. Woodger.

In this essay I will address three broad questions: 1. What was the content of Needham's approach? 2. To what extent did the developing third way approach contrast with the earlier "mechanism" and "vitalism"? Here I am really asking: Was the new and improved organicism / emergentism a true improvement on alternative approaches to mechanism vs. vitalism or a rehashing of early 20th century organicism? 3. How was Needham's third way developed into later systems approaches?

Jon Umerez, University of the Basque Country

"Paul Weiss and the organicist roots of hierarchical thinking"

In this paper I trace the organicist roots of hierarchical thinking which, having characterized early twentieth century theoretical biology, reappeared quite prominently at the turn of the 1960s, and is again acquiring currency today. The concept of 'levels of organization' was a key element in the theories of organicists such as Paul Weiss, Ludwig von Bertalanffy, Joseph Needham, J. H. Woodger, and others. The work of Paul Weiss in particular embodies rather nicely the continuity of hierarchical thinking in biology. In 1922 Weiss published a dissertation on the resting positions of butterflies in response to light and gravity that criticized Jacques Loeb's mechanistic theory of tropisms, offering in its place "a general systems theory of animal behavior" (see Weiss 1969) that explicitly adopted a hierarchical approach. Hierarchical thinking remained essential to Weiss's subsequent experimental work (in embryology, neurology, and general cell biology) and it also contributed to a number of theoretical debates at the end of the 1960s and the beginning of the 1970s. This is exemplified in a volume edited by Weiss on *Hierarchically Organized Systems in Theory and Practice* (1971), to which he contributed an essay on "The Basic Concept of Hierarchic Systems" in the life sciences, and in his contribution to the celebrated Alpbach symposium on the limits of reductionism organized by Arthur Koestler in 1968. My paper will present a preliminary analysis of the connection between Weiss's biological work on developmental issues and his philosophical perspective grounded on systems and hierarchical thinking as a way to assess the scope and limits of his influence (and that of other scientists with a similar approach) at different moments in the last century as well as today.

Daniel J. Nicholson, Konrad Lorenz Institute for Evolution and Cognition Research
"The enduring relevance of Ludwig von Bertalanffy's organicist conception of the organism"

Ludwig von Bertalanffy is mainly remembered today as the founding father of General Systems Theory. However, Bertalanffy was first and foremost a biologist, and for most of his professional career he was principally concerned with theoretical and philosophical questions arising from the biological sciences. Borne out of a general dissatisfaction with both mechanicism and vitalism, Bertalanffy developed, from the late 1920s onwards, an organicist (or "organismic") theory of living systems based on the concurrent repudiation of the mechanistic assumption that organisms are machines fully explainable in terms of their parts on the one hand, and the vitalistic appeal to mysterious agencies to account for the holistic capacities of organisms on the other. Instead, Bertalanffy's organicism emphasized the emergent and irreducible properties of organisms and regarded their teleological, self-producing, hierarchical organization as the hallmark of their ontological distinctiveness. The implication of this view was the conviction that biology ought to be regarded as an autonomous science possessing its own theoretical principles grounded in the characteristic thermodynamically open nature of living systems. Bertalanffy argued that the living state results from a specific organization of the material tissues and energetic streams that flow into the living system, are exploited by it, and are then ejected by it. Using this conception of the organism (which he attributed to Heraclitus), Bertalanffy sought to bring a wide range of organismic phenomena, such as metabolism, growth, and morphogenesis, under a single unified theoretical framework. This paper will draw on Bertalanffy's two most influential biological works, *Modern Theories of Development* (1933) and *Problems of Life* (1952), in an attempt to illustrate how his basic theoretical understanding of the organism is being increasingly vindicated by the latest empirical findings of biology, and that consequently revisiting his work can be of great value in advancing current disputes in the philosophy of biology.

Richard Gawne, Duke University
"J.H. Woodger, logical empiricism and the unity of science"

Although the work of Joseph Henry Woodger is often said to exemplify all that was misguided about positivistic excursions into the life sciences, close historical research reveals that his relationship to logical empiricism is complex and difficult to characterize. In this essay, I provide an overview of some of the recurring themes in Woodger's corpus, and then compare his views to those of several prominent logical empiricists. Woodger's popular reputation as a logical empiricist, I argue, can be traced to his attempts to axiomatize biological theories. This formal work was undoubtedly inspired by certain theses that are often connected with the logical empiricists, however, the use of formal logic is hardly a sufficient condition for being a member of the movement. Indeed, many themes in Woodger's work are antithetical to some of the well-known tenets of logical empiricist philosophy. Among other things, he refused to accept the verificationist criterion of meaning, and argued that metaphysics and science can be mutually complimentary. What, then, are we to make of the claim that Woodger was a committed logical empiricist? I argue that Woodger's connection to logical empiricism is best understood by studying his relationship to Otto Neurath's unity of science program. Throughout his career, Woodger vigorously criticized the sort of disciplinary overspecialization that Neurath's movement sought to combat, and repeatedly argued that the use of unclear terminology in the biological sciences presented a serious obstacle to epistemic progress. I conclude by suggesting that these features of Woodgerian biophilosophy are virtues, rather than vices. Insofar as the alleged shortcomings of Woodger's work are not easily attributable to logical empiricist influences, the claim that this tradition stunted the development of twentieth-century philosophy of biology will need to be reconsidered.

Session III.3 Symposium: Normative naturalism in Comte's positive philosophy

How can a positivist talk about values? This is a problem for Comte's positivism, which includes both a philosophy of science and a political philosophy. His philosophy of science is supposedly drawn from the history of science, and the application of this philosophy to the study of social questions is supposed to provide the basis for a social science that grounds social policy. But neither a normative philosophy of science nor a social and political philosophy can be drawn from the study of history alone. Minimally, these disciplines must be premised on some conception of our epistemic, social, and political goals, which cannot be defended just by appealing to empirical facts.

Comte's problem bears some analogy to that subsequently faced by twentieth-century positivists: how does one engage in normative inquiry when one's philosophy says that only empirical questions are meaningful? We do not pretend that Comte provided answers to this question. Nevertheless, his attempts at a naturalized epistemology and social philosophy shed light on the role that empiricism can play in philosophy.

Warren Schmaus argues in his paper that Comte's philosophy should be evaluated relative to the alternatives available in his day, rather than more recent philosophies. His approach to questions of knowledge is certainly no worse than these alternatives. Furthermore, he should be credited with turning philosophers' attention to the history of science and to the pursuit of knowledge as a collective rather than an individual endeavor. Whewell, of course, was engaged in a similar project. But as Laurent Clauzade shows in his paper, he reached very different conclusions from the history of science. He criticized Comte for claiming that metaphysical concepts had no future in science and argued that history shows instead that disputes over such concepts are integral to the scientific process. These differences between Comte and Whewell highlight the very problem of attempting to ground a philosophy of science in the history of science. Yet this case also illustrates the role that the history of science can play in philosophy. Although it may not be sufficient to establish a philosophy of science, a philosophy of science must still take the history of science into account.

Similar conclusions can be reached concerning Comte's social philosophy. Michel Bourdeau's paper once again illustrates the problem of naturalism by showing how Comte and Hayek were able to reach very different conclusions from the facts about history and society. But Hayek nevertheless accepted that the complexity of social phenomena should serve as the starting point for discussions of social policy. Vincent Guillin considers the relevance and role of factual knowledge in formulating social policy, given a sociology that assumes a deterministic social order. These two papers show us how, although facts about history and society alone cannot tell us what our goals should be, they are certainly relevant to philosophical discussions about the means to achieve these goals.

In sum, although Comte's naturalism ultimately fails, he succeeded in turning philosophers' attention to empirical facts.

Warren Schmaus, Illinois Institute of Technology
"Comte's revolution in epistemology"

Auguste Comte anticipated late-twentieth century attempts to ground a normative philosophy of science in the history of science. It would be unreasonable to expect this early attempt to suggest solutions to the problems faced by more recent normative naturalists. To appreciate the significance of his contribution to philosophy, it would be more instructive to compare him to his contemporaries rather than ours.

Theories of knowledge were inextricably bound up with philosophical psychologies in both the continental rationalist and British empiricist traditions. Even Kant had sought a grounding for mathematics and the sciences in the structure of conscious thought. In Comte's France, the eclectic spiritualists carried on this individualistic, mentalistic tradition in epistemology. Victor Cousin, the leader of this dominant school of academic philosophy, sought a foundation for philosophy and thus for all knowledge in an introspective psychology. Comte argued that this epistemological tradition had achieved no consensus or lasting results. Like Hume, he held that introspection could reveal only the results of our mental activity, not the activity itself, the study of which belonged to physiology. At best, introspection could reveal facts about only a single, presumably healthy, adult human mind. For Comte, the study of our collective intellectual development provided a broader empirical basis for a theory of knowledge. As he regarded mathematics and the sciences as our highest intellectual achievements, the history of the methods of these disciplines, summarized in the three-state law, provided the starting point for his philosophy.

It would be easy to criticize Comte for failing to ground a normative philosophy of science in the history of science. However, his predecessors who would base epistemology on a philosophical psychology could equally be charged with the attempting to derive prescriptions from descriptions. The distinction between normative and descriptive inquiries was not clear to philosophers at first. It is only implicit in Mill's criticism that Comte had provided only a logic of discovery and not one of justification. One could argue that the fallacy of naturalism was thrown into relief by the shift of philosophers' attention from the individual mind to our collective intellectual development. Unlike introspection, which is private, the history of science provided a common ground for philosophical discussion. Whewell, for instance, drew very different normative lessons from history than Comte did. Spencer, Huxley, Flint, and Renouvier also drew on history in their criticisms of Comte. In his critique, Renouvier made an explicit distinction between normative and descriptive theories. But Renouvier had to agree with Comte that science is the product not of some isolated Cartesian genius who has discovered the correct rules of method, but of communities of interacting researchers who have experimented with different ideas, methods, and epistemic norms over the course of history. Renouvier then turned his attention correcting Comte with regard to the social conditions that make this possible.

Laurent Clauzade, Université de Caen Basse-Normandie
"In defense of 'historical epistemology': Comte and Whewell on metaphysics"

Our aim is to study Whewell's criticism of Comte's rejection of metaphysics in relation to the defense of historical epistemology. Along with Comte's condemnation of the inquiry into causes, this rejection is the main topic of Whewell's examination of the positive philosophy: it is well known that these issues are crucial to understanding the differences between the two systems. Our thesis is that what is at stake here is the legitimization of what we call today historical epistemology.

According to Georges Canguilhem, historical epistemology rests on two main theses: 1. A theory of knowledge must be founded on the study of actual scientific practice ("*les actes mêmes du savoir*"). 2. Any study of actual scientific practice is necessarily historical. Concerning the first thesis, Comte and Whewell largely agree that the philosophy of the sciences ("*la philosophie des sciences*" in French) "ought to be based on a survey of the truths which have been discovered" (*History of the Inductive Sciences*, 3rd ed., 1857, Preface, p. 8).

The two philosophers also roughly agree about the second thesis, but they deeply differ in its defense. For Comte, "it is true that a science cannot be completely understood without a knowledge of how it arose" (A. Comte, *Cours de philosophie positive*, 2nd l., tr. H. Martineau, vol. 1, p.43). This widely known quotation implies two main elements of the positive philosophy: on the one hand the three stages law, which describes the theoretical order of the development of the sciences, and, on the other hand, sociology, upon which historical knowledge depends. So that we can say without exaggeration that Comte's defense of thesis 2 is "hyperbolic" and involves the whole Comtian system.

Whewell's defense is quite different. It is based on the assumption that the law of the three stages is absolutely false because metaphysical discussions about ideas, together with the study of facts, are an essential part of scientific discovery. However, putting forward evidence of metaphysical discussions is not only an argument against the law of the three stages; it is also the best way to defend historicity in epistemology. Past controversies are "a necessary part of the inductive movement" that a non-historical epistemology could not explain. Whewell's account of discovery through a process of metaphysical elucidation may be a better argument in favour of thesis 2 than Comte's hyperbolic defense.

Michel Bourdeau, Université Paris 1
"Two conflicting ideas upon the nature and the goals of man's action upon social phenomena"

Hayek and Comte both give much importance to the notion of natural order; they both have a theory about the limits put upon our power to modify the natural course of events but, while Comte thinks that our power grows with the complexity of phenomena and that, social phenomena being the more complex ones, it is where our power is maximal, Hayek thinks that the very complexity of those phenomena is a good reason to abstain from acting. I will study the objections Hayek raises against his adversary in order to see if they really affect Comte's position.

Vincent Guillin, Université du Québec à Montréal
"The sociological rule: Positive polity and its epistemological foundations"

Although later-day positivism has been sanitized as a pure philosophy of science, whose endorsement of the fact/value distinction was considered a protection against ideological ravings, Auguste Comte's "positive philosophy" was through and through a political endeavour. As his early writings of the mid 1820s show, Comte first conceived it as the only proper theoretical answer to the practical issues faced by post-revolutionary societies. And as the publication of the four volumes of the *Système de politique positive* between 1851 and 1854 demonstrates, Comte's mature achievements mostly focused on the political impact of "positive philosophy."

Now, what distinguished Comte's project from the other political philosophies available at the time was its emphasis on the key role scientific knowledge had to play in the reorganization of society, most notably through the elaboration of a scientific understanding of social phenomena – what Comte first called "social physics" and later "sociology." Inspired by the examples of the various natural sciences, sociology was conceived by Comte as a systematic inquiry that would eventually lead to the formulation of general laws governing the statics and dynamics of societies, i.e. the laws governing the structural coexistence of the various social elements and those of their historical development. Thanks to this knowledge, Comte argued, it would be possible to provide modern societies with a goal and with the adequate means of achieving it.

In my paper, I would like to elucidate how Comte had articulated the theoretical and practical elements of his own "positive philosophy," conceived as a "scientific polity," through a minute appraisal of Comte's reflections on the various sorts of guidance a deterministic knowledge of society formulated by way of social laws can offer political rulers. For, it is not exactly clear what would be the proper scope and import of political interventions in a social world such as the one described in Comte's sociology. In other words, I would like to define more clearly, both in his *Plan des travaux scientifiques nécessaires pour réorganiser la société* (1824) and the *Cours de philosophie positive* (1830-1842), the political function and epistemological nature of the "sociological rule" advocated by Comte.

Session III.4 Aristotle

Phil Corkum, University of Alberta
"Aristotle on quantification"

The relation between Aristotelian demonstrative science and the syllogistic, on the standard interpretation, is that of an axiomatic system to its underlying logic. In an axiomatic system, theorems are established as true by deriving them from other propositions, axioms or theorems, whose truth has already been established or, in the case of axioms, accepted without derivation. Such a system relies on an underlying logic or system of inferential reasoning, so to justify the derivation process. On this interpretation of Aristotelian science, then, we grasp axiomatic truths through induction from experience, and employ the syllogistic to derive scientific theorems from these axioms. The syllogistic accordingly is represented as what is by our lights a paradigmatic logic, a natural deduction system: the validity of complex arguments are established by the step-wise application of small set of intuitive valid rules of inference.

I shall argue that this a misleading interpretative framework. The syllogistic is something sui generis: by our lights, it is neither clearly a logic, nor clearly a theory, but rather exhibits certain characteristic marks of logics and certain characteristic marks of theories. Just as some aspects of the syllogistic fruitfully may be represented as a natural deduction system, so too we can learn from its representation as a theory. In particular, I shall argue in this paper that the syllogistic fruitfully may be seen as a generalized quantifier theory.

The paper comes in three parts. In the first part, I introduce Aristotle's quantifiers and contrast them with the standard quantifiers of modern logic, ∀ and ∃. Here I shall discuss the interrelations among Aristotelian quantifiers which define the traditional square of opposition, and contrast this with the modern square of opposition, defined by the interrelations among the standard modern quantifiers. Several significant differences between these two squares will emerge. For example, the universal affirmative Aristotelian quantifier possesses, and ∀ lacks, existential import. And there are textual reasons to doubt that we can represent Aristotelian negation through the modern method of employing complementation.

In the second section, I present an Aristotelian theory of quantification and draw on contemporary general quantifier theory so to discuss some distinctive features of Aristotelian quantifiers. For example, the Aristotelian quantifiers are montonic, domain independent, conservative and topic-neutral. I shall argue that the differences between the Aristotelian and modern quantifiers noted in the previous section can be explained by appeal to these features: for example, since the Aristotelian quantifiers are conservative, domain independent and topic-neutral, Aristotelian negation can be characterized without the need to employ complementation.

Finally, in a brief conclusion to the paper, I return to the role of the syllogistic in Aristotelian demonstrative science. I shall argue that the relation between demonstrative science and the syllogistic is not that of an axiomatic system to its underlying logic. Rather, syllogistic theory is a generalization of demonstrative science. As such, demonstrative science may also be viewed as a restricted general quantifier theory.

Richard Dewitt, Fairfield University

"Does Aristotle say an object that weighs twice as much falls twice as fast? (Hint: No)"

Galileo famously criticized Aristotle for believing that an object that weighs twice as much will fall twice as fast, and to this day this belief continues to be (very) often attributed to Aristotle. But did Aristotle hold such a belief? The answer appears to be yes. Consider, for example, this passage:

If a certain weight move a certain distance in a certain time, a greater weight will move the same distance in a shorter time, and the proportion which the weights bear to one another, the times too will bear to one another, e.g., if the half weight cover the distance in x, the whole weight will cover it in x/2. (*De Caelo* I,vi; Loeb edition.)

In spite of the usual interpretation of such passages, my main contention is that there is little question that Aristotle held no such belief. An example might help sow some doubt about the usual interpretation of such passages.

As far as we know, Euclid was the first to give what we would call an operational definition of weight or, as I will hereafter refer to it in ancient contexts, heaviness. In particular, Euclid characterizes heaviness in terms of how much an object would displace a balance scale. Suppose we have two pieces of lead that in a non falling context displace a balance scale equally. We would thus say, in the non falling context and using Euclid's characterization, they are of equal heaviness.Now consider a context in which the pieces of lead are dropped from different heights. If dropped from suitable heights, the one will cover the final 10 meters of its fall in half the time the other covers that same 10 meters. Moreover, the one covering the distance in half the time will displace a balance scale roughly twice as much as the other. (We would have to imagine a balance scale suitably modified to gauge the influence of falling objects, but that is easy to do.) Thus the one object has not exactly, but quite close to, twice the heaviness and has covered the distance in half the time. In short, in this scenario Aristotle's description that an object twice as heavy falls twice as fast seems, at least to the limits of what could be measured without modern instrumentation, quite correct.

I do not want to put too much emphasis on this example, but it does serve to illustrate substantial differences between earlier conceptions of heaviness and our conceptions of weight. And the example illustrates that we might be well served to do a more careful analysis of Aristotle's conception of heaviness and of his claims involving the relationship between heaviness and rates of fall. One upshot of my analysis is that heaviness is not comparable to weight, and in terms of anything resembling a modern conception of weight, there is little question that Aristotle did not believe, nor did he say, that an object that weighs twice as much will fall twice as fast.

Janine Gühler, University of St Andrews

"Aristotle's way of abstracting"

According to Aristotle, mathematical objects are gained by abstraction from physical objects. Mathematical properties in physical objects are discovered but whatever holds the status of an object in mathematics is created by abstraction. Although we talk of mathematical objects, mathematics is concerned with physical objects in view of the fact that they are a quantity. Mathematics is a science of certain properties of physical objects. According to Aristotle, substances, such as Socrates, exist separately but mathematical objects depend on physical objects. Mathematicians simply treat them as if they were independent from physical objects without questioning whether they exist or not. The postulate of being an 'object' is an auxiliary without any ontological commitment. In the process of abstraction from physical objects to mathematical objects, motion and material of the physical object are ignored but mathematics is still dealing with physical objects. Aristotle uses the qua-operator to signify abstraction. A property X is abstracted from an object Y if we examine Y qua X, in other words, Y in the respect that Y is X. The greenhouse of the Eden project in Cornwall, for example, resembles the structure of a fullerene (C60). The material of the greenhouse is subtracted and what is left is a geometrical structure.

Opposed to geometry the case of arithmetic is more difficult. A group of sheep, for example, equates a certain number, say 7. The fact that sheep are made of flesh and bones is subtracted. As soon as we talk of a number of things, we already presuppose something common between these things, namely that they refer to the same unit (here: sheep). Frege holds that if we take a group of 'counting blocks' and abstract whatever distinguishes them then we cannot count them anymore. The blocks become identical and there is only one block left. What cannot be distinguished, cannot be counted. Frege's idea behind this claims is that if we don't already know what it is that we want to count then we don't know where to stop subtracting properties that distinguish objects from one another. If we have a group of animals, say sheep and goats, which we want to count by species, we have to know what the conceptual difference between these animals is. If we already know what the concept of these are then we don't need abstraction anymore. I hold that Aristotle's Way of abstraction is not at risk here. For Aristotle mathematics, geometry and arithmetic, is based on units. These units are recognised by knowledge. If we recognise a unit, e.g. a sheep, then we know what the characteristics of this unit are. Abstraction is an adjustment of individual things that we examine to what we already know about similar individuals. The knowledge from previous experience with similar individuals is, according to Aristotle, formed by the frequent sight of them and is built in a step-by-step process depending on the capacity of knowledge. In summary, I'll show that Frege's criticism is no harm for Aristotle.

Mark Sentesy, DePaul University
“The compatibility of dunamis and energia”

It is widely believed the potency and actuality are opposites in Aristotle. This essay examines three forms of this opposition hypothesis—the Actualization, Privation, and Modal hypotheses—and argues that they are untenable, and that instead, it is necessary to argue for the compatibility of *dynamis* and *energeia*. This argument is supported first by critical examination of the texts taken to support the Opposition Hypotheses, and then, by a discussion of Aristotle’s argument against the Megarians in *Metaphysics* IX, which shows that if potency and actuality are incompatible, movement is self-contradictory and therefore impossible. The discussion closes with a brief discussion of whether it is circular of Aristotle to define movement through *dynamis* and *energeia*, and then in *Metaphysics* IX, to work out what *dynamis* and *energeia* are starting from movement.

Friday, 22 June

Parallel Session IV

Session IV.1 Symposium: Robert Merton and the philosophy of science

More than a century after his birth, and nearly 75 years after issuing his eponymous thesis on the rise of science in early modern Protestant England, the contributions of Robert K. Merton to sociology and history of science look to be foundational and robust. The relative fates of theoretical themes or particulars may wax and wane: yesterday’s Strong Programme gives way to today’s Analytic Sociology, and Merton’s approach lives on. Yet the heritage is clear and enduring: the very sociological analysis of science, and the sociologically-informed history of science, we owe to Merton. Where, if at all, does philosophy of science fit in this picture? Merton famously departs from Parsons’ penchant for grand theoretical schemes, but his theoretical deflationism and other methodological proposals form a well-known canon of postwar sociological theory; none of this happened in a philosophical vacuum. From a historical perspective, relative to philosophy of science, at least two issues emerge: 1. What relation does Merton’s sociology and history of science have to the philosophy of science, in his historical context and the aftermath? 2. What is the relationship of his methodological perspective to philosophical reflections on method in the social sciences?

Further, in the HOPOS context, we may ask what in the Mertonian heritage may pose a challenge or represent an opportunity to the philosophy of science and our understanding of its history.

Part of the story is situating Merton in philosophical context, and here the Logical Empiricists play a significant role as *contemporaries*, while much that is Mertonian is *born of* his rich immersion in the history of social thought. Durkheim and Simmel are only two key influences.

Running in the other direction, we can map the philosophy of science’s historical trajectory relative to Mertonian analyses, notably, of scientific values in social context, social norms of science, and method and explanation in the social sciences.

Our close analyses of Merton’s thought in context underscore his avoidance of the magnificent conceptual gesture, and numerous subsequent critiques: the goals are advertised as modest, gaps and sketches abound, and foundations don’t always stand up to inspection. For all that, Merton's rich range of methodological proposals and science studies innovations leave an important legacy to philosophy of science: a contextually-significant picture of sociological method, an engagement with ethics and policy orientation at the core of science studies, a historically fundamental approach to viewing science through social lenses, and a package of tools, modest or otherwise, for understanding the discipline's own development over time.

Saul Fisher, Mercy College
"Merton and Nagel on the functional explanation"

Working at rather close proximity—at some 25 meters distance over four decades—Robert Merton and Ernest Nagel approached functional analysis in social explanation from very different starting points. In his landmark 'Latent and Manifest Functions' (1949), Merton surveys the literature, summons the history of functionalism and a wide variety of empirical studies, and crafts a 'paradigm' of functional analysis which poses take-home assignments to the reader. Nagel (1956), for his part, offers a formal assessment of Merton's discussion on a classically Logical Empiricist model, taking his account of functional analysis in biology as the base model.

The pocket literature on the Merton-Nagel discussion tends to see Nagel as rejecting Merton's model. While there *is* a problem that Nagel locates in Merton's view, it may be fairly stated that Nagel's is an extraordinarily *friendly* account that takes Merton's view to lay out the fundamental direction for applying his own Logical Empiricist approach to functional analysis in the social sciences. Moreover, Nagel recognizes as significant key facets of Merton's view: his expansion on traditional functionalism to include an account of dysfunction, and the importance of homeostasis or equilibrium to the Merton picture.

The main 'complaint' Nagel registers is that gaps in Merton's story make it unclear how his account should meet the standards of the Nagel formalization, and the principal difficulty here is that we don't get a picture as to how to specify state coordinates under general laws. This difficulty—a nomological strictures problem—cannot undermine Merton's approach, though, for two interrelated reasons. First, Merton famously does not think sociologists should be hunting down, or waiting around to discover, general laws. Second, Merton takes the biological model of functional analysis as an important influence (as Nagel notes) but outlines what he takes as important differences, including a causal holism that doesn't accommodate the Nagelian nomological picture.

Of greatest significance, perhaps, are Nagel's appreciations of Merton's systems-oriented approach, concern with functions as consequences (or utility-bearing features), and the context-sensitivity of such analysis. These are core elements of thinking about functional analysis that would come to dominate the discussion some thirty and forty years later, which are still largely unrecognized as pioneered by Merton and Nagel.

Stephen Turner, University of South Florida
"Robert Merton and Dorothy Emmet: Deflated functionalism and structuralism"

Robert Merton's writings tend to obscure his philosophical sources. Indeed, he had a habit of claiming support from sources that were opposed to him. He had no explicitly acknowledged philosophical sources, and was estranged from and consciously distanced himself from the philosophical language of his rivals, notably Talcott Parsons, even where he employed it. The central mystery of his career, commented on by Jon Elster and many sociologists, was the sense in which he was a "functionalist." Late in his career, after functionalism had gone out of fashion and he was attempting to reconstruct his legacy, he argued that he was a structuralist. The key issue with his "functionalism" had to do with the question of what his employment of functionalist language meant. Ernest Nagel was frustrated with Merton's unwillingness to go beyond preliminary assertions about the functions of various social institutions to a full fledged theory, without which "functional" meant only "having consequences."

Dorothy Emmet, in two books, one of which was based on extensive personal contact with Merton and Columbia sociology, provides the closest thing we have to a philosophical defense of Merton. It features a deflationary account of functionalism which dispenses with the idea of general teleological ends. What it replaces it with is close to Merton's self-conception: an account of "structures" which have various consequences and which are maintained because, on Emmet's account, people accept the general social order which the consequences help maintain, and this deflated "unconscious teleology" suffices to explain the maintenance of structures.

Gary Hardcastle, Bloomsburg University
"Merton, ethos, and sentiment"

Despite the sustained and often passionate attention paid to it since its publication in 1938, in *Science, Technology and Society in Seventeenth Century England*, only in the past two decades (and only following efforts of Abraham (1983), Shapin (1988), and I. B. Cohen (1990), among others) has the Merton Thesis come to be understood and appreciated with anything like the sensitivity and attention to detail Merton invested in its articulation and defense.

A notable, if overdue, benefit of this newfound understanding is Merton's placement on the edge of Harvard's enormously influential "Pareto Circle," the group devoted to understanding and applying the theories of Vilfredo Pareto. (Pareto's ideas were introduced to Merton in seminar by the Harvard historian L. J. Henderson, one of the Circle's most prominent members, in 1932.) As a further consequence, there is a new recognition, not only of the central role that Pareto-style "sentiments"—"socially patterned psychic structures that lie behind… a more or less coherent body of cultural expressions" (Shapin, 1988)—played in the Merton Thesis, but of the methodology adapted from Pareto and his followers to limn such sentiments from what actors say and do. Specifically, Paretan-style sentiments fuel the Pietist and

Protestant ethos that, Merton claimed, contributed to the "enhanced cultivation" of 17th century science. "Sentiments," Shapin writes, "make Merton's system go."

Yet Paretan sentiments are absent from Merton's familiar articulation of a scientific ethos a mere four years later, in 1942. There, Merton repeatedly (and variously) claims that the mores of science "derive from the goals and methods of science," that is, from the aim to extend "certified knowledge" by means of "empirically confirmed and logically consistent predictions" (Merton, 1942). This raises a number of questions: Is this a *genuine* change in Merton's theoretical framework, and, specifically, in Merton's understanding of what counts as appropriate sociological explanation? If so, what motivated such a change?

Session IV.2 Nineteenth-century German scientific epistemology

Liesbet de Kock, Ghent University
"Im Anfang war die Tat: Helmholtz and the problem of externality in perception"

This paper concerns Hermann von Helmholtz's viewpoint on human vision, and addresses the problem of how we escape the world of our nerve sensations and gain access to the realm of external reality (Helmholtz, 1896 [1855; 1878]). More specifically, it (1) offers an analysis of the philosophical foundations of the problem of externality in Helmholtz's psychophysiological optics, and (2) demonstrates the way in which Helmholtz's (philosophical and psychophysiological) treatment of this problem implies a principled decision with regard to the nature of the epistemic subject as an active and embodied being, constituting external reality through an infinite series of actual encounters.

It will be shown how the problem of externality (or the question of the origin of a Not-I in perception (Helmholtz, 1896 [1878])) emerges from the combination of (i) Helmholtz's rejection of the metaphysical assumption of pre-established harmony, (ii) his partial rejection of the Kantian apriorism concerning the intuition of space (Helmholtz, 1883 [1878]; Hatfield, 1990), (iii) and his objections towards Oswald Hering's nativism (see amongst others Helmholtz, 1896 [1868]; 1910 [1866]; Turner, 1994; Heidelberger, 1999). Whereas the problem at hand is rooted principally in the assumption of a radical fissure between mind and matter (or discursivity and nature), Helmholtz is unwilling (thereby motivated by his rather strict empiricist stance) to ascribe the consciousness of a Not-I either to an unanalyzable intuition (Helmholtz, 1896 [1892]) or to an inborn capacity.

In his treatment of this problem, Helmholtz starts by assuming that the only thing of which we can be immediately aware, is the consciousness of our own free will in initiating movement (Helmholtz, 1896 [1878]; see also Heidelberger, 1994). This originary state is internally differentiated when an objectum appears. The latter is not seen as a positive entity, but as a negation of that which a subject can produce by its own free will. The experienced covariation between bodily movement and sensory modification in experimentation is the ultimate condition under which subjectively felt sensations can be objectified, viz. determined as the effect of a stable, external cause, indifferent towards the subject's voluntary acts (see also McDonald, 2003). Actuality thus first comes to be represented to perceptual consciousness under the form of an obstructive force [uns entgegentretenden Macht] (Helmholtz, 1896 [1878], pp. 241). Consequently, Helmholtz recasts this abstract idea in psychophysiological terms, operationalizing the consciousness of free will as 'Muskelgefühl' or 'Innervationsgefühl' (['feeling of muscles' and 'feeling of innervation'] Helmholtz, 1910 [1866], pp. 204) in initiating movement, and establishing it as a precondition for the internal external distinction in perception. Helmholtz's treatment of the problem of externality implies that an epistemological account of objectivity and objectification cannot be adequately given without a consideration of what constitutes subjectivity. Moreover, Helmholtz's account of the emergence of externality in perception as a result of the felt opposition between freedom and constraint, implies a view of epistemic subjectivity that provides a middle way between the passive subject of sensationalism, and the excessively active subject of post-Kantian idealism (Helmholtz, 1910 [1866]).

Scott Edgar, Yale University
"Continuity and the constitution of individuals in Hermann Cohen's *Prinzip der Infinitesimal-Methode*"

In his 1883 *Prinzip der Infinitesimal-Methode und seine Geschichte*, the Marburg neo-Kantian Hermann Cohen developed an idea he had first suggested over a decade earlier: namely, that there are a priori principles "latent" in our theories of mathematical natural science that somehow "constitute" the objects of science. Thus he earlier called those principles "constitutive." In this paper, I offer an interpretation of Cohen's constitutive a priori in the *Prinzip der Infinitesimal-Methode* with an eye to articulating a central question about Cohen's views in that work, and attempting to answer that question. The question is: Why for Cohen is the mathematical concept of continuity a necessary presupposition of our representations of *individual* objects?

The significance of the question is revealed by considering what Cohen thinks objecthood in physics consists in. He defends the view that the very objecthood of the objects in physics depends on the mathematical structures we use to represent them. (He claims, for example, that if Kepler had not had mathematical representations of conic sections, then neither the planetary orbits nor even the planets themselves would be *objects*.) At the same time, Cohen is perfectly aware that this cannot be a complete account of the constitution of physical objects, because mathematical structures, on their own, are ideal. That is, on his view, physical objecthood requires a kind of concrete reality that mathematical structures on their own do not have. Crucially, Cohen identifies the ideality of mathematical structures with the fact that they are relational. He thus argues that what is required to explain our representation of real objects (as opposed to ideal structures) is an account of how we represent individuals. Those individuals, he suggests, will provide the concrete reality required for physical objecthood.

Here, finally, is the significance of the mathematical concept of continuity for Cohen. He argues that those mathematical concepts are necessary for our representations of individuals, and consequently necessary for our representations of physical objects. His argument turns on the Kantian distinction between extensive and intensive magnitudes. The former are magnitudes composed of homogenous units, but Cohen argues that the magnitudes of those units themselves can only ever be defined relationally. Consequently, extensive magnitudes cannot explain our representation of fundamentally non-relational individuals. In contrast, intensive magnitudes are magnitudes that come in continuous degrees of intensity, and thus they presuppose the concept of continuity. Further, Cohen thinks they *can* explain our representations of fundamentally non-relational individuals. I aim to articulate fully, and to assess, his account of why only intensive, continuous magnitudes can explain our representations of individuals, and of how they do that.

The paper will be of interest as an interpretation of an under-researched text in the history of the philosophy of mathematics. However, its principal interest will be as a detailed case study of the neo-Kantian constitutive a priori. That is, it will be an account of how one neo-Kantian, at one point in his career, articulated a theory of the constitution of objects in physics.

Christain Damböck, University of Vienna
“Critical remarks on neo-Kantian interpretations of Carnap and Kuhn”

Recent interpretations of Carnap and Kuhn claim that both of these philosophers have some neo-Kantian background. Essentially, the idea of most of these interpretations is that both the philosophies of Carnap and Kuhn can be seen as instances of a philosophy of the relativized a priori. In the case of Carnap we may find a relativized a priori in the context of his phenomenalistic constitution system of the *Aufbau* and in the context of the P-rules of the *Logical Syntax*. In the case of Kuhn the so-called paradigms that form different historical instances of a theory may be seen as examples for relatively a priori theories. However, the present paper will argue that this analogy in both cases is remarkably ill founded. The relativized a priori as it was considered by both the Marburg School and the Southwest-German School of Neo-Kantianism is in both cases inevitably linked to a deeply foundationalist understanding of the nature of the sciences. Philosophers like Heinrich Rickert (as a representative of the Southwest-German School) and Ernst Cassirer (as a member of the Marburg School) propagated the vision of a science that is *relative only insofar as the amount of available empirical data is limited*. However, given a particular amount E of empirical data the scientists inevitably must arrive at the same scientific concepts, because these concepts follow with necessity from E (by means of some sort of traditional “logic”). In sharp contrast to this neither Rudolf Carnap nor Thomas Kuhn ever may have claimed that a particular amount E of empirical data can be represented by *just one* (logically possible) theory. Such a claim (though possibly not explicitly rejected) appears to be rather absurd, against the background of the *Aufbau* as well as the *Syntax* and the *Structure*. The neo-Kantian picture of the sciences, we may conclude, is *foundationalist*, in a sense that can be attributed neither to Carnap nor to Kuhn. On the other hand, there can be no doubt that there are important convergences between Carnap and Kuhn and *some* nineteenth Century philosophy. It is also quite obvious that both Carnap and Kuhn had *some* notion of a relativized a priori. The point is only that these kinds of “a priori” that were defended by Carnap and Kuhn are *much more relative* than the a priori that was defended by the neo-Kantians. At the end of my talk I will try to show that a better analogy to these kinds of extremely relativized a prioris may be found in nineteenth Century philosophers such as Adolf Trendelenburg and Wilhelm Dilthey.

Session IV.3 Philosophy of Experiment

Peter Anstey, University of Otago
“D’Alembert, the ‘Preliminary Discourse’, and the experimental philosophy”

This article argues that the Jean Le Rond d’Alembert’s ‘Preliminary Discourse’ to the *Encyclopédie* reveals a commitment, on d’Alembert’s part, to the experimental philosophy. This commitment is evident in its terminology, in its central methodological doctrines and in its deployment of the ideas of John Locke and Francis Bacon. In contrast to Jonathan Israel who downplays the influence of Locke on d’Alembert, it is argued that d’Alembert’s epistemology in the ‘Preliminary Discourse’ and elsewhere is thoroughly Lockean. In contrast to Thomas Hankins and Israel, it is argued that Francis Bacon’s classificatory scheme of knowledge is integral to the structure of d’Alembert’s preface and to the *Encyclopédie* itself. Finally, it is argued, *pace* Hankins, that d’Alembert’s ideal of a demonstrative natural philosophy is most likely derived from Locke and the example of Isaac Newton. The article concludes with an exploration of some of the implications of these claims for the historiography of the Enlightenment.

Madalina Giurgea, University of Ghent
“On the creative role of experimentation in Descartes’ study of colours”

Although the nature and evolution of Cartesian physics has been the subject of many debates, relatively little has been done so far to clarify the details of the way in which Descartes devised, constructed and used experiments. Even if there are significant studies of the status of hypotheses in Descartes’ works (see Blake 1929 and 1960, Garber 2000, Ariew 2011), they pay comparatively little attention to the process of experimentation as such. Therefore my aim is to bring into discussion/discus the particular way in which experiments act as problem-solving devices. The standard story is that, for Descartes, experiments function as illustration and have, therefore, a mere 'passive role'. My purpose in this paper is to challenge this account. I propose an alternative interpretation of the role that experiments play in the Cartesian natural philosophy by focusing on the reconstruction of the techniques of experimentation Descartes seems to have used in the explanation of colours. I claim that we do not have a hypothetico-deductive structure at work; experiments do not test predictions. They stand in a much more complex relation with Descartes' physics than usually assumed. Hence, studying the nature, function, structure and application of Descartes' experiments and the associated heuristic of the 'scientific discovery' sheds a new light on Descartes' doctrine, allowing a much less speculative reading of his physics.

Adopting the position stating that Descartes was less *a aprioristic* about the scientific method than usually thought (Galison 1984, Buchwald 2008) I will identify, on particular examples, some of the functions of Cartesian experiments. I will be particularly interested in a number of Cartesian experiments destined to bridge the gap between the visible and the 'invisible' world of particles of matter in motion. I will especially concentrate on Descartes study of the halo and the coronas around the flame from the ninth discourse of *Meteorology*.

The striking part of Descartes' study of colours is the fact that in order to settle the explanation of the phenomena, two methodological strategies are available. One is to manipulate the initial experimental setting in order to reproduce phenomena. The other is to use analogical reasoning and, starting from one phenomenal occurrence, to design a new experiment in order to extend the domain to related phenomena. The modifications of the experimental setting connect apparently dissimilar physical occurrences, as the halo around stars and coronas around the flame, under the same domain of investigation. I will show that these strategies allow Descartes to generate a body of knowledge about the meteorological phenomena by unifying the phenomena that shares a common explanation.

The same structure can be unearthed, I think, in other experiments of Descartes' *Meteorology*. It is a structure that demonstrates, I claim, the creative role of experimentation. By modifying the experimental setting and the field covered by the experiment, the process of experimentation plays a more productive role in the process of discovery that usually ascribed to Descartes.

Session IV.4 Gottfried Wilhelm Leibniz

Erik C. Banks, Wright State University

"The problem of extension in the philosophy of science (1700-1860)"

Leibniz's project for a 'construction of extension' was an ambitious attempt to undermine the extended space, time and matter of the seventeenth-century mechanical philosophy, in favor of what he called a deeper view of nature. In this view extension would be a well-founded phenomenon, founded on something deeper, of which it is simply a kind of representation. There is not only something besides extension, but something prior to extension, he famously insisted. This project for a construction of extended magnitudes was developed further in the 19th century by the philosopher Herbart and the mathematicians Grassmann and Riemann. Riemann proposed to develop extended manifolds, and Grassmann algebraic extensions, from scratch, without assuming a prior extended drafting board on which to do their constructions. They both claim explicitly that this is the goal of their new conceptions of extension. I wish to look at the contributions of each on this specific topic of the nature and origin (and potential dispensibility) of extension, and to compare the results with the original program of Leibniz. Finally, I wish to ask why this project and its development, considering its importance, has been neglected by philosophers and historians of science. Does the sheer success of these mathematicians overshadow their conceptual motivations and philosophical aims, or are these extra-mathematical philosophical programs considered mere intuitive ballast which can be kicked away in the process of formalization?

Douglas Bertrand Marshall, University of Minnesota

"Leibniz: Geometry, physics, and idealism"

Leibniz holds that nothing in nature strictly corresponds to any geometric curve or surface. Yet on Leibniz's view, physicists are usually able to ignore any such lack of correspondence and to investigate nature using geometric representations. The primary goal of this essay is to elucidate Leibniz's explanation of how physicists are able to investigate nature geometrically, focussing on two of his claims: (i) there can be things in nature which approximate geometric objects to within any given margin of error; (ii) the truths of geometry state laws by which the phenomena of nature are governed. A corollary of Leibniz's explanation is that physical bodies do have boundaries with which geometric surfaces can be compared to very high levels of precision. I argue that the existence of these physical boundaries is mind-independent to such an extent as to pose a significant challenge to idealist interpretations of Leibniz.

Kenneth Pearce, University of Southern California

"Leibniz on phenomenalism, mechanism, and the great chain of being"

Nicholas Jolley has argued that Leibniz "never did more than flirt with phenomenalism" because actually endorsing such a theory would undermine Leibniz's mechanism. I argue, on the contrary that Leibniz has a phenomenalistic theory which is capable of providing a foundation for mechanistic science. The existence and attributes of bodies, according to Leibniz, arise from the inherent limitations of our perceptual capacities. This does not, however, make the material world an illusion. The concept of body can be used in accurate descriptions of the world, and description in terms of this concept is more perspicuous as to fundamental reality than any other sort of description to which human empirical science can aspire. It is for this reason that humans ought to aspire to mechanical explanations for all phenomena.

Leibniz's account of bodies includes two theses which are supposed to be inconsistent with phenomenalism. These are the aggregate thesis, which states that bodies are aggregates of monads, and the confused perception thesis, which states that sensory perceptions are confused perceptions of monads. I argue that these two theses are in fact components of a consistent and genuinely phenomenalistic theory of bodies.

The aggregate thesis is thought to be inconsistent with phenomenalism because the monads exist independent of their being perceived. To say that bodies are aggregates of monads is therefore apparently to say that bodies exist independent of their being perceived. Leibniz would not, however, allow this inference for, according to Leibniz, "nothing is truly one being if it is not truly one being" (WF 124), but the unity of an aggregate comes from what is "added [to the monads] by perception alone, by virtue of the very fact that they are perceived at the same time" (AG 203). Being requires unity, and the unity of a body comes from its constituent monads being co-perceived by some mind. Its being is therefore dependent on its being perceived. The confused perception thesis is thought to be inconsistent with phenomenalism because the it takes sensory perception to be perception of a reality which exists independent of its being perceived, namely, the monads. However, Leibniz's dictum about the convertibility of being and unity has the consequence that nothing can be identified with a plurality until that plurality is somehow unified and, in the case of a being by aggregation, such as a body, this unification can only be due to the activity of a perceiver. Leibniz's actual view is that the plurality of monads from which a given body arises is unified into an aggregate when that plurality is united in a perceiver under the concept of body.

Parallel Session V

Session V.1 Symposium: Transforming methods: Late Aristotelian roots of modern approaches to medicine, natural philosophy and civil service

A seventeenth century philosopher's claim to fame rested on his possession of a new method. Both René Descartes and Thomas Hobbes hailed William Harvey as a great innovator; Hobbes even characterized him as having revolutionized the biological sciences just as Copernicus had revolutionized astronomy and Galileo physics. Following on the heels of Renaissance debates about the proper method for scientia, the modern hallmark of innovation was the replacement of the Aristotelian demonstrative syllogism with a new method for scientific demonstration. Francis Bacon thus announces by the very title of his *Novum Organum* that his new logic will make the old Aristotelian organon redundant. Whatever other elements early modern philosophers retained from their predecessors, it has long been presumed that their true innovations lay in the rejection of syllogisms in favor of experimental/ inductive methodologies, on the one hand, and demonstrations modeled after mathematics, on the other. Despite the fact that early moderns often appropriated late Scholsatic metaphysical and physical theories and concepts, the distinct methods they applied to them had a transformative effect – this much is clear.

What is less clear are the ways in which the new methods themselves grow out of and transform methods found within the Aristotelian tradition(s) of the Renaissance. Attempts to link the scientific methods of early moderns directly to late Scholastic Aristotelian theories of scientific demonstration, as found for example in William Wallace's work on Galileo Galilei's early writings in relation to Jesuit teachings, have since been questioned. Nonetheless, progress has been made in specific domains, most notably in understanding the relationship between the kinds of geometrical demonstrations developed within the Aristotelian Mechanica tradition and early modern mechanics. This symposium aims to advance similar connections within the domains of early modern medicine, natural philosophy and politics. One of the advantages of the approach taken here is to examine the connection between formal statements of method and practical engagement with specific problems. We begin with Peter Distelzweig's paper on "William Harvey's Aristotelian Experimentalism" in which he traces the influence of Harvey′s Aristotelian Professor, Fabricius, on Harvey's methods. This enables Distelzweig to reunite two traditionally opposed Harveys: Harvey the Aristotelian and Harvey the experimentalist. Nathan Smith's paper on "Simple Natures and Scientific Explanation in Bacon and Descartes" explores both common ground and conflicts between the methods of Bacon and Descartes, through the lens of their respective transformations of the Scholastic notion of a simple nature. He thus challenges the traditional divide between Bacon the empiricist and Descartes the rationalist, providing us with a more nuanced view of how their approaches to scientific explanation differ. Finally, Helen Hattab's paper "Method and Mathematical Order from Zabarella to Hobbes" examines the universal method by which Hobbes claims to construct the first scientia civilis. She argues that while it is a mistake to identify his uses of analysis and synthesis with the two phases of Zabarella's regressus, innovative features of Hobbes' method can be traced back to Zabarella's discussion of mathematical order via subsequent logicians

Peter Distelzweig, University of Pittsburgh
"William Harvey's Aristotelian experimentalism"

William Harvey has long been hailed as an important early 17th century proponent of experimental methods. Indeed, even in his own lifetime, Harvey's explicit emphasis on and successful use of observation and vivisection were noted and lauded by many. This feature of Harvey's work is often singled out as characteristically modern, earning him a place among the canonical figures of the "Scientific Revolution". However, beginning perhaps with Walter Pagel′s early work in the 1960s historians have also come to acknowledge that Harvey was a self-conscious Aristotelian. This has created as yet unresolved tensions in our understanding of Harvey and his work. For example, the relationship between Harvey's experimentalism and Aristotelianism has not been adequately articulated—or at least so I argue in this paper.

In order to understand Harvey's experimental method, I suggest we look first at the work of Hieronymus Fabricius ab Aquapendente (1537-1619), the longtime professor of anatomy at Padua. Fabricius was Harvey's teacher at Padua and a continuing influence on his work in the decades after completing his medical studies in 1602. An appreciation of Fabricius' "Aristotle Project"—as historian Andrew Cunningham has called it—directs our attention particularly to the wide range of animals Harvey vivisects and why he does so. Drawing on Harvey's lecture notes from (roughly) the decade leading up to the publication of De motu cordis in 1628, I argue that this aspect (and others) of the highly successful vivisectional method found in the De motu cordis is motivated and guided by Harvey's view (shared with Fabricius) that the goal of anatomical research is Aristotelian scientia of the parts of animals.

I conclude by arguing that such a unified interpretation of Harvey's Aristotelianism and experimentalism is to be preferred over what I call two-Harvey interpretations—interpretations which in one way or another see a conflict, tension or disconnect between Harvey the experimentalist and Harvey the Aristotelian. Such interpretations, I suggest, depend on employing problematic dichotomies such as modern vs. Aristotelian, experimental vs. a priori, or observational vs. theoretical.

Nathan Smith, Houston Community College
"Simple natures and scientific explanation in Bacon and Descartes"

A textbook presentation of Francis Bacon and René Descartes would label the former an empiricist and the latter a rationalist, suggesting very different scientific methods, one inductive the other deductive. However, these characterizations are misleading, not only because Descartes engaged in experimental research or Bacon imported rationalist elements in his metaphysics, but also because these words are not apt to describe Cartesian and Baconian methods. In order to develop a more illuminating basis for comparing these two seminal philosophers, I will examine the role of simple natures in their natural philosophical methods.

Simple natures provide a useful lens through which to understand the methods of Bacon and Descartes. First, the use of such terminology suggests a reference to a classical notion, developed by late sixteenth century commentaries on Aristotle, where simple natures are taken to be quiddities or essences. The canonical view is provided by Francisco de Toledo's commentary on the *De Anima* (1591), while a divergent puzzle is posed by the Coimbrian commentaries on Aristotle's *Physics* (1594). I will explain the issues presented in these texts and suggest why Bacon and Descartes proposed a central role for simple natures in their philosophies of nature.

Second, I will show that Descartes' Regulae demonstrates a positively Baconian method of discovery (ars inveniendi) when he turns to "imperfect" problems, containing some element of nature that cannot be entirely grasped by the intellect (and therefore requiring more than arithmetic and geometry). In particular, I examine his discussion of the nature of the magnet. If this account is referred to the *Principia Philosophiae*, where Descartes provides his full account of the nature of the magnet, it can be effectively compared with, for example, Bacon's explanation of the yellowness of gold or the heat of fire in the *Novum Organum*. I argue that the appropriate way to understand the common features of Baconian and Cartesian method are with reference to what Antonio Perez-Ramos has called —the maker's knowledge tradition. That is, both Descartes and Bacon conceive a given nature to be understood just in case its effects can be reproduced or fabricated by human ingenuity.

Third, attending to these examples more carefully leads to the realization of important differences between Bacon and Descartes that, in turn, enables a more nuanced appreciation of the distinction between Baconian and Cartesian methods. What we will discover is that, for Bacon, simple natures are real qualities that play a causal role in nature while, for Descartes, simple natures are irreducible, ideal categories on which natural scientific explanations are based. Furthermore, while Bacon is committed to the physical reproduction and material manipulation of natures, Descartes is satisfied with an ideal, logical, or geometrical reproduction of the given nature. This renewed appreciation of the difference between Baconian and Cartesian science is fruitful for our understanding of their influence on early modern scientific method.

Helen Hattab, University of Houston
"Method and mathematical order from Zabarella to Hobbes"

Since John Herman Randall's seminal article, there has been much discussion of Jacopo Zabarella's version of the regressus and its potential influence on scientific methods developed by early modern natural philosophers, most notably, Galileo Galilei, Thomas Hobbes and René Descartes. Unfortunately, the results have not been encouraging as fundamental differences between early modern scientific methods and the Zabarellian demonstrative proof known as the regressus revealed themselves. In this paper I propose that, instead, we pay more attention to other aspects of Zabarella's *De Methodis* and its immediate reception by subsequent logicians who wrote on method. To illustrate the fruitfulness of such further forays into Renaissance methods, I discuss a particular problem Zabarella raised regarding the order of Euclid's Elements and show that the way his views on method and order were taken up by Protestant logicians serves to explain key elements of Hobbes' method for attaining scientia simpliciter. Hobbes conceived this method as a universal one, applicable in every domain, from geometry to natural philosophy to politics. Indeed, Hobbes took himself to be the first philosopher to construct a civil science by employing this method.

Hobbes is most commonly read as advancing a version of the regressus in his treatment of the methods of analysis and synthesis in chapter six of *De Corpore*. I first show that this interpretation only gains some plausibility from the 1656 English translation of *De Corpore*, which confuses key distinctions Hobbes makes by its imprecise translations. Moreover, if one carefully examines Hobbes' examples of how analysis and synthesis function in attaining as much knowledge of the causes of things as possible, it is clear that there are fundamental differences from the resolutive and compositive proofs that formed part of the regressus. Nonetheless, apparently divergent features of Hobbes' method are not far removed from the ways in which Bartholomaeus Keckerman and Franco Burgersdijk develop Zabarella's claims about method and order. Hence one can situate Hobbes' methodological endeavors in a more or less continuous line of reflections on method originating in Zabarella's text. This example illustrates that while there may not be a direct connection between early modern and late Scholastic scientific methods, there may well be stepping stones that account for the shift in thinking that occurs between the late Scholastics and the early moderns.

Session V.2 Twentieth-century neo-Kantianism and the exact sciences

Thomas Oberdan, Clemson University
"Cassirer's response to Russell's *Principles of Mathematics*"

The past two decades have witnessed a growing interest in the influence of Marburg neo-Kantianism on the development of 20th Century analytic philosophy. Founded by Hermann Cohen and Paul Natorp, the Marburg School adapted Kant's transcendental project to ground the objectivity of recent developments in advanced theoretical science. Then, in 1910, Cohen's student Ernst Cassirer, radically transformed the aim and method of the Marburg project in his monumental Substance and Function, effectively abandoning the work of his teacher. In a recent work on Cassirer, Edward Skidelsky has emphasized the salient role of Bertrand Russell's *Principles of Mathematics* (1903) in the revolution in Marburg thought. Russell devoted an entire chapter of *Principles* to the re-orientation of the transcendental project developed in Cohen's *Princip der Infinitesimal-Methode* (1883). Cohen argued that the understanding of the differential calculus which emerged from recent physical applications provided the key to comprehending continuity as a law of the understanding, thus transforming Kant's transcendental project and stamping Marburg neo-Kantianism with a distinctive interpretation and a characteristic method. But in *The Principles* Russell offered a scathing critique of Cohen's argument, charging that Cohen's understanding of infinitesimals and limits was both internally flawed and inadequate to the task of re-vitalizing Kant's transcendental project. Thus Russell adroitly eviscerated the foundations Cohen had established for Marburg neo-Kantianism. At the same time, however, Russell's criticism triggered a reaction from Cohen's intellectual heir apparent, Ernst Cassirer. Astonishingly, Cassirer drew on *Russell*'s Principles for the tools and materials to form a new foundation for Marburg neo-Kantianism. Of course, the point of Russell's construction was to establish the logicist thesis that mathematics is nothing but logic. But Cassirer had no interest in logicism or the definition of numbers in terms of sets of sets. Instead, Cassirer argued that the logicist analysis of mathematics is merely an abstraction from the synthetic construction of empirical science and that modern logic provides the fundamental methodological tool for the reconception of the Kantian transcendental project. Indeed, the construction of mathematics from logic shows that the basic primitive of modern symbolic logic, the general relational form, epitomizes the productive synthesis which functions as the a priori source of the objectivity of the advanced theoretical sciences. Thus Cassirer effectively transformed Russell's achievement into an instrument for overcoming the deficiencies of Cohen's foundations, thereby re-interpretating the Kantian transcendental project, and re-establishing Marburg neo-Kantianism on an altogether new foundation. In addition, Cassirer also abandoned the key innovation of Russell's logicism, the definitions of cardinal numbers, and substituted instead the formalist approach to definition by axioms or implicit definition. This maneuver results in an epistemology which is structuralist, not only in the structural judgments based on the general relational form, but in the formation of concepts which are defined in terms of one another in axiomatic structures. This final innovation sets Cassirer's view apart from previous Marburg efforts, successfully moving Kantian philosophy into the 20th Century.

Nabeel Hamid, University of British Columbia
"The 'Duhem thesis' in Ernst Cassirer's philosophy of science"

Don Howard (1990; 2010) submits that Einstein's sympathy for the Duhemian theses of underdetermination and theory holism is 'one of the keys' for understanding Einstein's criticisms of neo-Kantian defenses of the *a priori* in scientific theories. Howard contends that Einstein's objection to a central neo-Kantian position – that the choice between empirically equivalent theories is determined by *a priori* principles – in favor of conventionalism in the matter of theory choice, owes much to Duhem's arguments for confirmation holism (1990). And recently, Howard (2010) has suggested that Einstein deployed Duhem's thesis to object to Ernst Cassirer's view that certain elements of any mathematical scientific theory are privileged and not subject to revision, i.e., that certain elements function as constitutive *a priori* elements in a given theory – a view revived in recent years by Michael Friedman (2001). This paper aims to shed light on the dispute between Einstein and Cassirer – and thereby on a parallel debate between Howard and Friedman – by turning to Cassirer's ([1923]/1910) reception of Duhem's theses. What did Cassirer take to be of value in Duhem's philosophical reflections on science? And having acknowledged their importance, why and in what respects did Cassirer feel the need to reinterpret Duhem's original thesis? As has been argued by authors such as Ferrari (1995), Cassirer embraced the basic insight of Duhem's holism, but gave it a distinctively neo-Kantian interpretation. In this paper, I first spell out the particular variety of holism that Cassirer develops in *Substance and Function*. Then, I argue that, despite his enthusiasm for Duhemian holism, Cassirer nonetheless resisted some of the potential consequences of Duhem's corollary thesis of the underdetermination of theories by evidence. In particular, Cassirer explicitly blocks an inference to global, or radical underdetermination, according to which scientists are free to make adjustments in *any* part of the theory when faced with conflicting evidence. Cassirer argues that, in the face of a conflict between evidence and prediction, correction takes place according to a definite method of scientific advance, on which the more inclusive relations among theoretical principles are retained, while the less general ones get changed, until theory and observation are brought into accord. The more inclusive relations that form the privileged, constitutive *a priori* elements on Cassirer's view of the historical development of scientific method consist of the mathematical parts of physical theories. This indicates to Cassirer recognition within scientific practice of the importance of preserving a general "form" of experience through the course of scientific progress, whereby the new form must always address questions asked in the old form. In this way, a logical connection and, hence, a common conceptual framework, is retained as one theory supersedes another. I conclude that, whereas Cassirer's acceptance of Duhem's thesis of holism led to a distinctively neo-Kantian variety of theory holism, important differences in emphasis between Cassirer and Einstein with respect to the thesis of underdetermination help to shed light on the crux of their dispute over the a priori.

Dan McArthur, York University
"Exploring neo-Kantianism in Bohr and logical empiricism"

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

Session V.3 Historical method in HPS

Xavi Lanao, Melissa Charenko and Alex Djedovic
"The evolution of case studies in philosophy of science: A path towards integrated HPS?"

Since history and philosophy of science started to collaborate in the 1960s, the rationale for the integration of these two disciplines has continuously been questioned. In 1969, a meeting of the United States National Committee for the International Union of History and Philosophy of Science was held at the University of Minnesota to explore this specific topic. In an influential review of this meeting, Ronald Giere argues that the only common interest in HPS is science and that "this common interest is not a sufficient basis for [anything] other than a marriage of convenience." Despite this initial skepticism over the motivation of HPS, the relationship between history and philosophy of science, at least at the institutional level, has survived to the present. Further, in the last decade, interest in the rationale and prospects for the integration of HPS seems to have resurfaced: several meetings have been held on this topic; journals have devoted focused discussion sections to this question; and, most recently, an edited volume entitled *Integrating History and Philosophy of Science: Problems and Prospects* has been published. Despite this interest in the integration of HPS, the concern that this relationship is a mere marriage of convenience has persisted. In response to this concern and others, advocates of integrated HPS have defended the rationale for it by pointing out that, in contrast to positivist philosophy of science, the examples and case studies used by historicallyinclined philosophers of science are not about black ravens or flag poles, but are actually concerned with scientific practice, both contemporary and historical. Accordingly, this methodological shift in philosophy of science justifies the need for a closer relation between history and philosophy of science.

Our project seeks to track the use of different kinds of case studies and examples during the last century in order to determine the influence on the philosophical literature of the shift towards HPS, and to analyze the possible methodological trends in philosophy of science before and after this change. We undertake an analysis of the Philosophy of Science Association meetings from the first meeting in 1933 up until the present, classifying papers according to the nature and use of their case studies. By examining the contributed and symposia papers at these meetings, we will determine the percentile distributions of the different case studies and their temporal evolution, surveying the changes produced during the shift towards HPS in particular. The distribution of the data that results from this analysis may be indicative of the historical trends in the philosophy of science and suggest how effectively history and philosophy of science have been integrated. In short, we attempt to construct the structural foundations for a history of the use and nature of case studies in philosophy of science. This project, by reconstructing the path that case studies in philosophy of science have followed, will come to bear on both sides of the normative debate about the rationale for HPS and will ground further sociological analysis of this integration.

Aaron D. Cobb, Auburn University at Montgomery

"Exploratory experimentation and securing understanding"

Friedrich Steinle and Richard Burian independently introduced the term 'exploratory experimentation' to characterize a form of experimental practice chiefly concerned with the discovery of stable regularities and the conceptualization of novel experimental phenomena. They contrasted this with a theory-directed form of experimentation aimed at the testing, articulation, and extension of a governing theoretical framework. Since Steinle's and Burian's pioneering work, scholars most interested in this form of experimental practice have been focused on articulating the aims of exploratory experimentation, carefully describing the relationship(s) between theory and exploratory experimentation in various scientific domains and historical contexts, and illustrating the methods and strategies of exploratory experimentation in concert with the functions they serve in scientific inquiry. Concerning the aims of exploratory experimentation, they have identified a variety of goals including the stabilization and characterization of experimental phenomena, the identification of regularities among leading phenomena, the development of proper conceptual schemes for representing experimental results, the creation of instrumentation, experimental protocols, and systems essential to the stabilization and characterization of experimental phenomena, the discovery of mechanisms producing experimental phenomena, and the resolution of potential anomalies for existing theoretical frameworks. These diverse aims suggest varying levels of theoretical influence. Exploratory experimentation is not a "theory-free" form of experimental practice; rather, it is a form of experimentation that is conducted free from the specific direction of a governing theoretical view. With respect to the methods and strategies of exploratory experimentation, several distinct emphases have emerged including the fluidity of experimental parameters and the systematic variation of experimental processes, the construction and use of novel instrumentation ideally suited to experimental variation, and the use of multiple independent experimental techniques and tools as cross-checks. In spite of the considerable attention to these issues, relatively little has been said concerning the epistemology of exploratory experimentation. In particular, philosophers have not devoted sufficient attention to assessing the notion or notions of epistemic justification underpinning the various roles assigned to exploratory experimentation. To the extent that this question has been discussed in this literature, philosophers of science have pointed to the conceptof epistemic iteration as a fruitful way of thinking about epistemic justification in these contexts. In this paper, I seek to extend the discussion of these under-explored questions of epistemic justification in the literature on exploratory experimentation. I develop my discussion historically by considering important experimental work in the early history of electromagnetism. In particular, I discuss some of Charles Babbage and John F.W. Herschel's joint experimental work on Arago's discs and Michael Faraday's ultimate explanation of these phenomena in terms of induced electrical currents. This discussion illustrates the manner in which exploratory experimental practices serve to secure an experimental understanding of phenomena. This kind of security ensures that the experimental understanding produced by exploratory techniques can be employed as a directive foundation for subsequent scientific inquiry.

Philipp Haueis, Freie Universität Berlin

"Logical and experimental underdetermination"

In this paper, I argue that the notion of underdetermination of theory by evidence is practically irrelevant to scientific research and inapplicable to the actual history of science, if it is formulated in its most common form. I call this form 'logical underdetermination' (LUD) which holds that for any scientific theory or system of the world, there is at least one—if not infinite—other theories that rest on the same empirical evidence and imply the same observational consequences (Quine 1975). In the first section of the paper, I contend that this practical irrelevance and historical inapplicability rests on two assumptions about (i) empirical equivalence and (ii) observation. When two theories are empirically equivalent iff there is no possible evidence which can distinguish between them, they cannot be rival theories in the sense that a scientist has to choose between them, a point already acknowledged by Duhem (1954/1906, 100f.). The support for (i) rests furthermore on a concept of observation that is restricted to linguistic entities, i.e. sentences. Understood as a *skill*, I argue, an experimenter's observation has an independence from theory which is different from the usual discussion of the theory-ladenness of observation (Hacking 1983). With this concept of observation in place, I want to go back to Duhem's original thesis and give it a form which I call 'experimental underdetermination'

(EUD). One predecessor of EUD was the philosopher and art historian Edgar Wind (2001/1934), who links underdetermination and experiment through the concept of 'embodiment'. Unlike Duhem, Wind also thought that EUD does not dismiss the possibility of an *experimentum crucis* because for him, the number of unconceived alternatives (Stanford 2001) is restricted to the hypotheses which can be embodied in an experimental apparatus by the scientific practitioner. In the rest of the paper, I briefly want to show how EUD is relevant to scientific research and applicable to the history of science. My first example concerns the experiments of Michelson and Morley (1881, 1886, 1887) about the relative motion of the earth with respect to the ether. Although they initially did not test any theory at all, these experiments embodied a phenomenon which any hypothesis about the ether had to account for in the end. My second example is the Meselson-Stahl experiment (1958) which initially underdetermined three proposed mechanisms about DNA replication (Weber 2006), while retrospectively becoming a crucial experiment for Watson and Crick's (1953) proposal of a semi-conservative replication scheme.

Session V.4 Newton and Huygens

Ari Belenkiy, British Columbia Institute of Technology

"The master at the Royal Mint: How much money did Newton save Britain?"

From the extant statistical data, this paper reconstructs several episodes in the history of the Royal Mint during Isaac Newton's tenure. We discuss four types of uncertainty embedded in the production of coins, extending S. Stigler's work (1977) back in time. The thirteen *Jury Verdicts in Trials of the Pyx* for 1696-1727 allow judgment on the impartiality of the Jury at the trials. The *Verdicts*, together with several remarks by Newton in his correspondence with the Treasury, allow us to estimate the standard deviation σ in weights of individual guineas coined before and during Newton's Mastership. This parameter, in turn, permits us to estimate the amount of money Newton saved Britain after he put a stop to the illegal practice by goldsmiths and bankers of culling heavy guineas and recoining them to their advantage; a conservative estimate for savings to the Crown is £41,510, and possibly three times as much. The procedure with which he likely improved coinage gives historical insight on how important statistical notions – standard deviation and sampling -- came to the forefront in practical matters: the former as a measure of variation of weights of coins, and the latter as a test of several coins to evaluate the quality of the entire population. Newton can be credited with the formal introduction of testing a small sample of coins, a pound in weight, in the trials of the Pyx from 1707 onwards, effectively reducing the size of admissible error. Even Newton's "Cooling Law" could have been contrived for the purpose of reducing variation in the weight of coins during initial stages of the minting process. Three open questions are posed in the Summary.

Alistair Isaac, University of Pennsylvania

"Newtonian answers to Baconian questions: 'Proof by experiment' in Newton's optical research"

Both Francis Bacon and Isaac Newton considered hypothetico-deductive reasoning too tenuous a method to serve as a foundation for scientific inquiry and turned to inductive reasoning as an alternative. This paper examines the theoretical challenges for a Bacon-like approach to induction and the practical solutions to those challenges implemented by Newton in his optical research.

In Book 2 of *Novum Organum* (1620), Bacon attempts to outline a procedure for performing "true induction," i.e. for deriving scientific conclusions directly from data. Bacon's fundamental insight is that induction proceeds by rejecting possible explanations: "True induction is founded on exclusion" (II.XIX). Tables of data on the phenomenon of interest are compiled and systematically evaluated in order to rule out spurious theories. Then, certain special data points, the "privileged instances" (of which the most famous is the *instantiae crucis*), point the way toward positive theory. However, any investigator seeking to implement this method must answer several practical questions: How much data is enough? *How can one ensure that all relevant alternatives have been excluded? How does this method justify an affirmative conclusion?*

Newton's 1672 letter to the Royal Society describes a series of experiments on light refracted through a prism, culminating in a positive conclusion derived from an *experimentum crucis*. Although the close similarity between the experimental procedure described in Newton's letter and Bacon's method has been noted before, past discussion has focused on the veridicality (or lack thereof) of Newton's presentation of the history of his theory. In contrast, I defend the idea that Newton's discussion of experimental procedure is intended as a *justification* for his conclusion.

I argue that Newton has answered the Baconian questions by linking the phenomenon to be explained to a specific experimental setup. In this case, the phenomenon is the oblong shape of the spectrum cast on a wall by light from a circular hole passed through a prism. By varying all the physical parameters of this setup (the type of prism, the place on it through which the light passes, the distance and orientation of the surface on which the spectrum is cast, etc.), Newton thinks he can ensure that all relevant alternative explanations have been checked. Since he has checked all physical features of the setup, Newton considers himself justified in asserting that the conclusion "that *Light* consists of *Rays differently refrangible*" is the "true cause of the length of that image."

I apply this analysis to the argumentative strategy of Newton's *Opticks* (1704 / 1718), where Newton calls this method of reasoning "proof by experiment." These considerations motivate a novel interpretation of the *Queries* which conclude the *Opticks*. Although these are commonly interpreted as a breach of Newton's principle not to feign hypotheses, I argue instead that many of them are better understood as proposals for experiments which can serve as a basis for Newton's method of inductive justification.

Maarten van Dyck, Ghent University
"Mechanics and natural philosophy in the work of Christiaan Huygens"

While Eduard J. Dijksterhuis could still call Christiaan Huygens "the first perfect Cartesian", more recent scholarship has often started from an explicit problematization of the relationship between Huygens's work and Cartesian philosophy (see especially, but not exclusively, Yoder 1989, Mormino 1993, Dijksterhuis 2004, Chareix 2006). Rather than assuming that his breakthroughs in many areas of the mathematical study of natural phenomena were in some way tributary to or explicitly motivated by a Cartesian inspiration, these authors take serious Huygens's very critical remarks towards Cartesian philosophy as recorded by him late in his life. Simultaneously, they often try to relate his work to a different tradition, that of mixed mathematics as practiced most influentially by Galileo, again following Huygens's own unwavering praise for the Italian.

This reassessment of Huygens's work has obviously been not unrelated to a greater sensitivity towards seventeenth century classificatory schemes. It has become increasingly clear that the contours of "natural philosophy", and especially that of "mechanics" and its relations to the former category, were not only quite different from what twentieth century scholars have often been assuming, but also that they were continually being renegotiated throughout the seventeenth century, a process in which the work of Huygens was heavily implicated. This is especially interesting because one of the reasons why earlier scholarship was often interested in the supposedly Cartesian nature of Huygens's "research program" was that it was thought to exemplify the determining influence of philosophy and broader worldviews on the establishment and development of "modern science" (this interest is obvious throughout much of the earlier scholarship, but maybe nowhere as explicit as in Elzinga 1972, who spoke of Huygens's "research program"). If we want to retain this focus on the mutual determination of science and philosophy, which after all has been crucial in constituting the field of history and philosophy of science, we will thus have to take into account how not only science but also philosophy is a category with a very specific history.

In my presentation I will use Huygens's case as an invitation to further think through some of the issues involved. I will use two sets of texts that on first sight could be easily associated with a Cartesian program in natural philosophy, his different drafts for a preface to a never completed treatise on the laws of collision from the 1650's, collected in volume 16 of his complete works, together with the relevant letters from the same time period, and his *Discours de la pesanteur* from 1690, together with its earlier drafts from the late 1660s. I will try to show what a careful reading of these texts reveals with respect to the disciplinary place that Huygens tried to provide them with. The differences that will be revealed between the treatments of respectively collision and weight can be related to a broader distinction between mechanics and natural philosophy, as I will argue is operative in Huygens's work. I will also show how both the identities and mutual articulations between these fields are interestingly different from the Cartesian approach.

Session V.5 Perspectives on post-positivism

Peter Olen, University of South Florida
"Pure pragmatics and logical empiricism: Contextualizing Wilfrid Sellar's early publications"

Wilfrid Sellars's initial foray onto the American philosophical scene was defined by his attempt to construct a pure pragmatics of language. Sellars's introduction of this project turned on his defining philosophy as a formal pursuit; that is, Sellars initially argued that "philosophy is pure formalism; pure theory of language". At first glance such an approach seems heavily indebted to Rudolf Carnap's work on language, especially as found in his *The Logical Syntax of Language*, as well as various approaches to pragmatics that could be generally ascribed to adherents of logical empiricism. In context, Sellars's own remarks seem to support this claim; one of Sellars's early publications describes his philosophical orientation as "a rationalistic realist who has deserted to the camp of logical empiricism". Yet, when reflecting back on this period, Sellars describes his early publications as "at war with positivism" and seems to indicate nothing like a shared philosophical framework between himself and various logical empiricists. These kinds of conflicting claims raise the question: What is the proper historical context and metaphilosophical commitments that frame Sellars's early publications?

The point of this proposed paper is to argue that the context of Sellars's early publications is significantly more complicated than a construal of it as internal corrections to Carnap's philosophical project. I argue that Sellars's early works should be understood as framed by Gustav Bergmann and Everett Hall's critiques of Carnap's approach to semantics and their positive conception of pragmatics. This reading of Sellars's early papers suggests that Sellars's position was not necessarily that of a reformer of logical empiricism; his critiques, ostensibly of Carnap, should not be read as internal corrections to a movement of which he was a member. Instead, Sellars's overriding commitment to a more "traditional" model of philosophy and philosophical problems should be seen as underwriting his critiques of Carnap and logical empiricism as well as his attempts to construct a pure pragmatics.

Aside from the published remarks of Sellars, Bergmann, Hall, Carnap, and others, my analysis of Sellars's early works is rooted in the unpublished correspondence between Sellars and a myriad of philosophers. Specifically, I argue that a number of unpublished letters between Sellars and Bergmann, Sellars and Thomas Storer, and others go a significant distance in supporting my reading of Sellars's early publications. Although Sellars scholarship is currently in vogue, little to no attention has been paid to his early work or the unpublished correspondence surrounding it. It is this correspondence, in conjunction with the work of Sellars's early contemporaries at the University of Iowa, which provides the key to historically situating his project of pure pragmatics.

Vasso Kindi, University of Athens
"The influence of Wittgenstein's philosophy on historical philosophy of science"

The so-called historical turn in philosophy of science in the late 1950s and early 1960s is usually attributed to the fact that history had been brought to bear on philosophy. In the paper I plan to consider another factor that has influenced significantly, and I would claim more than history, the work of Thomas Kuhn, Paul Feyerabend, Stephen Toulmin and N. R. Hanson, namely Wittgenstein's later philosophy. The connection between Kuhn and Wittgenstein has, to a certain extent, been discussed in the literature but the relation of Feyerabend, Toulmin and Hanson to Wittgenstein, as regards the developments in philosophy of science, has been much less explored.

In the paper, I plan, first, to establish that there was a connection between Wittgenstein and all four thinkers, both historically and philosophically speaking. I will highlight the main Wittgensteinian themes in their work and will give an account of the way they interpreted him. I will discuss Toulmin's use of the Wittgensteinian term 'paradigm' in his book *Foresight and Understanding* (1961) and how this use differs from Kuhn's in *The Structure of Scientific Revolutions* (1962), Feyerabend's contextual theory of meaning in "Explanation, Reduction and Empiricism" (1962) which draws upon Wittgensteinian considerations and will touch upon Hanson's appropriation of Wittgenstein's notions of 'seeing' and 'seeing as' in *Patterns of Discovery* (1958). Secondly, I will argue that highlighting Wittgenstein's impact on the turn in philosophy of science in the late 1950s and early 1960s, is not an inconsequential change of focus. The shift of attention from an abstract and intellectualist understanding of science to a more practical one associated with Wittgenstein's philosophy will show that

1. history was not a coincidental (and independent) factor that happened to be implemented by the protagonists of historical philosophy of science in their assessment of science, but rather a means of illustrating the variegated landscape of scientific practice.

2. the very critical reception of historical philosophy of science was shaped, for the most part, by the requirements of the then dominant view of science which was itself the target of the innovators' criticism. Historical philosophy of science was found in the 60s and 70s to be self-defeating, inconsistent, philosophically naive and guilty of promoting relativism, idealism and irrationalism. Incommensurability was largely marked out as the villain. However, if the impact of Wittgenstein's philosophy is recognized and brought into relief, these problems are seen as not following from historical philosophy of science.

3. a rather different set of problems arise from concentrating on the understanding of science as practice, namely, how to account for the unity of a particular practice, how to differentiate between normal development and radical break.

In conclusion I will maintain that Wittgenstein's philosophy, rather than just history, helped induce, as Kuhn expected, a transformation of the image of science by which we were at the time possessed.

Matteo Collodel, Humboldt Universität zu Berlin
"Between logic and history: The development of Feyerabend's idea of incommensurability"

The 50th anniversary of the introduction of the metaphor of incommensurability into the philosophy of science by Kuhn and Feyerabend offers a vantage point from which to evaluate its significance for a philosophical understanding of science, both as a body of knowledge and as a rational enterprise. The aim of this paper is to offer a detailed reconstruction of the development of Feyerabend's idea of incommensurability, supported by the results of historical research into unpublished primary sources of archival origin. The resulting account returns a much more complex picture of Feyerabend's changing idea than the one currently widely accepted and helps in assessing its import against van Fraassen's and Friedman's serious consideration of the issues posed by incommensurability to scientific rationality, on the one hand, and their recent demise by Sankey on the other.

In 1962 Feyerabend deployed the idea of incommensurability at the centre of his attack against the formal accounts of explanation and reduction of logical empiricism. Accordingly, his formulation of the idea was clad in logico-linguistic terms: It highlighted the logical disjointedness of supposed paradigm cases of successive inter-theoretic reductions (e.g., the sequence: Galileo's law of free fall, Newton's general theory of gravitation, Einstein's special theory of relativity) and, on this basis, it pronounced the failure of the alleged attempts of logical empiricism at giving a descriptively adequate reconstruction of the history of science and in advancing a convincingly progressive methodology for science by logical means.

The lively debate sparked by Feyerabend's idea during the 1960s and early 1970s identified some crucial general limits (vagueness, ambiguity) and more specific flaws (paradox of concept acquisition, relevance, rivalry, and analyticity objections) in Feyerabend's logicolinguistic formulation, besides emphasizing the ultimate consequences of the idea, i.e. its relativism and irrationalism. Confronted with unremitting and inescapable criticism, Feyerabend eventually (1975) came to consider impracticable any formally neat and clear-cut formulation of the incommensurability between scientific theories, paradigms or conceptual frameworks. Instead he favoured an anthropological and morphological approach: He regarded the existence of incommensurability as a thesis to be supported by a qualitative, in depth examination of historical and anthropological evidence and incommensurability itself as a relation to be shown – rather than logically explicated - through the display of a series of juxtaposed instances.

This novel approach led Feyerabend to articulate a hermeneutic-ontological formulation of the idea of incommensurability which can be interpreted in terms of a Kantian metaphysics elaborated through Gadamerian categories. Theory changes entail world changes insofar as any world or ontology is constructed on the basis of a human environment that is conceptually mediated as unavoidably experienced through language. However, worlds are not inescapable cages and the way out of them does not require irrational leaps. The constitutive plasticity of language – conceived essentially as a universal medium, rather than as calculus – and the rational resources with which it endows any human community, scientific or otherwise, exorcise the spectres of relativism and irrationalism, since they allow for the concrete possibility of a fusion of necessarily bounded horizons through language construction.

Parallel Session VI

Session VI.1 Symposium: Descartes' Metaphysical Physics: Twenty years young

Daniel Garber's *Descartes' Metaphysical Physics* was published in 1992. The book is filled with innovative interpretative claims not just about Descartes' adversaries, but also Descartes' development, method and his views of matter and motion. In the Prologue to the work Garber explained that he sought "to place both the metaphysics and the physics in their proper intellectual context" with the hope that by so doing he would "illuminate both in ways that more specialized studies of individual arguments and doctrines [did not] do" (2). But twenty years ago, there was no "standard interpretation of Descartes' natural philosophy against which to react." As a result, *Descartes' Metaphysical Physics* aspired to pull "together various aspects of Descartes' metaphysical approach to the world of body" and by presenting "them in a systematic and coherent way" to provide "a kind of handbook of Cartesian physics [and] a general introduction to the mechanical philosophy" (3). Garber's book has since become both the handbook he envisioned as well as a standard interpretation of the metaphysical foundations of Descartes' natural philosophy. In addition, it has come to exemplify the combination of history of philosophy and history of science that animates HOPOS. This symposium seeks to treat *Descartes' Metaphysical Physics* and the last twenty years of Descartes scholarship that is has informed as an object of historical study. We wish to reflect on the book's continuing significance both for Descartes scholarship and seventeenth-century scholarship at the intersection of history of science and philosophy. Examining Descartes life, his method, natural science, and philosophy, the papers in this session will examine the lessons we can draw from Garber's scholarship and from the two decades of discussion that *Descartes' Metaphysical Physics* has helped to create. Dennis Des Chene's paper will reopen the question of the relation of Descartes' metaphysics to his natural philosophy; in particular it will consider whether a new relation was made possible by Descartes' remodelling of natural philosophy so as to found it upon a few simple concepts and laws. Dana Jalobeanu's paper will examine the role Descartes' conceptions of geometry and mechanics playedin shaping his abstract physics. In addition, Jalobeanu will provide a new case study by detailing the reception of Descartes' physics among mathematicians at the Royal Society who navigated the mixed- mathematical features of Descartes' physics in their discussions of his rules of collision. Tad Schmaltz's paper will consider two specific questions – namely, when, if ever, did the "mechanical philosophy" become a unified response against Aristotelian physics and what connection was there between occasionalism and mechanism in the Cartesian aftermath. Daniel Garber's role in the symposium will be to respond Des Chene, Jalobeanu and Schmaltz. Collectively it is our hope that this session will offer insight into how we might continue to explore Descartes' intellectual, social and political context so that we may further our understanding of Descartes' work and one of the most influential programs of the early modern period.

Dennis Des Chene, Washington University
"Descartes' revision of the relations of metaphysics to natural philosophy"

In the famous tree of knowledge put forward by Descartes in the *Principles* (1644) to represent the structure of the sciences, the roots stand for metaphysics. Metaphysics is thus implied to be distinct from natural philosophy and yet continuous with it. To what extent did Descartes impose a new task on this venerable discipline, and to what extent is the continuity also new? In this paper I propose first to examine late Scholastic characterizations of the relation, especially in the quadripartite *cursus* that had increasingly become the vehicle for transmitting Scholastic philosophy. There is, on the one hand, overlap of metaphysical and physical questions, and on the other hand a tendency to isolate for metaphysics a peculiar subject matter consisting in the "incorporeals"—spiritual substances, including God and the rational soul. I then describe the structure of Cartesian physics, and in particular the ideal according to which truths concerning the physical world, characterized in terms of a very limited range of concepts, are to be derived from a few necessary laws together with assumptions concerning the initial state of the universe. The ontology of the physics is rooted in the divine understanding; the necessity of the laws is founded upon the necessity of the divine attributes. In all this, I will argue, the new Cartesian conception of "law of nature" is crucial and marks the most fundamental difference between his natural philosophy and that of his predecessors in the Schools, and so too of the relations held by each to obtain between metaphysics and natural philosophy.

Dana Jalobeanu, University of Bucharest
"Descartes' mathematical physics and Descartes' Metaphysical Physics"

One of the major achievements of Daniel Garber's *Descartes' Metaphysical Physics* was to persuade us to take

Descartes' physics seriously. More precisely, the book showed convincingly that Descartes' physics should be studied because Descartes opened up directions and formulated problems in physics in a way that would go on to shape natural philosophy in the seventeenth century. Garber pictured Descartes as engaged in an enterprise in many ways similar to that of the present day contemporary physicist: the construction of a 'formal physics' working with abstract concepts of bodies, motion, force(s) and laws. There are two major ways in which Descartes' physics is 'metaphysical' in Garber's interpretation: on the one hand, the definitions of its concepts are formulated in terms of a metaphysics of extended matter and (a version of) occasional causation. On the other hand, metaphysics is prior to or more fundamental than mathematics. For Garber, Descartes' physics is metaphysical in large part because it is both more and different than a mere mathematical physics (293).

Substantial amount of work has been done in the past 20 years to pursue this very successful direction of investigation, shaping an entire field of Cartesian 'dynamics'. Meanwhile, comparatively little has been done to integrate Descartes' physics with Descartes' mathematics.

My paper will address two related issues. First, I will discuss ways in which Descartes' conceptions of geometry and mechanics shed light on the construction of his abstract physics. Second, I will address a particular Cartesian problem, namely Descartes' formulation of the rules of collision. As it is well known, Descartes' rules of collisions were considered by a large majority of his contemporaries and successors as a problem of mixed-mathematics. In the second part of my paper I will discuss the way in which the subject of collisions was treated by the mathematicians of the Early Royal Society who deemed themselves "Cartesians." I will specifically emphasize the ways in which a disciplinary separation of mixed-mathematics from a 'metaphysical physics' might prove useful for understanding how a particular Cartesian problem was shaped, received and eventually solved by the philosophical community in the second part of the seventeenth century.

Tad M. Schmaltz, University of Michigan
"The mechanical philosophy and occasionalism: Reflections on <u>Descartes' Metaphysical Physics</u>"

In my contribution to this symposium, I would like to address two questions concerning early modern philosophy that Dan's justly-celebrated book on Descartes's metaphysical physics broaches. The first question concerns the status of "the mechanical philosophy" during the early modern period. In his book, Dan is concerned to emphasize the importance to Descartes of this new philosophy, and indeed even presents him as one of its founders. However, in more recent work he claims that there was in fact no united mechanistic front against Aristotelian physics prior to Boyle's "invention" of the notion of a mechanical philosophy. I agree that there was no united mechanistic front prior to Boyle, but I also claim that even after his purported invention there remained significant divisions in the mechanist camp that have pre-Boylean roots. I consider whether given this fact, the claim that the mechanical philosophy displaced Aristotelian physics during the early modern period remains a useful one. My second question concerns the status of occasionalism in the new physics that derives from Descartes. Toward the end of his book, Dan claims that for Descartes, God is the only real cause of changes in motion due to bodily collisions. I have argued against this claim in some detail elsewhere, and so will address this issue only briefly. My focus is on the post-Descartes debate over the connection between Cartesian mechanism and occasionalism. I emphasize in particular the complications for this connection that derive from the argument in Fontenelle that this kind of mechanism is in fact incompatible with the sort of occasionalism that Malebranche defended.

Daniel Garber, Princeton University
"Response"

Session VI.2 Symposium: Poincaré in perspective: Conventionalism one hundred years later

Poincaré's conventionalist philosophy has been widely influential, both during his lifetime and in the 100 years since his death. The logical positivists, of course, appealed to it in their distinction between empirical sentences and linguistic, or analytic, principles. There are also echoes of Poincaré in the revolutionary ideas of Quine and the later Wittgenstein. More recent attempts to build on Poincaré's ideas can be found in the work of Grunbaum, Giedymin, and Friedman. This symposium will revisit the idea of convention in Poincaré to further clarify both his position with respect to physics as well as his influence on twentieth century philosophies of science.

Poincaré's conventionalism is the thesis that there are principles in science that must be decided upon. We must decide them because they are not imposed on us - by the empirical world or by the nature of our own minds. Conventionalism thus entails that there is some degree of arbitrariness in physics, a degree that varies with the principle and the theory that it supports. For example, Poincaré softens the arbitrary nature of some conventions by appealing both to their convenience and to the fact that experience can "suggest" them. Nevertheless owing to their role in scientific theory they are "constitutive" of what experiences will count as confirmations or disconfirmations.

Philosophers of science in the past century have developed the doctrine there are principles between those that are necessary *a priori* and those that are empirical. Sometimes called the "relativized *a priori*", it is the view that there are necessary preconditions for the possibility of science (hence the "*a priori*") that are not fixed once and for all (hence the "relativized"). The participants in this symposium will attempt to further clarify the relativized a priori and its roots in Poincaré. Maria de Paz will argue that there are actually six concepts of convention in Poincaré's writings as well as two historical lines of interpretation that need to be detangled in order to correctly understand Poincaré's "third way" epistemology. Janet Folina will address the general category of convention in Poincaré by contrasting geometric conventionalism with both mathematical intuition, which yields synthetic *a priori* principles, and with the stipulations of formalist approaches to mathematics. David Stump will call into question the extent to which the relativized *a priori* is really a consistent development of Poincaré's geometric conventionalism. Finally, Robert DiSalle will argue that rather than Poincaré's conventionalism about space, it is more the particularly privileged position of space and spatial concepts in Poincaré's philosophy that prevents him from proposing the centrality of space-time in physics.

The goal of this session is to reconsider Poincaré views and the role that they have played in various attempts to clarify the different types of principles in physical theory. Each presenter will contribute to this goal by approaching Poincaré's conventionalism from a unique set of questions and perspectives. This session will also celebrate the fact that in the year of the centenary of his death, Poincaré's ideas remain stimulating and important.

Robert DiSalle, University of Western Ontario
"Poincaré on the construction of space-time"

One of the enduring challenges for the interpreter of Poincaré is to understand the connections between his analysis of the geometry of space and his view of the development of the theory of space-time. On the one hand, he saw that the invariance group of electrodynamics determines a four-dimensional space with a peculiar metrical structure. On the other hand, he resisted Einstein's special theory of relativity, and continued to regard the Newtonian space-time structure as a sufficient foundation for the laws of physics. Thus Poincaré did not treat the fundamental symmetry that he discovered in the way that Minkowski did, that is, as the fundamental symmetry group of space-time itself.

One way of approaching this circumstance is to ask, to what extent was his comparatively conservative treatment of electrodynamics influenced by his conventionalist approach to geometry in general? I propose to begin with a related but quite different question, namely, why did not Poincaré extend to space-time the kind of epistemological analysis that he had applied, with such success, to the notion of space? It might be argued that his argument for resisting relativity was identical to his argument for resisting non-Euclidean spatial geometry: that it is a matter of conventional choice, in which physicists are justified in choosing the simplest possibility. But this is a crucial part of the context, not a complete explanation. I suggest that a fuller understanding requires an understanding of the privileged position that space plays, according to Poincaré, in our conception of the physical world, and particularly in the construction of the fundamental concepts by which physical processes submit to objective measurement. Poincaré's epistemological analysis of the construction of space could be extended to the construction of space-time, and it was Minkowski who argued that, given the new developments in electrodynamics, such an extension was epistemologically necessary. From this perspective, Poincaré's position results from granting the concept of space an epistemological priority that, in the face of modern physics, it was unable to sustain.

Janet Folina, Macalester College
"Poincaré's conventions: Between intuition, empiricism and stipulation"

Jules Henri Poincaré is famous for his "conventionalist" views. But what did he mean by conventionalism? Is geometric conventionalism different from physical conventionalism, and if so how? How can we assess his conventionalism? I propose to address the category of convention by contrasting it with intuition, empirical truth, and stipulation.

On one side Poincaré's geometric conventionalism is clearly different from what we might call "pure mathematics" – the mathematics that depends only on mathematical intuition and reasoning. For Poincaré, number theory is a special example of a discipline governed by intuition. His appeal to intuition here is intended to be Kantian, though the intuition to which he appeals is neither spatial nor temporal intuition but "indefinite iteration". Indefinite iteration is an intuition that, he says, explains why the principle of induction is "forced" on us as true. And induction is the basic principle for uncovering truths about the natural number structure. In contrast, geometric conventionalism is precisely the view that geometry is not forced on us as true. An entirely different story is thus called for in geometry.

Empirical truth constitutes a second contrast with geometric conventionalism. As pointed out by Michael Friedman, Robert DiSalle and others, for Poincaré, fixing the principles of geometry is what makes it *possible* to discover empirical truth in physics. Geometry is therefore a precondition of empirical physics in his view. So it is clearly distinguished from empirical truth.

A third contrast to make with Poincaré's conventionalism is with a more straightforward formalist view. A crucial further question regards how conventionalism, in particular geometric conventionalism, is distinct from formalism? With Hilbert and against Russell Poincaré argues that geometric axioms are implicit definitions of the basic concepts – they are meaning-*determinations* rather than meaning-*reflections*. But commentators typically agree that Poincaré's conventions are not, as a rule, entirely arbitrary. I will attempt to clarify the distinction between convention and stipulation by contrasting Poincaré's views about geometry with a more straightforward formalism. In so doing, I aim to highlight what is special, or new, about the category of convention, which Poincaré invents.

Maria de Paz, Universidade de Lisboa
"The third way in epistemology: A re-characterization of Poincaré's conventionalism"

Poincaré's philosophy of geometry and physics is widely known as 'conventionalism', which comes from the author's use of the word "convention". We can identify two classic lines of interpretation in philosophy of science, one from the 1960's and the other from the 1970's. The first is the view that physical conventionalism is a natural consequence of geometrical conventionalism. This was held and spread by Grünbaum, who probably took it from Rougier and Reichenbach; it is also linked with the interpretation of Poincaré by the Vienna Circle and its intellectual heirs.

On the other side, we have Giedymin's criticism of this interpretation, developed during the 70's and 80's, which supports instead the epistemological independence of physical conventionalism. Physical conventionalism may have originated in the geometrical conventionalism, but it is a separate doctrine. Among today's defendants of this view is Pulte, who argued that the 19th Century development of Mechanics provides an independent grounding for physical conventionalism.

The two conceptions of conventionalism require us to clarify the relevant notion of 'convention'. The aim of this paper is to show the different meanings of convention in both Poincaré's four philosophical books and in the work of his contemporaries (Le Roy, Duhem, Milhaud), which forced him to clarify his use of "convention" so he would not be mistaken for a nominalist. Another aim is to address the idea of a 'third way epistemology'; this is taken from Pulte, who asserts that physical principles, named as "conventions" by Poincaré, "are neither inductive generalizations nor are they synthetic *a priori* propositions imposed by reason". In this sense, Poincaré's conventionalism is a middle path between the empiricism and the rationalism, taking elements from both.

We will show firstly that in his texts we can identify at least six uses of the word 'convention', going from the arbitrary ones that he always tried to avoid to the more refined use of convention as a principle in physics. Secondly, we will examine the use of these six different kinds of conventions in geometry and physics, showing that some of them are only interpretable in one of these scientific domains. Finally, we will show that both conceptions of conventionalism are right in some respects, in that each emphasizes different aspects of Poincaré's epistemology. Clarifying the meaning of "convention" would solve important puzzles and perhaps contribute to an even greater recognition of Poincaré's "third way" epistemology.

David J. Stump, University of San Francisco
"From Poincaré to pragmatic *a priori*: Lenzen and Pap on the conventionality of principles"

Poincaré argues that certain elements of empirical science can be "erected" (érigées) into principles, that is, they can be taken to be definitely true and never questioned. Victor Lenzen and Arthur Pap both used the conventionality of principles in Poincaré to ground their theories of a relativized, or functional, *a priori*. The conventional principles stand in for what had formerly been taken to be *a priori* in that they are taken to be true prior to any empirical inquiry. These conventional principles can be changed however, so they are not fixed necessities like the traditional *a priori*.

While the conventionality of principles seems to fit with the relativized *a priori*, the main type of conventionalism for which Poincaré is known, geometric conventionalism, has a quite different status and a quite different justification than the conventionality of principles. So it is unclear the extent to which Poincaré's views are really compatible with the relativized *a priori*. He certainly pioneered the idea that Euclidean metric geometry is not a necessity imposed on us as Kant thought, but he also held (somewhat infamously) that Euclidean geometry will continue to be used in physics no matter what, showing that he does not envision a historically changing *a priori* set of principles. Nevertheless, Poincaré set out one of the most influential arguments that what Kant took to be fixed and necessary was in fact neither.

Here I will explore the ways in which Poincaré can be properly said to have a relativized *a priori* and how his views on these matters fit with his overall philosophy. He seems to advocate a traditional form of the *a priori* in the limited area of arithmetic, and seems to reject the *a priori* in physical theory (e. g. Poincaré holds that we have no *a priori* intuition of space or time). Thus the place of a relativized *a priori* clearly must be limited as well.

Session VI.3 History of Philosophy of Biology

Marij van Strien, Ghent University
"Vital instability: How Maxwell, Kelvin and others created a domain for life through physics"

Around the 1870's a number of physicists, such as Maxwell, Kelvin and Boussinesq, published theories which have become known as an example of the concern of their time with scientific determinism and as an attempt to save free will (see Hacking, *The Taming of Chance* (1990), Porter, *The Rise of Statistical Thinking* (1986)). I argue that these theories can be better understood as an episode in the nineteenth century debate on materialism and vitalism than as an early version of the familiar debates about determinism and free will. I show that an important part of the project of these authors was to allow for non-materialistic explanations of physiological processes, and that their ideas can be understood as a defence of a form of vitalism. I describe a vitalist school among physicists, mathematicians and engineers of the 1870's, which besides Maxwell, Kelvin and Boussinesq also involved people like Balfour Stewart and Antoine Augustin Cournot.

It is no coincidence that these theories only appeared around the 1870's and not at the time of Newton or Laplace, when physical determinism was developed. In the mid-nineteenth century physiologists like Helmholtz and Du Bois-Reymond applied the law of conservation of energy to physiology to argue that non-material entities such as the human will, the soul or a vital force could not have an impact on the body. They thus argued against both mindmatter interaction and vitalism.

The theories which I describe were a reaction to this, and were triggered by developments in physiology rather than by physical determinism. Whereas Hacking and Porter make it seem that developments in physiology triggered a pure determinism-free will debate, I argue that it remained largely a physiological debate. Physicists like Kelvin and Boussinesq became involved in this debate when they argued that physics did not support the materialistic view that these physiologists put forward. They described physical systems that were unstable, and argued that in such systems , a non-physical "directive principle" may be able to act unnoticed by exerting a very small force. The body may be such an unstable system. For Boussinesq and Maxwell, the "directive principle" could be human free will, but it could also be a vital principle, while Stewart and Cournot regarded it exclusively as a vital principle. According to them, the intervention of such as a vital principle was needed to explain organisation in living beings. Thus, through studying unstable systems it was possible to show how a non-physical cause, such as a vital principle, could intervene in physical systems. That this is really something different from arguing against determinism can be seen from the fact that Cournot and Boussinesq did not think that the intervention of a vital principle made physiological processes indeterministic. Rather, such processes were subjected to a 'physiological determinism', which was irreducible to physical determinism. The group of physicists that I describe were thus opposed to the exclusion of non-physical causes in physiology, and in this way they defended vitalism and the autonomy of physiology against the physical reductionism of the physiologists.

Charles H. Pence, University of Notre Dame

"The early history of chance in evolution: Causal and statistical in the 1890s"

The traditional history of the understanding of chance in evolution, as told by those like Depew and Weber (1995), goes roughly as follows: for Darwin, evolution is a non-statistical theory (because Darwin predates statistics) of a non-chancy process (natural selection, taken to be analogous to artificial selection). Francis Galton introduces statistics into the study of evolutionary theory in his work on regression and the Law of Ancestral Heredity. Sewall Wright, then, introduces a more robust notion of chancy evolutionary processes when he proposes that drift, in his shifting-balance model, is capable of actively, yet probabilistically, driving populations down the adaptive landscape, away from a selective optimum.

This history thus asks two questions: (1) When did evolutionary theory become statistical? (2) When did evolutionary processes come to be seen as chancy? While both these questions are certainly interesting and while the two standard answers to them may well be correct I argue that they miss a vital shift in thinking about chance in biology that was happening well before Sewall Wright. Two of Galton's students, W.F.R. Weldon and Karl Pearson, founded what would come to be known as the biometrical school, dedicated to the statistical study of evolutionary phenomena. Further, they both spent extensive time considering the philosophical grounding of their statistical study of biology. The concerns they had, however, do not map cleanly onto the two questions asked by the standard history: on each of these questions, Weldon and Pearson appear to differ very little from the views of their mentor Galton.

I argue, then, that it is time to deploy a new perspective when evaluating the views of these early evolutionists. Rather than searching for the ontic or reified sort of chance implied by the standard history's question (2), we can find more profitable results if we consider the work of those like Weldon and Pearson from a different angle: What is the relationship between (statistical) biological theories and the processes they describe? Further, when we examine the positions of Pearson and Weldon on this question, we find both that the two men, who are commonly thought to agree on nearly all points of interest, diverge in important and significant ways, and that this divergence parallels a heated debate in contemporary philosophy of biology: that between causal and statistical interpretations of natural selection, fitness, and genetic drift.

Olivier Sartenaer, Université Catholique de Louvain

"Neither metaphysical dichotomy nor pure identity: Clarifying the emergentist's creed"

In 1875 the British philosopher George Henri Lewes introduced the concept of « emergence » in the philosophical literature. This concept, intended to suggest the idea of « an apparent discontinuity grounded in an actual continuity » – in the way an iceberg is emerging from water – constituted the core of a new philosophy of nature that explicitly claimed to constitute a middle ground between the antithetical views that were, on the one hand, monist materialism inherited from Democritus's atomism and, on the other hand, modern versions of Plato's substance dualism. By claiming to be the defenders of such a conciliatory view, Lewes and the subsequent « British Emergentists » developed their philosophy on the basis of a fundamental tension. They had to conceptualize a kind of natural creativity – in continuity with French spiritualists like Bergson – without giving up the materialist ideal of scientificity; they had to provide a view that pays tribute to the monist and determinist commitments of modern sciences without eliminating from nature what seems to be genuinely novel or unpredictable.

The constitutive tension of British Emergentism – holding at the same time some form of natural discontinuity and continuity – crystallized in different scientific controversies in the beginning of the 20th century. For instance, in the field of biology, the « emergence of life » was meant to capture the materialist thesis of the dependence of living organisms on a physico-chemical basis while arguing that living systems are not identical to physico-chemical systems. Emergentists were thus positing themselves in the « no man's land » between, on the one hand, dualist vitalists postulating – à la Bergson or Driesch – the existence of an irreducible vital stuff like élan vital or entelechy and, on the other hand, monist (iatro-) mechanism directly inherited from the Cartesian concept of animal machine. In the field of mind sciences, British Emergentists held that « the mind emerges from the body », asserting in this way an actual continuity in the mind-body relationship (mental properties depend on neurophysiological properties), but also a kind of discontinuity between such entities (mental properties are not neurophysiological properties). Emergentists were then defending a middle ground doctrine between versions of Cartesian interactionist dualism (committed to the existence of radically heterogenous stuffs like res extensa and res cogitans) and Spinozist monism typical of materialist neuroscientists like – for instance – proponents of Gall's phrenology.

An immediate question arises here : is such a middle ground between radical monism and dualism philosophically viable or consistant ? In other words : is it possible to hold together a certain form of natural continuity and discontinuity ? The main objective of the proposed talk will be to give an insight of the ways contemporary emergentists answer these questions. In particular, on the basis of a purely conceptual distinction of different levels of tension between monism and pluralism (the levels of substances, properties and predicates), I will provide a framework allowing to understand the main emergentist strategies that make the ideas of continuity and discontinuity compatible. These strategies will then be associated with different and frequently discussed concepts of emergence, like for instance theoretical, explanatory or ontological emergences. The overall upshot of such a conceptual analysis will be the building of a taxonomy that allows to clear up the nebulous debates pertaining to the reductionism issue.

Jan Baedke, Ruhr Universität Bochum
"'The epigenetic landscape in the course of time': A transdisciplinary survey of Conrad Hal Waddington's landscape images"

The interrelationship between art and the sciences, especially the life sciences, has a long history but is often skimmed over in contemporary philosophical literature: usually scientific images simply are understood as tools to visualize theoretical concepts or the phenomena under study. But this view on pictures carelessly neglects their potential heuristic role in modeling and theory formation. These issues will be addressed in this paper, based on a case study of Conrad Hal Waddington's epigenetic landscape images and their history of reception. Throughout his life Waddington – leading British embryologist and geneticist from the late 1930s to 1950s – was interested in topics transcending conventional disciplinary boundaries. Today he is best known for introducing the concept of the 'epigenetic landscape' to developmental biology in the late 1930s in order to describe cell differentiation. Waddington's drawings of this landscape subsequently became well-known not only in biology. The following issues will be addressed here: First, taking Waddington's uses of his landscape images as a starting point, a general epistemological approach of pictures as theory-constitutive visual metaphors will be presented. Despite the conventional function of visualization, three further heuristic roles of pictures can be reconstructed from Waddington's work: they can be used (i) as a tool for transdisciplinary research (i.e. they establish a consisted tradition of illustration to unify distinct disciplines), (ii) as a creativity tool (i.e. the stimulation of visual thought by art), (iii) as a heuristic tool to coordinate methodological strategies and modeling efforts.

Then, it will be argued that Waddington's rather intuitive, but visionary insight into the versatility of his visual metaphor in theory formation had a tremendous impact on the history of reception of the epigenetic landscape images: until today several lines of tradition emerged, applying these landscape images (i.e. Waddington's originals or modified versions) to highly diverse phenomena – in disciplines like cellular reprogramming and epigenetics, but also in developmental psychology, STS and cultural anthropology. These traditions will be reconstructed and classified, using the developed concept of visual metaphors as a tool to distinguish the different methodological roles Waddington's landscape images play within these accounts.

Session VI.4 Historical methods in philosophy of science and mathematics

Jacobo Asse Davan, Universidad Nacional Autónoma de México
"Incorporating history into the philosophy of mathematics"

Consensus is that, if mathematical objects exist, then they must be abstract, independent, eternal and immutable objects – Platonic objects – the kind that don't have a history. Yet, mathematical practices have changed considerably through time, from the ancient Greeks, who studied the properties of magnitude, multitude and proportion, to today's mathematicians, who, if asked, will probably respond that they're studying sets, structures or inferential relations. This presents the realist philosopher of mathematics with serious difficulties, as he sees himself forced to adopt a presentist historiography of mathematics, one that must reject genuine mathematical innovation, and explain mathematical growth as a history of the inevitable road towards the discovery of today's true mathematical knowledge; an explanation that makes mathematical history inconsequential for the philosophy of mathematics.

In three recent articles (1999, 2003, 2007), Madeline Muntersbjorn exposes these difficulties and rejects presentist historiography, while still espousing mathematical realism. She does so by rejecting the implication from realism to Platonism, and proposing instead that mathematical objects, while real, are cultivated objects, the result of our mathematical practices, in analogy to new vegetable species that are the result of our agrarian practices. From this point of view, mathematical objects do change, and their history becomes relevant in the study of their nature. This history is closely related to the representational innovations mathematicians come up with to make hidden premises explicit, and to abbreviate – and sometimes reify – successful mathematical procedures. These are, according to Muntersbjorn, two of the driving engines of mathematical growth, a thesis that implies that novel mathematical notations have a causal power in the creation of new mathematical objects. In this talk I try to take Munterbjorn's proposal a little further, by assimilating these cultivated objects into a much larger set of objects of which they are a subset, that is, into the set of artifacts. As Sperber (2007) argues, artifacts go well beyond the prototypical concrete tools we use in daily life, and are instead characterized by their function, a characteristic shared by a very diverse and populated set of objects that include domestic animals, engineered bacteria, paths in the grass, queues, models, and many others.

More specifically, I will argue that mathematical objects are formal artifacts, selected from the universe of formal structures because of their function. But artifacts go beyond what constitutes them,and so, mathematical artifacts go beyond their formal structure, and include their function and their history. This assimilation serves the purpose of providing Munterbjorn's thesis with a more general framework that makes it less ad hoc. Some of these kinds of artifacts, like models, are considered by some to be epistemic artifacts (Knuuttila 2005) and are thought to play an important role in the growth of knowledge. This makes Munterbjorn's thesis much more than a solution to the realist's dilemma, as it provides an explanation of the role mathematics plays in our general knowledge.

Mark Dietrich Tschaepe

"John Dewey's conception of scientific exploration: Moving philosophers of science past the realism-antirealism debate"

In his essay, "Aspects of Scientific Explanation," Carl Hempel begins his argument for his theory of explanation with an example about soap bubbles from John Dewey's book, *How We Think*. Later in the essay, Hempel will draw upon another example utilized by Dewey: explaining the rising of water by using a pump. Despite explicitly drawing upon Dewey for at least one of these two examples, Hempel nowhere acknowledges Dewey's own conceptions of science or scientific explanation, yet John Dewey provided a robust and thorough conception of scientific explanation within his philosophical writing. Despite the attention paid to Dewey by notable philosophers of science such as Hans Reichenbach and Ernest Nagel, Dewey's conception of scientific explanation has been almost entirely neglected by philosophers of science in the latter half of the twentieth century and the beginning of the twenty-first century. Here I provide an exegesis of Dewey's concept of scientific explanation and argue that this concept is important to contemporary philosophy of science for at least two reasons. 1) Dewey's conception of scientific explanation avoids the reification of science as an entity separated from practical experience. 2) Dewey supplants the realist-antirealist debate within the philosophical literature concerning explanation, thus moving us beyond the current stalemate within philosophy of science. Through the reconstruction of Dewey's concept of scientific explanation, and a comparison of Dewey's conception with that of Wesley Salmon and Bas van Fraassen, I hope to bridge his largely neglected work concerning science and explanation with contemporary philosophy of science.

Matthias Neuber, Universität Tübingen

"Is logical empiricism consistent with scientific realism?"

Scientific realism is the view that the theoretical entities of science exist. Atoms, forces, electromagnetic fields, and so on, are not merely instruments for organizing observational data but are real and causally effective. This view seems to be hardly compatible with the logical empiricist agenda: As common wisdom has it, logical empiricism is mainly characterized by a strong verification criterion of meaning, i.e., by the project of defining the meaning of theoretical terms by virtue of the meaning of purely observational terms. However, it has been largely ignored by the historians of logical empiricism that there indeed existed a *realist faction* within the logical empiricist movement. Among the few authors who have recognized both the historical and the programmatic relevance of this realist faction is Stathis Psillos who, in two recent papers, attempts to emphasize the important role played in this connection by Herbert Feigl (see Psillos 2011a) and by Hans Reichenbach (see Psillos 2011b). According to Psillos, it was these two thinkers who documented in their writings the *compatibility* of logical empiricism and scientific realism.

Like Psillos I am of the opinion that the realist faction within the logical empiricist movement deserves more attention than it has received so far. However, I will come to a different result than Psillos. According to the view I wish to defend, Feigl and Reichenbach (and with them Psillos) are still too optimistic about the ontological impact of language. In order to establish the intended realist account of logical empiricism, more metaphysics is needed than Feigl and Reichenbach (and with them Psillos) would allow. As will be shown, among the logical empiricists themselves it was Eino Kaila (1890-1958) who came closest to this—less linguistic and more metaphysical—kind of approach.

Charles T. Wolfe, University of Ghent

"Materialism before physicalism: Cultured brains and reductive materialism from Diderot to J.C.C. Smart"

Materialism is the view that everything that is real, is material or is the product of material processes. Better put, it tends to take either of two forms: a more 'cosmological' claim about the ultimate nature of the universe, and a more specific claim, that the mental is really the cerebral – mental processes are brain processes. Of course, both of these seem to indicate a privileged relation between materialism and scientific inquiry – or rather a privileged *role* for scientific inquiry. (A little-known historical detail testifies to this: prior to becoming a philosophical term in the later seventeenth century, with More, Cudworth and others, the word 'materialist' originally referred to pharmacists, that is, purveyors of the *materia medica* [Bloch 1978].) In the twentieth century, the science that predominated in this vision of things was physics. Materialism became synonymous with 'physicalism'; the entities that were considered to be real were those described in the physics of the time. This has spawned some new problems, both for materialism (what happens to an ontology of material entities in the era of quantum physics?) and for ontology in general (is physicalism an ontological claim? A claim about the suppleness of the relation between philosophy and science?).

However, I shall not focus here on the shifts in the relations between materialism and physics, but instead on the second species of materialism: claims about minds and brains. In the last third of the eighteenth century, Denis Diderot (1713-1784) was one of the first thinkers to notice that any self-respecting materialist had to address the question of the status and functional role of the brain, and how much of our mental, affective, intellectual life is contained therein. In the nineteenth century, brain-mind identity becomes a rather dogmatic affair, with repeated, knee-jerk reiterations of 'psychophysical identity' by Vogt, Moleschott, Büchner et al.. In the 1960s, a group of primarily Australian philosophers took up brain-mind materialism afresh as 'identity theory', i.e. the claim that there is an *identity* between mental processes and cerebral processes. (They in fact waver in between being brain theorists – with surprisingly little invocation of neuroscientific evidence [Bickle and Mandik 2010] – and being metaphysicians bringing the rest of the world into line with physics.)

If we contrast Diderot's materialism with that of the identity theorists, a notable difference is that Diderot allows for a much more culturally saturated or sedimented sense of the brain, which he describes in his unpublished *Éléments de physiologie* as a "book – except it is a book which reads itself"; and that he expresses his materialist credo in the form of an experimental novel, *Le Rêve de D'Alembert* (written 1769, unpublished in his lifetime; it combines 'science-fiction' with an empiricist and materialist critique of metaphysics). I have examined the identity theory as an episode in the history of materialism (Wolfe 2006) and Diderot's idiosyncratic form of materialism (Wolfe 2009), but here I seek to contrast the two as forms of materialism regarding the specific issue of whether materialism can allow for a 'cultured', 'social' understanding of the brain.

Saturday, 23 June

Parallel Session VII

Session VII.1 Symposium: Newton's place in the rationalist tradition

It has been long standard to address questions surrounding Isaac Newton's place in the history of philosophy by attending to Newton's relationship to seventeenth and eighteenth British empiricism. There are, of course, good reasons for adopting this strategy: Newton shared close professional ties with "empiricists" such as John Locke, he promoted and practiced what he termed an "experimental philosophy," and, as emphasized in the scholarship produced over the last several decades, Newton forcefully rejected central aspects of Cartesian natural philosophy and offered his program as an alternative to the "rationalism" that held sway on the Continent. On this approach, then, to understand the philosophical commitments that directed and informed Newton's work is precisely to understand the nature and extent of his "empiricism," and moreover, the implication is that to understand Newton's impact on the history of philosophy is to understand the extent to which his natural philosophy influenced "empiricist" trends in the eighteenth century and beyond.

As fruitful and informative as such discussions of Newton-and-empiricism have been, our goal in this symposium is to broaden our view of Newton's relationship to the history of philosophy by exploring the ties between Newton's philosophy and the "rationalist" tradition. Domski will examine the connections between Newton's metaphysics of space and the neo-Platonist position adopted by Proclus, and bring light to the rationalist epistemology that informs the notion of absolute space presented in the *Principia*. Schliesser's focus is on Newton's place in debates surrounding Spinoza's metaphysics. He will give a fresh look at how Newton's natural philosophy was used against Spinoza and also how Spinoza's defenders attempted to reply to the Newtonian challenge. Finally, Folina will turn attention to William Hamilton's nineteenth century appropriation of Newtonianism in his philosophy of mathematics. She argues, in particular, that the epistemic ideals that underpin Newton's preference for geometry also emerge in Hamilton's philosophy of algebra.

Our papers touch on a variety of historical episodes and a range of philosophical issues; however, they all share the common purpose of deepening our understanding of how Newton's philosophy is connected to various forms of "rationalism" in the history of philosophy, science, and mathematics. Collectively, the papers in this symposium will, we hope, plant the seeds for further exploration of Newton's complicated place in the history of philosophy of science and mathematics, and for further discussion, specifically, of the sense in which Newton's philosophy straddles the empiricism-rationalism divide.

Mary Domski, University of New Mexico

"Newton and Proclus on the geometry of absolute space"

Newton's pre-*Principia* text, *De Gravitatione* ('On the Gravity and Equilibrium of Fluids'), has been at the centerpiece of several recent discussions of Newton's rejection of the natural philosophy forwarded in Descartes' *Principles of Philosophy* (1644) (cf. Janiak 2009, McGuire 2007, Stein 2002). What has drawn far less attention is the detailed account of space that Newton offers in this short text. The notion of space he presents in *De Gravitatione* is intended to replace and improve upon Descartes' identification of space (or extension) with material body, and Newton's claim in particular is that we generate an idea of space – an idea of "the uniform and unlimited stretching out of space in length, breadth, and depth" – "by abstracting the dispositions and properties of a body." My goal in this paper is to clarify the notion of abstraction that Newton adopts by putting his account of space into conversation with the account found in Proclus's commentary on Euclid's *Elements*. What we find, in particular, is that both Newton and Proclus ground their mathematical treatment of real space on a metaphysical picture according to which the mathematically intelligible is part and parcel of the general natural order. As I argue in this paper, Newton's commitment to this neo-Platonic portrait of a mathematical order of nature lends important insight into the process of abstraction on which Newton's *De Gravitatione* treatment of space relies. Moreover, the metaphysics on which Newton's "mathematization" of nature relies brings us to a better understanding of Newton's relationship to his mathematical contemporaries, such as Isaac Barrow, and, perhaps most importantly, it lends important insight into the philosophical grounding for the absolute space presented in the *Principia*.

Eric Schliesser, Ghent University

"Spinoza and the Newtonians on motion and matter (and God, of course)"

The purpose of this paper is three-fold. First, I document a battery of arguments that were generated by the first generation of English critics of Spinoza: Henry More and, especially, Samuel Clarke. These arguments focus on the perceived deficiency of Spinoza's treatment of motion. These arguments are offered as criticisms internal to Spinoza's system as well as external criticisms. The internal criticisms are two-fold: i) Spinoza cannot account for the origin of motion from within his system; ii) Spinoza offers contradictory analysis of motion. Both criticisms are connected to Spinoza's views on the nature of matter. The external criticisms can also be distinguished in two kinds: i) Spinoza's treatment of motion does not lend itself to (mathematical) natural philosophy and some of its detailed empirical claims; ii) the particular motion(s) exhibited in the world require(s) a different conception of God than Spinoza offers. It is no surprise that in Clarke's (1704) *A Demonstration of the Being and Attributes of God* the empirical success of Newtonian natural philosophy is explicitly used in various arguments against Spinoza (in what here I have dubbed the external criticism).

Second, I show that even though Newton does not explicitly mention Spinoza in the *Principia*, he added an argument to the General Scholium (added to the second, 1713 edition of the *Principia*) that is almost word for word identical to one of Clarke's argument in the *Demonstration*. Informed readers of the *Principia* would have recognized the target. (Moreover, it turns out that some of the other changes to second edition of the *Principia* can be related to Clarke's treatment of providence.) I provide evidence that the leading Newtonian of the Scottish Enlightenment, Colin MacLaurin, relied on the Clarke arguments and extended it against Spinoza by showing how Spinoza's views on motion, conservation laws, and the vacuum are a connected package. (As an aside, Clarke and MacLaurin sensitize us to Spinoza's critical remarks about mathematical natural philosophy.)

Third, I then turn to a more sympathetic treatment of Spinoza's views on motion. In particular, by drawing on Toland's *Letters to Serena*, I show that Spinoza's most sophisticated defenders recognized that the Newtonians had hit a significant target. But I show that Toland also provides Spinoza's matter theory with resources that can salvage Spinozism in an age where Newtonian mathematical science rules supreme.

Janet Folina, Macalester College
"Hamilton's Newtonian defense of truth in algebra"

Sir William Rowan Hamilton supported a Kantian account of algebra. In particular, he thought that algebra should be justified as a science like geometry, based on a Kantian, rationalist conception of a priori temporal intuition. In contrast, Newton avoided algebraic methods in his calculus and celebrated the ties between geometrical constructions and empirical motions in his priority dispute with Leibniz. These differences in both their mathematical work and their general philosophical approach to scientific knowledge seem obvious and substantial. However, I will argue in this paper that despite these differences, there is an interesting sense in which Hamilton exemplifies the Newtonian tradition. I will focus on the fact that both are anti-formalist and that both emphasized mathematical methods that are based on insight into a subject matter.

Following Guicciardini (2009), we can read the Newton-Leibniz controversy over the calculus as a debate, not simply about scientific *priority*, but also and importantly as a debate over methodological *superiority*. Taking this route, we gain a better sense of Newton's complicated attitude toward algebra: Newton accepted that algebra was a useful tool for finding out things – or, as we might put it, algebra was deemed useful for the context of discovery. But, Newton objected, algebra was less suitable for justification and, in particular, for explaining results. A geometrical method grounded in the nature of *things*, rather than in general, empty rules, might be narrower in its focus; but it would be superior on explanatory grounds, and thus superior for general epistemological reasons.

The epistemic ideals that underpin Newton's preference for geometry emerge, also, in Hamilton's philosophy of algebra. That is, though Hamilton defends algebra, which Newton avoids, and though Hamilton's general epistemological approach is rationalistic rather than empiricist, his defense of algebra is based on the goal of showing that it has a basis in truth. In some sense, in fact, he agreed with Newton that geometry was epistemologically superior, for his aim was to make algebra more like geometry. Hamilton wanted to show, specifically, that algebra need not be thought of as a mere language, or tool, or as a mere "art". Rather, algebra is a bona fide mathematical science, like geometry, and it provides a source of truth because it is a *science* with its own genuine subject matter.

Session VII.2 Carnap, Carnap, Carnap

Christopher F. French, University of British Columbia
"Reconstructing rational reconstructions in Carnap's *Aufbau*"

My paper is concerned with the role rational reconstructions have in Carnap's *Aufbau*. Although, retrospectively, Carnap associates the method of rational reconstruction with explication in the second preface of the *Aufbau* in 1961, I argue that rational reconstructions circa 1922-1925 play a different role in the *Aufbau* itself in comparison to how Carnap later understands the role of rational reconstructions and explications in the 1940s and later. For under this later understanding, rationally reconstructed concepts were meant to replace imprecise concepts in science or ordinary language. For example, it is with this replacement interpretation of rational reconstruction that Carus 2007 can interpret a nascent notion of rational reconstruction found in Carnap's early unpublished work from 1922 to 1925 as a sort of stepping stone to Carnap's mature notion of explication (e.g. see the first six sections of Carnap 1950) and, indeed, as part of a larger project in the Enlightenment tradition (2007, 181). For example, Carus tries to locate the notion of rational reconstruction in Carnap's unpublished transcript *Vom Chaos zum Wirklichkeit* in terms of a 'clarificatory' and 'constructive' task for which he argues are analogous to the context of discovery and justification distinction (2007, 164).

The aim of my paper is to argue instead that rational reconstructions in the *Aufbau* should be understood in a much more narrow and technical sense than explications or Carnap's use of method in his later works. In particular, I argue the role of rational reconstructions in the *Aufbau* are not so much meant to replace old concepts, but merely to clarify and reorganize scientific concepts within a formal structure (what Carnap calls a "constitutional system"). More specifically, their role is to facilitate the construction of a unified science, by capturing the rational parts of concepts and transforming them into formal, objective concepts in a constitutional system. While explications take place somewhere between everyday language and the language of science, rational reconstructions in the *Aufbau* are just the process of introducing constitutional definitions within a particular constitutional system. Furthermore, if we try to characterize rational reconstructions as taking place in two separate stages, as a 'clarificatory' and 'constructive' task, we risk interpreting reconstructions as a static process. For if we understand rational reconstructions as just a part of constitutional theory then reconstructions are an on-going, dynamic process (e.g. see § 147 in the *Aufbau*).

In sum, I argue that once we understanding the role of rational reconstructions in the *Aufbau* as merely the reordering of concepts in a formal system separate from the language of science, it is not as straightforward as Carus would like to assimilate that notion with Carnap's later work on explications and language planning.

Georg Schiemer, Ludwig-Maximilians-Universität Munchen
"Carnap's mathematical structuralism"

Carnap's philosophy of mathematics is usually identified with his adaption of a Fregean or Russellian logicism (e.g. Carnap 1930) and, more importantly, with his principle of tolerance first formulated in *Logische Syntax der Sprache* (Carnap 1934). However, recent scholarly work has shown that Carnap also made significant contributions to the (meta-)theory of formal axiomatics, in particular in his unpublished manuscript *Untersuchungen zur allgemeinen Axiomatik* (Carnap 2000), written around 1928. While his early metalogical work presented there has been investigated in detail (e.g. Awodey & Carus 2001, Reck 2007), no closer attention has so far been dedicated to the structuralist account of mathematics underlying Carnap's "general axiomatics."

This talk will investigate Carnap's mathematical structuralism developed in the late 1920s and early 1930s. The aim here will be twofold. First, to present and clarify Carnap's main ideas concerning the structural properties of mathematical theories as documented in his published and unpublished work. Second, to reevaluate Carnap's contributions in light of modern theories of mathematical structuralism. Specifically, the aim here will be to see to what extent Carnap's original account can be made relevant for the modern discussion. A central motivation underlying Carnap's work on general axiomatics in the period in question was to make precise in logical terms, i.e. in a simplified type theory based on *Principia Mathematica*, the structural content of formal theories. Two notions investigated by him illustrate this fact. The first is based on the idea - first expressed in *Abriss der Logistik* (Carnap 1929) - that axiom systems not only implicitly define the primitive terminology of a theory. They also specify "explicit concepts" ("Explizitbegriffe") that determine the class of models the theory is true in. Carnap, on several occasions, refers to the Explizitbegriff of a mathematical theory as an abstract structure, shared by all of its models.

Roughly at the same time, in his *Untersuchungen*, a more refined account of the structural content of mathematical theories is developed in terms of so-called "model structures." These are defined in type theory by the use of abstraction principles. A theory therefore not only specifies a single abstract structure, its Explizitbegriff, it can also possess a number of distinct model structures.

In the first, historical part of the talk we investigate Carnap's two accounts of mathematical structure. In particular, we consider how the two notions are interrelated and also how Carnap's early mathematical structuralism is connected to his more general structuralist conception of scientific theories in *Der Logische Aufbau der Welt* (1928) (see Richardson 1998, Friedman 1999).

In the second, more systematic part of the talk, Carnap's early structuralist approach is reevaluated in the light of the current debates on mathematical structuralism. We attempt to locate his views on the ontological status of abstract structures in the spectrum of modern (e.g. eliminative and non-eliminative) versions of structuralism (Reck & Price 2000). Finally, we argue that a bottom-up account of structural properties based on Carnap's 1928 definition gives a more sensitive reconstruction of the "structuralist methodology" in mathematics than the philosophical versions of structuralism currently under discussion.

Matteo Collodel, Humboldt Universität zu Berlin
"The Neurath-Carnap disputes: Carnap's final attempt at their dissolution"

It is now well known – especially thanks Mormann and Uebel – that the relationship between Carnap and Neurath leaned strongly towards the negative, both theoretically and personally, as it developed in time and was abruptly interrupted by Neurath's death in 1945 at one of its lowest peaks. On the theoretical level, reasons for disagreement spanned from their different versions of scientific philosophy (Carnap's Wissenschaftslogik aimed at exploring the boundless ocean of logical possibilities open to logico-linguistic framework building vs. Neurath's Gelehrtenbehavioristik focused on the actual or empirically realizable linguistic and inferential practices of science), to their diverging models of a unified science and its language (Carnap's hierarchy of reductively related theories formulated in the universal language of theoretical physics vs. Neurath's encyclopaedia of collaborating disciplines making use of a physicalistically cleansed but still constitutively imperfect universal jargon), to their opposing attitudes towards formalization in general and semantics in particular (Carnap's acceptance of Tarski's semantic conception of truth vs. Neurath's qualms about the metaphysical flavour of very concept of truth).

Whereas Mormann has emphasized the elements of incompatibility and outright conflict between Carnap's and Neurath's views, Uebel has strived after their reconciliation in terms of a division of metatheoretical labour within a common collaborative programme, reconstituted with the help of minor adjustments. The working hypothesis here advanced is that if the scope of the interpretative analysis is not limited to the disputants' positions up to Neurath's departure, then Carnap's mature idea of scientific philosophy, when examined in due detail, can be read as an attempt at a dissolution of their disputes, however implicit.

Indeed, it was exactly in the mid-40s that Carnap started elaborating an improved formulation for the aim and method of scientific philosophy, i.e. "explication" and "conceptual engineering", respectively. According to this view the theoretical work of philosophy amounts to the proposal of replacing concepts used in scientific or everyday language only in a vague or otherwise non completely rigorous way (explicandum) by their exact and precise redefinitions within systematic logico-linguistic frameworks (explicatum). It seems clear that Carnap's novel standpoint presupposes his full recognition of the non-Cartesian nature of ordinary as well as scientific languages, i.e. their being infested with congested concepts with fuzzy edges (Neurath's Ballungen). Moreover, the requirements for a good explicatum indicated by Carnap (its exactness, simplicity, resemblance to the explicandum and fertility) restrict the space of logical possibilities available to conceptual engineers within the range of those viable to linguistic and scientific practice. Finally, also in the light of Carnap's earlier acknowledgement of the incommensurability of linguistic frameworks, it is to be excluded that he conceived of the pragmatic acceptance of the conceptual engineers' proposals as a progressive, piecemeal amendment of everyday or scientific language towards an ideal constituted by a single final linguistic framework. Thus, under Neurath's stimulus and by integrating his perspective, Carnap not only drastically tempered the strong formalism of his syntactic and semantic phases, but he also arrived at a synthesis which acknowledged the fundamental empirical and pragmatic nature of scientific philosophy.

Session VII.3 Anglo-American HOPOS

Trevor Pearce

"Evolution in the Metaphysical Club: Wright and Fiske on Darwin and Spencer"

The "Metaphysical Club" of Cambridge, Massachusetts, which began meeting in the early 1870s, has been the topic of a great many discussions in the history of science and philosophy. Philip Wiener argued long ago that evolutionary ideas were an important influence on the club's members. The notion of evolution was most frequently associated at this time with two names—Charles Darwin and Herbert Spencer. Many of those who attended the club's meetings, however, were ferociously critical of Spencer even though they supported Darwin's ideas. In this talk, I will explore the reasons for this critical attitude by contrasting the work of two club members, Chauncey Wright (a critic of Spencer) and John Fiske (a follower of Spencer). Both Wright and Fiske wrote reviews of Spencer and Darwin's books as they appeared; they disagreed in person and in print about the relative merits of the two British thinkers, but still praised one another's work. I will argue that their divergent attitudes toward Spencer stemmed from their different accounts of philosophy and its relation to natural science.

Thomas W. Staley, Virginia Tech

"The 'Scratch Eight', Aristotelians, Metaphysicals, Mind and more: An exploration of late Victorian philosophical institutions and their context(s)"

In late Victorian Britain, philosophy-as-such was in a period of significant reorganization. In particular, philosophical activity in the recognized 'mental' and 'moral' domains was under pressure from many competing institutions. Most obviously, the rise of positive psychological science threatened to subsume subject areas that self-identified philosophers had previously regarded as uniquely their own. Additionally, pressures from religious, aesthetic, sociopolitical, and even occult organizations made traditional philosophy vulnerable to a loss of cultural authority.

In this paper, I examine some of the new philosophical institutions and organizational forms that arose in this environment to preserve philosophy as a unique endeavor. These include the foundation of formal groups such as the Metaphysical Society and the Aristotelian Society, the inauguration of scholarly journals such as *Mind*, and educational initiatives in British universities, as well as more ephemeral groupings such as the London-based 'Scratch Eight' dinner discussion group led by Shadworth Hodgson and occasionally attended by William James.

Using the 'Scratch Eight' as a central organizing example, and tracing its membership into various other new contexts of intellectual activity, I explore the multiple social affiliations of typical late Victorian philosophers and the relationship of these affiliations to the conceptual work such philosophers were engaged in at the time. I demonstrate parallels between the arguments in the written work of these figures and their varying institutional commitments and conceptions of philosophy as a social endeavor. I also discuss the complex interrelationships between philosophy and science, religion, politics, and other cultural domains in this period and the active effort involved in maintaining philosophy as its own distinctive field in the environment of High Modernism at the close of the nineteenth century. I construe these cases as instructively constrasting with more recent organizational forms such as the paradigmatic behavioral sciences and disciplinary academic philosophy.

Alexander Klein, California State, Long Beach

"Russell's external world program and the psychology of spatial perception: The significance of James"

Just what was Russell trying to accomplish in Our Knowledge of the External World? That work uses logic to "construct" the world of material bodies out of sense-data. According to a new reading, Russell aimed to show that the apparently disparate theories of modern physics and psychology are in fact logically consistent (see Pincock 2006). My paper seeks to develop this reading by investigating the nature and depth of Russell's engagement with the empirical psychology of his day.

In Our Knowledge, Russell distinguished beliefs that are based on what is given directly in sensation—beliefs he called "psychologically primitive"— from those that are causal results of post-sensory mental processing—beliefs he called "psychologically derivative." He held that when we reflect, we lose confidence in psychologically derivative beliefs unless we can logically deduce them from others that are psychologically primitive (Russell 1914/2009, 55). A key example is a belief in the existence of material bodies that persist in time and space even when they are not being observed. The way we draw this distinction between what is psychologically primitive and derivative is a matter for empirical psychology to decide, Russell often suggested (Russell 1914/2009, 54, 90, 1921/1995, 140). Now, his constructions relied on several then-controversial assumptions about what is and is not given in sensation; but Russell gave few direct indications of which psychological theories he was drawing from to support those assumptions. Readers are left to wonder how seriously to take Russell's repeated claims that it is an empirical, scientific matter to determine what is really given in sensation.

I argue that Russell's construction of material bodies makes crucial use of two substantive assumptions drawn from (James 1890)—that spatial order is given natively in raw sensation, and that our sensory fields are spatially and temporally continuous rather than atomistic. The combination of these two assumptions distinguished James's heretical account of spatial perception from the main alternatives on offer at the turn of the 20th century—Helmholtz and Hering's contrasting versions of atomism.

What do we gain from this revelation, if correct? First, my reading helps make sense of why Russell should be at such pains to offer a detailed logical construction of spatial and temporal points in private space (Russell 1914/2009, 90-99)—for either Helmholtz or Hering, these would simply be part of raw sensation. Second, the reading helps correct the mistaken view that deliberation about empirical psychology played little role in early analytic philosophy.

Session VII.4 Varia

Eric Palmer, Allegheny College

"'A wise disposition of nature': Finding purpose in early modern explanation"

Purposes, including divine purposes, have held radically shifting status and justification in the history of natural explanation. The philosophical grounding for the rise of such explanation in the early modern period is the subject of this presentation.

This form of explanation experienced a great rise, beginning about the middle of the seventeenth century. Purposes held poor standing in methodological discussion in the early seventeenth century. Francis Bacon approved Ecclesiastes 3:11: "The work which God worketh from the beginning to the end, it is not possible to be found out by man." Descartes, perhaps chastened by the Galileo affair, concurred. At the midpoint of the following century, however, Carl von Linné (Carolus Linnaeus) applied such explanation liberally. In 1749, he and his student Isaac Biberg observed that "Goats … have feet made for jumping," and that "care is taken that [dogs] should exonerate upon stones, trunks of trees, or some high place, that vegetables may not be hurt by them. Nothing is so mean, nothing is so little, in which the wonderful order, and wise disposition of nature does not shine forth." Such explanation runs seamlessly from the purposes of the parts of animals ("feet made for jumping") to the activities of animals in their relation to other organisms, placed within a providential ordering of nature. The explanation also embraced geology, astronomy and anthropology in its furthest extensions, in what might be considered a project of unified science suited to the presuppositions of the times. That project would come to be called "physico-theology," following William Derham's Boyle lecture of 1711, and would retain real explanatory significance in many sciences through the second quarter of the eighteenth century.

The rising fortunes of physico-theology may be explained by developments in the philosophy of science to the third quarter of the seventeenth century, glimpsed in two distinct movements. The first movement is under development before Bacon's writing and is evident in Galileo, who confronts Bacon's claim regarding impenetrable divine mystery in his familiar renegotiation, in the *Letter to the Grand Duchess Christina*, of the relationship between the scholars of God's two books (the Bible and Nature). Galileo also lays aside the Renaissance view that nature is a book of moral lessons for humanity composed by God. These are components of the rise of a new, modern theological sensibility concerning nature that was taken up by natural philosophers.

A complementary modern philosophical sensibility that reframes the role of the divine in nature succeeds the careers of Galileo, Bacon and Descartes. It arises in a coupling of new British theories of the understanding with the empiricist and mechanist natural philosophies. The shift is most evident in John Wilkins' *Principles and Duties of Natural Religion* (1675), and is well rooted by the time of Locke's *Essay* (1690). Henry Power, Abraham Cowley and John Ray also play significant roles, as does Robert Boyle, whose death in 1691 yielded the bequest that established the Boyle lectures, the fruits of this methodological development.

John Barresi, Dalhousie University

"British psychology as an empirical science in the eighteenth century: Pneumatological lectures of Grove, Doddridge, Reid, and Belsham"

During the 18th century, science and religion were often partners in the advance of knowledge and enlightenment. An example of this cooperation occurred in the development of an early modern version of psychology. After the collapse of Aristotelian metaphysics of soul as living principle in the seventeenth century and its replacement with Cartesian metaphysics where the soul became conscious mind, room was made for a novel approach to the investigation of soul. In addition to continued discussion of metaphysical issues, such as whether the human soul was immaterial or material, which Christian Wolff termed 'rational psychology', there developed an interest in the activities of the embodied soul, or 'empirical psychology' in Wolff's terminology. Since the soul as mind was thought to be directly accessible to reflexive self-consciousness, an empirical science of the soul could be developed through an investigation of consciousness and its activities. It was thought that rational and empirical psychology would complement each other in the service of both religion and science.

This collaboration between science and religion in advancing psychology, as an empirical science, was especially apparent in liberal Protestant institutions in Great Britain. While Aristotelian psychology was still being taught at Oxford, early in the 18th century in Dissenting academies as well as in colleges in Scotland, a radical transformation was occurring in courses on pneumatology. Traditionally, pneumatology, or the science of spirits, focused on metaphysical and religious notions of the human soul, angels & demons, and God. But right from the start in 18th century, there was an understanding that the human soul could be approached naturalistically, and in natural theology, God as well. So courses were developed in pneumatology at these institutions that not only tracked the development of this new version of pneumatology, but through publications, teaching faculty also participated in its development. This was especially the case for the development of an empirical approach to soul as mind, or mental science.

In the present talk I will trace the development of courses in pneumatology in Britain through the lectures of Henry Grove (1684-1738), Philip Doddridge (1702-1751), Thomas Reid (1710-1796), and Thomas Belsham (1750-1829). Each of these ministers taught pneumatology at academies or colleges during different quarters of the century. Each also contributed to the rapidly transforming literature in English on philosophy of mind and moral philosophy. By the end of the century there were two main opposing positions represented here by Reid, who maintained a dualist orientation toward mind and matter, and Belsham, who was a materialist follower of Hartley and Priestley. In the confrontation of these opponent views, the collaboration between religion and science came to an end. While Reid's orientation toward psychology, which was compatible with traditional religious beliefs about an immaterial soul, would maintain a dominant position through most of the 19th century, a materialistic science of psychology would eventually overtake it in the 20th century.

Ina Goy, Universität Tübingen
"Kant on formative power"

The notion of a formative power is one of the most obscure in Kant's theory of biology. Before I discuss Kant's biological use of the term 'formative power', in section I of the paper I provide a list of all passages in which Kant uses the term, claiming that the older meaning of 'formative power' in Kant's writings is an epistemological one, whereas the biological meaning of the term appears not before the mid-1780s. I present and discuss some of those passages in closer detail, and give a precise interpretation of the most central passage in Kant's philosophy of biology in §65 of the *Critique of the Power of Judgment* (5:374.21–26). I defend the view that, for Kant, the formative power is a basic and immaterial power in the organism belonging to an account of final causation. As a cause, it does not generate form and matter, or the matter of organisms, but only the end-directed teleological form of the matter of an organism. As an alternative to White's (1997, 137) claim that 'form' means species, and Richards (2000, 28) opinion that 'form' is a synonym for 'archetype', I defend the view that 'form' means the intentionality and (necessary) directedness of the features of a being towards the idea of its purpose.

Reading the formative force as form-giving allows for a more careful analysis of Kant's famous tree example in §64, and of his central statements on the part-whole relationship in organisms in §65, which I investigate in section II. The self-generation of a tree with regards to its species, as an individual and in its parts, does not imply in general the generation of form and matter of a tree, or in particular the generation of its matter, but only the causation of the form of the matter of a tree. In section III, I briefly outline consequences of my interpretation for a placement of Kant's position within supernatural preformistic and naturalistic epigenetic accounts of organic generation. I claim that although the formative power as a form giving capacity in the organism is a natural epigenetic power, this does not rule out a supernatural preformistic interpretation of the creation of matter, and also not a supernatural creation of the formative power. The formative power of nature can be read as a secondary cause in support of the primary cause of God's creation, and Kant's position as mediating between preformism and epigenesis.

Parallel Session VIII

Session VIII.1 Symposium: Kant, Leibniz, and the foundations of the exact sciences

The Leibnizian inheritance present in Kant's early philosophy has long been recognized (see Laywine 2003 and Schönfeld 2001). Recent work on Kant's 'Critical' thought, however, has sought to demonstrate that the mature Kant actually accepts many more of the metaphysical and epistemological commitments held by those in what Kant calls the 'Leibniz-Wolffian school' than has typically been allowed. (See, for example, Langton 1998, Ameriks 1982/2000, and Jauernig 2008 and 2011.) Indeed, Kant himself encourages such an analysis by claiming, in his 1790 debate with Eberhard, that the Critical philosophy is the true apology for Leibniz (AA 8:250).

Our own symposium will aim to extend and yet also critically evaluate this approach to Kant's mature views via 'the Leibniz-Wolffian school', by shifting focus to Kant's Critical conception of the foundations of the exact sciences: logic, mathematics, and conceptual foundations of physics. The Leibnizian threads in Kant's logic and mathematics unfortunately continue to be severely under-discussed, with the Leibnizian features of Kant's foundations of physics only more recently getting the attention it deserves. Our symposium, therefore, aims to broaden the current interpretive focus to include these key aspects of Kant's philosophy, with the larger hope of beginning to determine Kant's nearness to the Leibnizians on questions of the methodology and structure of science more generally. Throughout, we will also aim for richness, nuance, and subtlety, by looking at the various, and sometimes conflicting, voices present within both the 'Leibniz-Wolffian school', as well as the 'Kantian school', addressing texts by Wolff, Meier, Euler, Lambert, Eberhard, Kästner, Schultz, Kiesewetter, and others.

First, building off of recent work that begins exploring the influence of the Leibnizian tradition upon Kant's mature philosophy of logic (e.g., Anderson 2005), Tolley identifies several further though under-appreciated points of continuity, concerning logic's objectivity and absence of existential commitment (pace MacFarlane 2001), though Tolley raises worries for any attempt to too closely assimilate the two, due to Kant's ultimate rejection of Leibniz's strategy for aligning logical structure with both the structure of divine understanding and (hence) the metaphysical structure of transcendental reality.

Second, again further developing still-nascent work on the relation between Kant and his Leibnizian predecessors on the philosophy of mathematics, particularly geometry (Anderson 2005, Dunlop 2009 and forthcoming, Sutherland 2010), Heis focuses on the neglected debate between the Kantians and the Leibnizians over the proper interpretation of Euclid's axiom of parallels. Heis uses this as a case-study to help tease out what is, and is not, at issue in the Kantian departure from the Leibnizians in the appeal to pure intuition at the foundations of geometry.

Third, Stan aims to extend recent attempts to re-orient our understanding of Kant's Critical views on the foundations of physics (such as Watkins 2005), through pointing to their nearness to those put forward in the Leibniz-Wolffian tradition, in order to caution against an overhasty assimilation of Kant's position to either Newtonianism or to more recent Humean positions. Stan explores an important, internal line of development from Wolff's dynamics to Kant's divergence from Newton, helpfully placing Kant amidst fierce foundational debates in post-Leibnizian Germany, in which broadly Leibnizian brands of metaphysical dynamics sought to displace Newton's mechanics as it arrived on the Continent.

Clinton Tolley, University of California, San Diego
"Kant, Leibniz, and the metaphysical foundations of logic"

In the recent resurgence of interest in Kant's relation to Leibniz, considerable attention has been devoted to the striking possibility that Kant's mature ('Critical') metaphysics maintains several – though, of course, not all – of the key metaphysical commitments of what Kant calls the 'Leibniz-Wolffian school'. (See especially Ameriks, Langton, Watkins, and Jauernig.) What has been left mostly under-explored, however, is the possibility that a similar, substantial – though, again, surely not total – degree of continuity obtains concerning Kant's views on logic. In light of the central role that logic has long been recognized to play within Leibniz's own metaphysics, it would be surprising if the degree of Kant's continuity with Leibnizian metaphysics was not paralleled by a continuity with its logic as well. What is more, Kant used the work of a Leibnizian philosopher (Georg Meier) for the textbook for his logic lectures, and in these lectures, Kant is reported to have said that another Leibnizian's work on logic (Christian Wolff) is 'the best we have' (AA 9:21; cf., AA 24:797). Perhaps, then, deeper research into Kant's views on logic could further confirm this crypto-Leibnizian interpretive thesis?

I will argue that, in fact, a surprising degree of continuity does exist between Kant and the Leibnizian tradition on the nature of logic, and, moreover, that seeing the points of continuity is quite instructive for coming to understand Kant's mature views on logic. This is so particularly on the question of the objectivity of the domain of logic (what both call 'understanding' or the realm of concepts) as well as this domain's absence of existential commitment (to actual individuals). I will also argue, however, that Kant rejects both Leibniz's strategy for providing metaphysical foundations for logic in the divine understanding as well as Leibniz's related thesis of the coincidence of logical structure with the metaphysical structure of what is transcendentally real. In the Critical period, Kant does not accept that the divine understanding is structured according to concepts, and so is not committed to a view according to which what is transcendentally real will have a structure that is isomorphic, in any interesting sense, to the conceptual structure on display in logic. Recognizing such distance on the metaphysical foundations of logic, then, should give us pause before trying to align Kant's metaphysics itself too closely with that of the Leibnizians.

My presentation will build off of – as well as critically evaluate – the few studies that have been done on this line of influence (by Tonelli, and more recently, Pozzo, Anderson, MacFarlane, and others) to help fill in this key piece of the picture of the historical development of Kant's thought out from its Leibnizian heritage. I will also make use of classical work on Leibniz's philosophy of logic (Bolzano, Russell, Couturat), more recent work on this topic (Lenzen, Mugnai, Lenders), and other recent attempts to take the measure of Kant's philosophy of logic (Hanna, Maddy), in particular those who have tried to draw Kant closer to Leibniz on questions of logical modality and logical truth (R.M. Adams and Watkins).

Jeremy Heis, University of California, Irvine
"Leibniz versus Kant on Euclid's axiom of parallels"

It is well known that geometrical research on Euclid's axiom of parallels led at the end of the nineteenth and beginning of the twentieth century to a fierce philosophical debate about the tenability of Kantian philosophy of mathematics. In particular, many philosophers – starting with Russell and Couturat – believed that the consistency of Non-Euclidean geometries confirmed Leibniz's claim that the axioms of geometry are derivable from definitions, and so are not synthetic as Kant claimed. What is less known is that a similar debate about Euclid's axiom broke out already in Kant's lifetime, drawing in many of the leading German philosophers and mathematicians. Indeed, as I will argue, Euclid's theory of parallels became a test case for evaluating the tenability of Leibniz's and Kant's rival philosophies of geometry. The purpose of this talk is to give an overview of this debate: who were the main participants? And what were the main issues of contention?

Among 18th century German philosopher-mathematicians, it was the consensus view that Euclid's axiom of parallels – though clearly, they thought, a true proposition – was not a legitimate axiom. It was also well known that Euclid's axiom could be derived from other propositions, like Wallis's Axiom (that there are two similar triangles of different size), Clavius's Axiom (that a line everywhere equidistant from a straight line is itself straight), or Lambert's Axiom (that space has no absolute measure of length). But are any of these new axioms themselves legitimate? And why are they any better (or worse) than Euclid's own? Answering this question clearly required answering a prior philosophical question: what is an axiom? This distinctly philosophical debate, which was already ongoing in the 17th century works (like those of Saccheri and Leibniz himself) that 'corrected' or 'defended' Euclid, was inflamed by the raging debate within 18th century Germany over the proper ways of characterizing the methods of mathematics and philosophy (for instance in the works of Christian Wolff, Kant, and Johann Lambert).

The debate between the Kantians and Leibnizians over the proper philosophical interpretation of Euclid's axiom reached its high point in the Kant-Eberhard controversy, with the mathematician Abraham Kästner (in Eberhard's Philosophical Magazine) arguing that Euclid's axiom could be demonstrated only with an improved definition of "straight line" and the mathematician and Kantian disciple Johann Schultz (in his Prüfung) arguing that the axiom could be demonstrated only with an improved way of constructing the size of angles in pure intuition.

As I argue, this debate – though fascinating in its own right and surprisingly not well known – was just a culminating episode in a long controversy that drew in Leibniz, Wolff, Lambert, W.J.G. Karsten, J.G.C. Kiesewetter, and Kant himself. In telling this story, I draw on recent work (by Judson Webb, Katherine Dunlop, Vincenzo De Risi, and Gideon Freudenthal) that discusses some of these figures individually.

Marius Stan, California Institute of Technology
"Leibniz and Kant on the relativity of motion and the law of inertia"

Newton famously identified true motion with motion in absolute space. Just as famously, Leibniz denied absolute space, claiming instead that motion is 'relative.' Kant too rejects Newtonian absolute space and argues that motion is 'relative.' Is that a mere coincidence or did Leibniz shape Kant's views? I argue here for indirect influence: Kant crafted his concept of motion so as to solve internal problems in a Leibnizian tradition of impact dynamics.

In § 1, I untangle the intricate Leibnizian account of motion. In Leibniz's middle years, motion is relativistic: a body always moves or rests relative to other bodies, but none of these motions is true or privileged. Later, Leibniz took motion to be relational: bodies do have true motions, after all, but always relative to other bodies. However, it is unclear what those bodies are, in his doctrine. In particular, Leibniz does not explain relative to which bodies or material system a body free of impressed forces remains at rest or moves uniformly in a straight line, as the Law of Inertia has it.

This obscurity persists among Leibniz's followers in Germany, notably Chr. Wolff, as I show in § 2. Kant first clarifies it, in his *New Doctrine of Motion* (1758). There, he defines (true) motion as the kinematic relation between colliding bodies. This relationist analysis allows him to derive a priori laws of motion and outline a metaphysical impact dynamics which improves on the consensus Wolffian account at the time. Kant's early relationist survives in the Critical period, where it becomes his account of 'necessary motion' in *Metaphysical Foundations of Natural Science*. Leibniz's influence amounts to making impact the paradigm case that Kant aimed to ground with his concept of relative motion.

In § 3, I uncover two problems for Kant exegesis. (1) Kant's theory of motion allows violations of the Law of Inertia. Euler had first pointed out this weakness, in a paper that Kant demonstrably knew. It remains a mystery why Kant did not heed Euler's warning. (2) In Kant's Critical doctrine, it is unclear relative to what a free body would move uniformly or stay at rest. One answer, stemming from Michael Friedman, comes at a high price for Kant. I offer another, which, though not based on direct textual evidence, is nevertheless at home in Kant's mature metaphysics. Yet not even this answer can avoid the weakness in (1) above. I conclude that Leibniz's and Kant's theories of relative motion retain a blind spot that Newton's absolute space was meant to avoid.

Session VIII.2 Fleck, Neurath, and social philosophies of science

Artur Koterski, Maria Curie-Sklodowska University
"Fleck's anti-relativism in his polemics with Bilikiewicz"

The first attempts made by scientists themselves to understand and depict the cultural dependency of science started in the early 30s of the last century. Among the pioneers of 'scientific' sociology of scientific knowledge one finds such a luminary as Schrödinger with his paper 'Ist die Naturwissenschaft milieubedingt?' (1932), however, much more significant results were reached by two Polish physicians: Tadeusz Bilikiewicz and Ludwik Fleck.

In his history of embryology of 17th and 18th century, i.e., *Die Embryologie im Zeitalter des Barock und des Rokoko* (1932), Bilikiewicz describes the development of culture, which, of course, includes science and philosophy, as a process guided by the epoch-specific zeitgeist. It sets the ideals that are followed in any branch of cultural activity. These ideals are so strong that they influence the choice and content of scientific theories, they determine selection and interpretation of empirical data, and indicate the scientific aims and scope of problems to be solved (or, to put it in a Lakatos' term—the logic of scientific discovery).

In *Entstehung und Entwicklung einer wissenschaftliche Tatsache* (1935) Fleck also indicated sociological factors that influenced scientific theories and activities. However, he criticized Bilikiewicz's approach and set it against his own theory. It started a polemical exchange between them just before the outbreak of the World War II—a short one but highly relevant to contemporary discussions over alleged relativism of Fleck's conception.

The aim of this paper is twofold. Firstly, it attempts to reconstruct Bilikiewicz's views on science in order to understand and outline the Fleck-Bilikiewicz argument. Secondly, with such a background it tackles the claim of Fleck's relativism and questions its validity so far as it ascribes more than a form of descriptive relativism to him.

Katherine Arens, University of Texas, Austin
"The science debate comes to the US: *The International Encyclopedia of Unified Science*"

The *International Encyclopedia of Unified Science*'s story is familiar: after publishing a short series of monographs in Europe in 1938 under the title *Einheitswissenschaft*, members of the Vienna Circle relaunched their project in the US under this title. Rudolf Carnap and Charles Morris, in the 1969 edition's preface, attributed the original concept to Otto Neurath and indicated that the series was to contain 26 volumes (260 monographic entries). Yet only two volumes (twenty monographs) were completed between 1938 and 1969, which were nonetheless significant for moving the Vienna Circle's project to the US and for naturalizing it within the contexts of US philosophy and philosophy of science.

What this narrative underplays is the provenance of the project from an older epistemological project: the debates in nineteenth-century German philosophy about the difference between the *Naturwissenschaften* (the sciences) and *Geisteswissenschaften* (the humanities), or between *Gesetzwissenschaften* (jurisprudence) and *Geschichtswissenschaften* (history, historiography). The history of philosophy takes Windelband's differentiation between nomothetic and idiographic sciences (rule-bound, most often natural science and descriptive, mostly humanistic studies, factoring in values) as central to the work of the Baden (or Southwest) School of Neokantiansism, a distinction continued in Heinrich Rickert's *The Limits of Concept Formation in Natural Science: A Logical Introduction to the Historical Sciences* which distinguished natural and cultural sciences. The counterweight is the so-called Marburg School (especially Hermann Cohen) which stresses a more anti-psychologistic epistemology, moving beyond the subject as a reference point.

The proposed paper will compare the epistemological frameworks of these two projects (the *Encyclopedia* and the *Wissenschaftsdebatte*, especially the Baden School) to argue them as a coherent, evolving epistemological project, with Neurath's work on isotypes and the Vienna Circle Manifesto as particular reference points. The result will situate the *Encyclopedia* project as less a project aimed at introducing the Vienna Circle to the Anglo-American circles, and more as an attempt to chart the genealogy of its epistemology. Most significantly, I will argue that the American voices included in it, especially Bloomfield and Morris in the language sciences, Dewey in values, and Kuhn in dealing with the practical epistemologies of the pure sciences, represent a kind of parallel evolution to the Vienna Circle's own. Their projects also refer back to both continental debates and to the kind of epistemological critiques familiar to the Vienna Circle itself. Thus the *Encyclopedia* is, I argue, most profitably seen as an attempt to drive the Vienna Circle's project in a direction that receded in importance behind the US-based project that took Carnap and Quine, instead of Neurath and the Mach, as the way forward for systematic philosophy.

Elisabeth Nemeth, University of Vienna
"Scientific knowledge, democratic decision-making and philosophy of science: Harry Collin's 'normative theory of expertise' in historical perspective"

Harry Collins et al. (2002, 2010) distinguish three waves of science studies during the second half of the 20th Century, each of which involves a particular way of looking at the relationships between science and political decision-making: 'positivism' (from 1950 to Kuhn), 'social constructivism' (from Kuhn to 2000), Wave Three (from 2000) which is their own project to develop a "normative theory of expertise and decision-making". In contrast to the "positivistic" view (=the sciences viewed as esoteric and authoritative), Wave Two stressed that science was a social activity that depends heavily on "extra-scientific factors". It criticized top-down decision making and plead for including the democratic public in science and technology related decision-making. This, however, brought about a particular problem: "sociologists have become uncertain about how to speak about what makes scientific knowledge different; in much the same way, they have become unable to distinguish between experts and non-experts" nor to spell out what the sciences' specific role in political decision making would be (Collins et al 2002, 239). It is this problem Collins' "normative theory of scientific expertise" sets out to solve. "Wave Three involves finding a special rationale for science and technology even while we accept the findings of Wave Two – that science and technology are much more ordinary than we once thought." (Collins et al. 2002, 240)

I will put Collins' project in a historical-philosophical perspective by taking a closer look at Otto Neurath's article "The lost wanderers of Cartesius and the auxiliary motive"

[(1913) 1973]. This paper has become quite famous amongst philosophers interested in Logical Empiricism (Haller, Stadler, Uebel, Cartwright, Cat, Mormann, Stoeltzner...). However, the philosophical research focused on its epistemological impact, but not on the way Neurath addressed the relationship between science and political decision making. Yet, Neurath put the epistemological questions exactly in this particular context. He argued that the way philosophers conceive of the foundations of science has an impact on the role science can play in political decision-making. He argued – quite similarly to what Collins called Wave Two – that there is no difference in principle between the production of scientific knowledge and other human activities. For Neurath, science is a human, historical enterprise which is shaped by decisions of scientists and external factors. Remarkably, Neurath gave not only epistemological reasons for his critique of the Cartesian view of science. He also argued that the Cartesian view damages the role science can (and ought to) play in democratic politics. For Neurath, the insight that science is a historical activity is the pre-condition of a nonillusionary, rational view of science in modern societies – and therefore also a pre-condition of democracy. In this respect his view is very close to Collins'. In contrast to Collins, however, epistemology mattered for Neurath. This difference will be the starting point for questioning the way Collins et al. relate Wave Three to Wave Two, and in particular to Wave One.

Session VIII.3 History of philosophy of mathematics

Jean-Paul Cauvin, Emory University
"Leon Brunschvicg's critical idealism and the epistemology of mathematical reason"

The paper addresses the work of Leon Brunschvicg (1869-1944) in order to situate him within the context of the development of twentieth century historical epistemology of science in France during the inter-war period. The paper proposes a synoptic review of Brunschvicg's major publications in order to suggest that his methodology as a historian of science and of philosophy is modeled closely on his epistemology of mathematical reason. The paper argues that Brunschvicg's philosophical project is organized according to a strict homology between his philosophy of history and his philosophy of scientific reason. At the core of this relation is Brunschvicg's epistemology of mathematical reason. The paper demonstrates that by clarifying the aim and method of Brunschvicg's epistemology of mathematical reason his philosophy of history also comes into focus -- initiating one of the most contentious syntheses in the tradition of French epistemology, a historical epistemology of mathematical reason. Following Brunschvicg, and reconstructing his argumentation in *Les étapes de la philosophie mathématique* (1912) and *L'expérience humaine et la causalité physique* (1922), the paper proceeds by distinguishing between two possible versions of idealism in the realm of the epistemology of mathematics, one which is consistent with a Kantian critical idealism and one which proceeds according to an axiomatic method which does not deduce a priori the forms of reason so much as proceed according to a genetic or constructivist model. The paper demonstrates that Brunschvicg's second form of idealism is the result of a rapprochement between a Kantian critical philosophy and a Spinozist philosophy of immanent causality. The historical example of the latter form of idealism is exemplified for Brunschvicg in the development of the differential calculus, a history which Brunschvicg unravels with considerable historiographic detail. The paper concludes by arguing that this instance from the history of mathematics can be read as a *locus classicus* of Brunschvicg's philosophical methodology in general and of his epistemology of mathematics in particular as it requires a simultaneous exposition of historical and epistemic conditions of possibility while also demonstrating the coherent conceptual framework uniting Brunschvicg's philosophies of history and of science.

Yvon Gauthier, University of Montreal
"Finitism from Kronecker to Gödel via Hilbert"

In this talk, I want to show that Kronecker's finitist foundations of mathematics survive in the logico-mathematical tradition from Hilbert to Gödel with the use of what I call polynomial functionals. Hilbert introduced functionals, that is functions of functions on the integers, in his 1926 paper « *Über das Unendliche* » in his attempt to solve Cantor's continuum hypothesis. Gödel reintroduced (finite-type) functionals in his consistency proof for intuitionistic arithmetic in his 1958 paper « *Über eine noch nicht benütze Erweiterung des finite Standpunktes* » known as the Dialectica Interpretation.

My historiographical hypothesis is that the idea of functional in Hilbert derives from Kronecker's theory of forms or homogeneous polynomials in his 1883 paper « *Über die Theorie der Formen höherer Stufe* » where he sketches a higher-order theory of polynomial functions for his 1882 arithmetical theory of algebraic quantities. It is well known that Hilbert has espoused Kronecker's finitism at the beginning and at the end of his philosophico-logicomathematical carrier, not without polemizing with Kronecker posthumously! Gödel's finitism is partly inspired by Hilbert and by intuitionistic restraints and since he was not satisfied with the use of transfinite induction in Gentzen's proof for the consistency of classical arithmetic, he thought of functionals as an abstract notion to reach inductively all finite types up to ω without reaching over to $lim\ \omega = \varepsilon 0$ of transfinite induction. This is in my view what Gödel meant by an « extension of the finitist view ».

Oren Magal, McGill University
"The logical in mathematics and the mathematical in logic"

The paper considers an argument (or set of related arguments) made by Paul Bernays with respect to the relationship between logic and mathematics. Bernays was Hilbert's assistant and collaborator in the foundational work that came to be known as 'Hilbert's Programme', and later became recognized a logician and philosopher of mathematics in his own right. The gist of Bernays' argument is that it is impossible to reduce mathematics to logic, not merely due to the familiar difficulties with the logicism of the Frege-Russell tradition, but due to the very nature of logic and mathematics. Since mathematics is a deductive science, it is hardly surprising to have it argued that in mathematics there is an ineliminable rôle for logic. However, the interesting thrust of Bernays' argument is that the same is true in the other direction: that in logic, there is an essential and ineliminable *mathematical* element. While examining Bernays' argument, this paper also discusses his insights regarding a centrally essential characteristic of each of logic and mathematics, and the different kinds of abstraction on which logic and mathematics are each based.

Session VIII.4 Descartes

Barnaby Hutchins, Ghent University
"The non-mechanical foundation of Descartes' mechanical physiology"

I claim that Descartes' mechanical account of the body rests on what amounts to a nonmechanical foundation: the 'fire without light'. Descartes takes himself to have a fully mechanico–physical physiology (indeed, this is required by his commitment to ontological dualism). The scholarship, too, has come to regard Descartes as the instigator of iatromechanism, and takes the unadulterated mechanism of his physiology to be its chief innovation (Voss, 1989; Des Chene, 2001). I claim, however, that Descartes' extensive mechanical physiology is underpinned by something non-mechanical. The fire without light in the heart, his principle of (animal) life, turns out to be underdetermined by mechanical explanation.

Descartes never provides an explicit account of the heat of the heart. Instead, he provides only allusions to obscure chemical processes. In *Treatise on Man*, Descartes merely states that the heart contains a fire without light, offering no further explanation. The *Discourse*'s summary of *Man* expands the account somewhat, claiming that the heat in the heart is no different from that in damp hay, or from the ebullition of fermenting new wine. Elsewhere (*Description of the Human Body, Passions of the Soul*, correspondence), he retains these allusions and adds another: the heating of a liquid by the addition of lime. He appears to associate all these processes with fermentation – the standard comparison for the heat of the heart since Galen. Comparison to fermentation has been shown to be used extensively in Descartes' physiology (Bitbol–Hespériès, 1990; Fuchs, 2001; especially Aucante, 2006): in addition to the heat of the heart, Descartes relies on it for digestion and the operation of the liver. Yet, its operation remains obscure.

With no reference to the functions of the body, Descartes considers fermentation of hay and heating by lime in the *Principles*. But the latter is addressed by vague, incomplete comparison to the former, which itself is one of Descartes' more cursory and speculative accounts of a natural phenomenon. The connections between the heat of the heart, lime, and fermentation are all left undetermined. This means that the closest Descartes comes to an explanation of his principle of life is this: a tenuous link to a superficial mechanical explanation of another chemical process. It is striking that Descartes, who explains the majority of the body's systems in intricate mechanical detail, should leave obscure the principle on which those systems depend. If he had a mechanical explanation, founded on a non-mechanical principle, probably imported from the Aristotelian and (al) chemical traditions.

Bret J. Saunders, University of Dallas
"Descartes' scientific poetics: Analysis, analogy and rhetoric in *Optics* I"

In this paper I argue that the received view of Cartesian analysis as practiced in his scientific essays needs to be revised from a literary perspective, which I believe could benefit Descartes scholarship in general, the philosophy of science, and science pedagogy. I begin by examining the series of analogies representing the nature of light with which Descartes opens his *Optics*, whereby light is compared to a blind-man's stick, wine in a wine-press and the motion of tennis balls. I argue that Descartes arranged these images rhetorically to draw his readers deeper into an *analysis* of the nature of light as the basis for the deductions and geometrical synthesis that follow. Furthermore, I show that the images of Descartes's "rhetoric of discovery" are chosen and constructed according to the distinction made in *Discourse on Method* VI between two different kinds of "experience": Descartes designs his analogies to lead us from a more common-sense perspective to a "more studied" mechanistic conception of the nature of light. Many scholars have missed the complexities of Cartesian analysis by assuming a too-rigid dichotomy between analysis and synthesis, which misses the analogical synthetic aspect *within* analysis. Perhaps this misunderstanding begins with Descartes himself: when reflecting on his method he tended to downplay the very poetics—specifically, the rhetorical presentation of analysis—that pervades his physical and metaphysical writings. For example, in a letter postdating the scientific essays, he construes his analogies as what we would call "scale" models, whereas in fact they are more like "analogous" models, or metaphors constructed from "secondary qualities," historical context and ordinary human experience.

If nothing else, this *ressourcement* of Cartesian method reminds us of the dialectical *poiēsis* (analysis) of Plato and Aristotle, and of the close association of literature and precise mathematical discourse at the birth of modern science. I believe it also suggests some criticisms of twentieth century model theory and science pedagogy, in an age where greater abstractions, automation and pragmatism often replace creativity and the wonder that was once the hallmark of all genuine *theoria*. Wonder is in part a function of a vivid imagination: here Descartes would side squarely with Campbell in his famous quarrel with Duhem over the proper construction of models. And Descartes might also criticize the tendency of model theory to dissociate problems of representation from problems of language; in other words, he might have us reconsider the distinction between models and analogies broadly construed. In sum, Cartesian poetics operates on the basis of two assumptions that are no longer widely held but perhaps need to be rediscovered: first, that in order to be fully human any method must appeal to the imagination to derive its first principles from experience; second, that theoretical models are developed, articulated, altered and passed down within a particular language community and therefore require a certain rhetoric.

Monica Solomon, University of Notre Dame
"Descartes and Newton: The influence of mathematics in conceptualizing motion"

In this paper I analyze some the influences of the mathematical methodological background of the seventeenth century on Descartes' and Newton's laws of motion. I restrict my research on detailing the reasoning behind concepts of bodies and authors' self-professed scope for the laws of motion (*viz*, I'm interested to give possible answers to the question "To what domain of entities/phenomena do the laws of motion apply?"). To this end, I will show that the practiced distinctions between pure and applied geometry, geometry and mechanics, play an important role in conceiving the entities to which the laws of motion could be applied. One of the upshots of my analysis is to approach the question: (1)"Just what exactly is 'mathematical' in the principles of physics or mechanics?"

First, I aim to show that Descartes' innovation in unifying algebra and geometry is overridden by his views on the relation between geometry and mechanics and that they bear on his manner of conceptualizing bodies and proper motions. The acknowledgment of Cartesian physics as being mathematical is in the spirit of Descartes' own professed intentions (also presented in *Geometry* and *Optics*) of accepting only the principles of geometry and pure mathematics in order to explain all "phenomena" (Descartes, *Principles* II.64). I believe there is general acceptance of the claim that Cartesian physics, while mathematical, is reducible neither to mathematics nor to pure rational mechanics. However, in the *Principles*, one can find a specific manner of abstraction. For instance, I aim to show that the clear and distinct idea of a body is reached by "stripping away" perceptual qualities in the same manner in which a geometrical line is a mental object in which length is mentally separated from breadth. The picture becomes rapidly more complex and problematic once Descartes tries to make the same ontological cut when it comes to the motions of bodies.

In contrast with the Cartesian adoption of mathematical reasoning into physics stand, in my opinion, Newton's considerations of subsuming geometry to rational mechanics. First, I trace back some of the historical influences on his view (from Cavalieri's geometry of indivisibles to Huygens' rational mechanics). Secondly, I show that the very pattern of thinking in terms of infinitesimals and relocating motion in geometry were instrumental in (a) Newton's altogether distinct manner of conceiving bodies in *De Gravitatione* and (b) are exemplified in the demonstrations for the first corollaries of *Principia*. In the end, I show the rupture between possible Cartesian and Newtonian answers to (1) by discussing a couple of examples where the central issue is the composition of motions. The examples will typify the radically different styles of conceptualizing even some of the common examples of motions.

Special Plenary Symposium: Reflections on Michael Friedman's Kant and the Exact Sciences

The appearance of Michael Friedman's *Kant and the Exact Sciences* in 1992 marked a watershed moment for Kant scholarship, in no small part because of the distinctive interpretative lens that Friedman adopts to capture the significance of Kant's philosophy. As stated in the book's Preface, Friedman's guiding thesis is that "Kant's philosophical achievement consists precisely in the depth and acuity of his insight into the state of the mathematical exact sciences as he found them" (xii). Friedman's synthesis of the history of science and Kant's philosophy separated *Kant and the Exact Sciences* from other twentieth century English-language commentaries, the vast majority of which focused on Kant's engagement with the philosophical problems surrounding skepticism and representation. This synthesis also informs the novel interpretive claims defended in the book: by granting pride of place to Kant's engagement with "the philosophical foundations of the exact sciences of his time" (xii), Friedman offers original insight into the development of Kant's Critical philosophy, the nature of Kant's philosophy of mathematics, and the importance of Newtonian science and the *Metaphysical Foundations of Natural Science* (1786) for Kant's Critical project.

Now twenty years after its initial publication, it is a fitting time to take stock and reflect on the book's continuing significance both for Kant scholarship and for the practice of history of philosophy of science. By attending to the impact *Kant and the Exact Sciences* has had on debates surrounding Kant's mathematics, natural science, and philosophy, the papers in this symposium will examine the lessons we can draw from Friedman and from the two decades of critical attention that *Kant and the Exact Sciences* has attracted. Collectively, our goal is to offer insight into how we might continue to push our current conversations about Kant forward and also how, following Friedman, we might continue to bring mathematics, science, and philosophy into fruitful dialogue.

Emily Carson begins the session by revisiting a recent exchange between Friedman and Béatrice Longuenesse concerning number and synthesis in Kant's mathematics, and she uses this debate to offer a new perspective on the role of mathematics in the development of Kant's Critical philosophy. Marius Stan turns attention to Kant's philosophy of physics, and critically extends some of Friedman's proposals concerning Kant's commitment to Newtonian mechanics and Kant's reading of motion, in particular. Robert DiSalle continues discussion of Kant's relationship to Newton and compares their views of the foundations of mathematical physics. DiSalle suggests that Newton's treatment of problems surrounding the interpretation of space, time, and force lays the foundation for a post-Kantian understanding of the a priori in applied mathematics. Finally, Michael Friedman will respond to Carson, Stan, and DiSalle by indicating some of the new directions his work has been taking since *Kant and the Exact Sciences* was first published. Among other things, Friedman will comment on Kant's *Metaphysical Foundations of Natural Science*, Kant's relationship to Newton and to Leibniz, and Kant's theory of geometry.

Emily Carson, McGill University
"Kant, quantity, and figurative synthesis"

One of the most significant contributions of Michael Friedman's 1992 ground-breaking *Kant and the Exact Sciences* was to articulate the significance of Kant's reflection on the mathematical sciences in the development of his Critical philosophy. Like Friedman, Béatrice Longuenesse has emphasized the importance of Kant's engagement with mathematical thinking in her *Kant and the Capacity to Judge* (see especially pp.30ff). In this paper, I explore this general theme by revisiting an exchange between Friedman and Béatrice Longuenesse about the relation between the logical forms of judgement, the categories of quantity and the concept of number. One of the many striking conclusion of Longuenesse's *Kant and the Capacity to Judge* is that "in relating number to the pure concept of quantity and the latter to the logical quantity of judgments… Kant thus appears strikingly close to Frege's view that numbers are properties of concepts, namely [that they] attach to collections of individuals falling under the same concept" (KCJ, p. 201). Against this, Friedman objects that this conception of number gives priority to the discrete over the continuous, and thereby makes it difficult for Kant to account for the mathematics of continuous quantity. Instead, Friedman claims that for Kant, the mathematics of continuous quantity is primary: "number is conceived in terms of the addition of line segments with an arbitrarily chosen unit, say, rather than in the Fregean style in terms of the extension of concepts" (Friedman 2000, p.206).

Longuenesse attributes Friedman's objection here to his lack of attention to the role she gives to figurative synthesis in the Transcendental Analytic. I will argue, to the contrary, that Friedman's criticism brings out a difficulty in her interpretation of the notion of figurative synthesis in accordance with the categories of quantity. Longuenesse argues—correctly, I think—that Kant's notion of synthesis must be understood against the background of his reflection on the model provided by mathematical thought. For her, however, the salient feature of mathematical thought is "its apriori generation of multiplicities, which may be represented as multiplicities of objects to be thought under concepts" (KCJ, p.33). This leads to her view that "forming the concept of number depends on constituting sets of objects thought under the same concept" (KCJ, p.257). I argue that a correct understanding of the sense in which mathematical thought provides a model for the notion of synthesis supports Friedman's view of Kant's conception of number, against Longuenesse's Fregean reading. In doing so, however, I also argue for a *different* role for mathematics in the development of Kant's Critical philosophy from that presented so persuasively by Friedman in *Kant and the Exact Sciences*.

Marius Stan, California Institute of Technology
"Physics in *Kant and the Exact Sciences*: Twenty years later"

Since its appearance in 1992, Friedman's *Kant and the Exact Sciences* has offered us a fruitful and sophisticated strategy for understanding the philosophy of physics that characterizes Kant's work. By taking a contextualist approach that places Kant's philosophy in dialogue with the scientific achievements of his day, and by also bringing attention to the technical uses of scientific concepts in the Kantian corpus, Friedman brought new light to the philosophical significance of Kant's Critical project and also, and importantly, to Kant's relationship with scientific contemporaries, such as Newton and Euler. These merits of Friedman's reading are hardly a matter of dispute, and I will not dispute them here. Instead, my goal is to outline a philosophical agenda that KES has bequeathed to us by critically examining the proposals that Friedman forwards.

In §1, I explore Friedman's emphasis on the role of motion in Kant's Critical corpus, and uncover some tensions in Kant's natural philosophy that are revealed by Friedman's construal of how Kant's "motion" differs from Newton's "motion." For instance, problems emerge from Kant's vacillation on how to define quantity of true motion (TM). If he lets Newton's laws define TM, then bodies have true accelerations but no true velocities. *Sed contra*, if TM is relative to a distinguished standard of rest—Kant's absolute space—true velocities do exist, after all. Moreover, Kant seems too concessive when he grants that the material universe as a whole may rotate, for if this is so, then the center-of-mass frame of the world—the asymptotic referent of Kant's absolute space—will not be inertial. Should that obtain, Newton's laws will not suffice to predict the true motions of bodies.

In §2, I turn to Friedman's innovative and now famous claim that the pre-Critical Kant was intent on bringing Newtonian and Leibnizian themes into cooperation. Based on recent scholarship, I defend the need to reexamine Kant's original motivation for his Critical philosophy. Eric Watkins and others have detected a complex web of Newtonian, Wolffian and post-Leibnizian themes in the early Kant. For instance, philosophical projects to ground dynamical laws were underway well before him in Germany. So, we can now ask again: what was Kant's pre-Critical agenda for the science of his time?

Finally, in §3, I draw attention to passages that seem to challenge Friedman's reading of "motion" in Kant's philosophy. For instance, throughout KES, Friedman identifies "actual motions" with "true rotations relative to an inertial frame." However, this reading leaves out important, and confounding, passages where Kant analyzes rotation as actual in a rotating frame. Moreover, in KES, necessary motions are centripetal accelerations, and yet Kant talks (at length) merely about uniform velocities in impact, with little to indicate how that account of necessary motion might extend to orbital motion.

Robert DiSalle, University of Western Ontario
"Transcendental philosophy from a Newtonian perspective"

In *Kant and the Exact Sciences*, Michael Friedman argued that to understand Kant's transcendental philosophy fully is to understand its connections with Euclidean geometry and Newtonian physics. At the same time, he raised the question whether just these connections diminish the relevance of the Critical philosophy to the profound changes that took place, after Kant, in the foundations of the mathematical sciences. Among the developments that undermined Kant's philosophy of mathematics, in the 19th century, was a new understanding of the relationship between mathematical structures and their physical interpretation, with especially striking consequences for Kant's account of geometry. On the one hand, geometrical structures could be entirely separated from any interpretation, and considered as formal systems in which deduction required no appeal to intuitive constructions. On the other hand, even the obvious intuitive interpretation of geometry could no longer be viewed as fixing the geometry of space: the classical constructions were shown to be compatible not only with Euclid's geometry, but also with any homogeneous geometry, any one of which therefore had an equal a priori possibility of being the geometry of the "space of intuition." In addition to undermining the unique status that Kant had granted to Euclidean geometry, this development undermined the idea that Newtonian physics could claim to be the unique extension of the concepts of the understanding, from ordinary experience to a complete mathematical representation of the universe. This raises the question, for any putative neo-Kantian view, whether the application of mathematics has any a priori aspect in Kant's sense—as opposed to the sense emerging in the later 19th century, allowing only for an a priori, but arbitrary, assignment of empirical interpretation to purely formal structures. Continuing the emphasis that *Kant and the Exact Sciences* placed on the interplay between mathematical laws and concepts of the understanding, I compare Kant's with Newton's view of the foundations of mathematical physics, and in particular Newton's approach to the problems of interpretation of physical theory that he confronted in connection with concepts of space, time, and force. I suggest that Newton addresses the transcendental aspects of these problems without overlooking the role of empirical contingency in their solution, and thereby lays the foundation for a post-Kantian understanding of the a priori in applied mathematics. I also suggest that this account of the extension of empirical concepts illuminates the relevance of the a priori to the conceptual transformations involved in relativity and quantum mechanics.

Michael Friedman, Stanford University
"Reconsidering *Kant and the Exact Sciences*"

I will take this opportunity to respond to the commentators by indicating some of the new directions my work has been taking in the last twenty years. With respect to philosophy of physical science, I have completed my book on the *Metaphysical Foundations of Natural Science*. It presents a new and more nuanced treatment of Kant's relationship to Newton, stressing Kant's "constructive" perspective on Newton's mathematization of the concept of quantity of matter. It also develops a more balanced treatment of Kant's relative debts to Newton and Leibniz. With respect to philosophy of mathematics, I have been developing a more complex approach to Kant's theory of geometry that embeds Euclidean constructions within the form of our spatial intuition, thereby forging a necessary connection between mathematical, perceptual, and physical space. I also consider how this approach to geometry illuminates the treatment of sensibility and understanding in the B Deduction. I will conclude by saying a few words, finally, concerning how I now view later developments in physics and mathematics after Kant.

Sunday, 24 June

Parallel Session IX

Session IX.1 Symposium: Life before the man-machine: Conceptualizing life and mechanism in early modern natural philosophy

In the past two decades, there has been a great deal of provocative yet careful scholarship on three areas which overlap, yet never seem to explicitly take account of each other's accomplishments: work on the nature and diversity of *early modern mechanism* (e.g. Gabbey 2004, Bertoloni Meli 2006); work on the status of the body in early modern science (Wolfe and Gal, eds., 2010; Lawrence & Shapin, eds., 1998), and lastly, work on the philosophical dimensions of *early modern biological theories* and 'life science' more generally, including medicine (n the former see Smith, ed., 2005, and on the latter, Distelzweig, Goldberg, Ragland, eds., forthcoming). Notable figures such as Descartes and Harvey have been interpreted in new ways, following various insights stemming from these three historiographic trends; the question of the status of existing sciences such as medicine and the set of practices and natural-historical theories that would partly come to be designated as 'biology' by the 1790s, has been a greater object of attention in studies of early modern science than in earlier generations. The work featured in this symposium tries to respond philosophically to this new cluster of problems and interpretive responses to them, focusing on *two interrelated questions*: the interrelation of 'mechanical' and 'teleological' models in early modern medicine (particularly focusing on Descartes and Harvey), and the question of these mechanical and teleological models faced with Life – that is, how they function considered as attempts to respond to the challenge of the status of living beings. The papers presented here combine historically focused analyses of individual figures (Hutchins on Descartes, Provijn on Harvey, Distelzweig on Descartes, Harvey and others) with more synoptic analyses of early modern mechanistic models (including automata) faced with Life (Wolfe). These papers combine extensive historical analysis with conceptual tools derived from contemporary philosophy of science (such as the debate on 'mechanisms' following Machamer et al., 2000) and contribute to a newly active field in the history and philosophy of early modern life science.

Peter Distelzweig, University of Pittsburgh
"Function, use and teleology in Descartes and early modern medicine"

Pointing to Descartes' well-known methodological rejection of appeals to final causes in natural philosophy, Hatfield (2007) insists that Descartes nonetheless does employ such teleological resources—especially in his physiology. Hatfield suggests that "the structure of such teleological thinking, and its place in Cartesian metaphysics, warrants further investigation." (30) Indeed, this topic has received the attention of a number of scholars in the last decade (e.g., Des Chene (2001), Simmons (2001), Shapiro (2003), Manning (2006), Brown (2011)). However, this much needed attention has not produced significant consensus. There is disagreement regarding whether teleology is present in Descartes' physiology, and if so, regarding its character, scope, and consistency with Descartes' rejection of final causes.

Although (some of) this work gives attention to the intellectual context of Descartes' physiology, most focuses on Descartes' exchange with Gassendi or on his relation to scholastic natural philosophy. This is problematic for, as Hall (1972), Cunningham (2002), and Manning have pointed out, Descartes' physiological writings belong especially to a *medical* context. It is not to the Aristotle of the *cursus* and *Physics* (or *De Anima*) commentary that we must look. Rather, we should turn to medical thinkers like Jean Fernel, Hieronymus Fabricius, and William Harvey (and to *their* Aristotle and Galen). For it is here (as Hall has amply shown) that we find the important sources and foils for Descartes' physiology. Furthermore, here we find the most prominent technical use of the central apparently teleological terms in Descartes' physiology: function (*functio/fonction*) and use (*usus/usage*).

In this paper I shift our attention to this medical context. I argue that almost (but not quite) all of Descartes' physiology should be understood as devoid of teleological explanations, as they are understood in the medical tradition. Instead Descartes provides mechanistic accounts of the phenomena of life meant to replace existing medical accounts—particularly accounts like those found in *Book Six* ("*De functionibus et humoribus*") of Jean Fernel's influential *Physiologia*. That said, I also argue Descartes does occasionally provide teleological explanations (e.g., in his treatment of variation in heart structure within and across species)—ones strikingly like those in Harvey (and Fabricius). I compare the accounts of Descartes and Harvey, suggesting that Descartes is here interacting with and (unwittingly?) employing Harvey's Aristotelianized approach to anatomy.

Barnaby Hutchins, Ghent University
"Descartes and the dissolution of life"

Prior attempts to understand Descartes' conception of life have tended to make an important error: they take him to *have* a conception of life. Instead, I claim that life is an empty concept for Descartes. He does not attempt to explain life, but to dissolve it: he is not concerned with a global definition of life, but rather provides local mechanical accounts for those phenomena that others associate with life. Of course, it is well known that life is not superadded to matter for Descartes, and that his physiology is concerned with reducing the functions of the scholastic vegetative and sensitive souls to mechanical interactions of matter. The scholarship does not dispute this. In looking for a concept of life, it is after a mechanical explanation, rather than some mysterious additional power. But, in attempting to find a concept of life, the scholarship turns it into something additional: life becomes its own special category, which allows for a categorical distinction between living and non-living matter. But, I claim, this is not a problem that concerns Descartes.

The scholarship has been misled by a few ambiguous passages. When *The Passions of the Soul* talks about '[t]he difference between a living body and a dead body', Descartes appears to be making just this kind of categorical distinction. And when he goes on to claim that this difference is equivalent to that between a mechanically functioning 'watch or other automaton (that is, a self-moving machine)' (1, a. 6) and a broken one, the literature infers that life is something to do with self-movement (Ablondi; Mackenzie). Similarly, when Descartes claims that '[w]hile we are alive there is a continual heat in our heats' (*Passions* 1, a. 8), the literature infers that heat is the basis of life (Ablondi; Aucante; Hall; Mackenzie), and then makes convoluted and unsuccessful attempts to develop a concept of life that allows life to plants and animals while denying it to clocks etc. (Ablondi; Hall; Mackenzie).

I claim that the literature has the problem backwards: there is no need to look for a concept of life in Descartes, because there is none to find. Descartes does not *explain* the difference between life and death on the basis of mechanical self-movement; he *reduces* it to mechanical self-movement, and so dissolves the problem. Heat is not the basis of *life* but merely happens to be the mechanical principle that underlies 'the movements of our limbs' (*Passions* 1, a. 8). Hence, Descartes never ascribes life to a body and denies it to a watch: he has no need to make that distinction.

Dagmar Provvijn, Ghent University
"Harvey's mechanisms"

Much attention has been paid both to Harvey the 'Aristotelian' and to Harvey 'the modern'. I will focus on his 'mechanisms' as "[…] entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions." [p.3, 4]. The 'mechanism'-concept as specified in [4] allows me to, whether or not 'anachronistically', perspicuously grasp Harvey's modelling of physiological processes in an *actio-usus-utilitas* programme of the heart. An important mechanical input on the action of the heart is already present in the *Lecture Notes* as pinpointed in [1]; i.e. the scupping-analogy concerning the cardiac contraction and the analogy of the glove. The scupping-analogy can be further related to the heart-as-a-muscle-analogy as described in [5]. Other issues that I will address will be the mechanical characterization of the pulse (and its support by the glove analogy) and the mechanical description of the full circulation.

My focus on mechanisms allows to further investigate the relation between Harvey's and Descartes' 'heart' and why Harvey's doctrine was both well received and wrongly modified in Descartes' 'mechanical' account of the heart. Hence, I will contrast Harvey's 'mechanisms' in the old natural philosophy with how Descartes' mechanical new natural philosophy forced him in the direction of a 'conservative innovation' [3]. Moreover, I will address the idea of 'the seeds of mechanism as a method of explanation' in Harvey's work as a foetal prior instance of one of the two components of mechanical philosophy: "mechanism as a method of explanation […]; and mechanism as a method of explanation." [p.245, 2].

Charles T. Wolfe, Ghent University
"Automata, man-machines and the challenge of life"

Early modern antimaterialists such as François Lamy or Nicolas Bergier assert that materialists reduce human beings to automata. After all, isn't one of the most famous books of the time entitled *L'Homme-Machine*? But reality is more complex, more eclectic, more hybrid. For one, La Mettrie really employs the machine as an *analogy*, and never reduces 'organic' properties to 'inorganic' properties (Thomson 2001, Wolfe 2009). Further, mechanist physiologies (Descartes, Boerhaave) and a fortiori micro-mechanist physiologies (Haller) never dispense with a functional explanatory dimension, seeking to account for the specificity of living beings without being 'finalistic' (or strongly teleological). Lastly, models of biological 'organization' including the 'animal economy' (Wolfe and Terada 2008) open up a conceptual space which sometimes resembles a kind of 'expanded mechanism', sometimes a heuristic vitalism which would remain compatible with mechanistic accounts of specific lower-level organs and functions (Bordeu, Ménuret de Chambaud). Our challenge then is to understand the figure of the automaton in its materialist context, as both irreducibly organic yet entirely 'automatic' in its physical and affective determinations (in a hedonistic sense). Is it still then a 'mechanistic' figure?

Session IX.2 Fin-de-siècle European philosophies of science

Daniela Barberis, Shimer College
"History and contingency in the work of Émile Boutroux"

Early 20th century French philosophy is best known for Poincaré's contingentism and Bergson's philosophy of the *élan vital*. Both of these philosophical traditions can be traced back to the philosopher Émile Boutroux, Poincaré's friend and brother-in-law, and Bergson's teacher. Boutroux's philosophy has attracted interest because of his critique of science, especially his opposition to the deterministic stance of positivism and late 19th century scientism. But there is another aspect of Boutroux's work — his history of philosophy — that although neglected in the scholarship is in my view crucial to an understanding of the transformation of French philosophy in the late nineteenth century as carried out by the more famous figures who drew upon it. As I will try to show, both his critique of science and his historical work were driven by the same concerns: a rejection of any form of determinism and the establishment of a space for freedom — freedom in nature and in man.

Boutroux's primary foe was a dogmatic concern with objectivity — "objectivism" as he called it — which was then dominant in French thought. Whether in regard to history or the natural sciences, Boutroux's main concern was to show what is lost — in the world and in ourselves — when an "objectivist" method governs our approach to understanding. Ontological commitments are at stake here; for Boutroux, the mind is not separated from ultimate reality, and indeed is a privileged point of access to that reality — and so any approach that sets intuition, introspection, or an inward turn aside necessarily produces an inadequate characterization of reality. As he argued, all knowledge aims to be objective, but the best way to achieve objectivity is not always to impose an exclusively objective method. There are cases in which relying on subjectivity is in fact a better way to achieve a greater degree of objectivity. "Objectivism supposes that things can be reduced to their relationships. This is a purely theoretical view. We cannot, without compromising their objective value, isolate the signs from their meaning." Meaning is an aspect of what things are. The world is characterized by contingently interrelated multiplicity and change, not unity and stability. When we reduce the world to that which can be stabilized, the world takes on the appearance of being determined, and what escapes us is the world as characterized by finality — as having meaning — as significant in its particulars, each picking out a specific possibility within a range of possibilities. For Boutroux, "contingency" does not refer to chance — it is another term for freedom.

Human reason partakes of this fundamental contingency; and so Boutroux came to see human reason as a developing entity — one that could not be understood as composed of a static set of categories. This view of reason opened the possibility for a new arena of study directed toward a history of the mind, for which the privileged materials of historical research were to be the scientific and philosophical theories of the past (with the implication that a history of science and philosophy would be a history of the mind). This paper will explore how Boutroux's conception of freedom produced a new and fruitful approach to the history of philosophy.

Klodian Coko, Indiana University

"Epistemology of a believing historian: Making sense of Duhem's anti-atomism"

Pierre Duhem's (1861-1916) lifelong opposition to 19th century atomic theories of matter has been traditionally attributed to his conventionalist and/or positivist philosophy of science. This thesis has been relatively recently challenged from the combination of two quite independent lines of historiographical development. The first one has to do with the status of the 19th century atomic debates; it argues that, during Duhem's lifetime, atomism was not the wellestablished theory most historians have presented it to be. The second one is concerned with the nature of Duhem's philosophy of physics; it argues that the latter was not positivist, conventionalist, or instrumentalist, but in fact compatible with belief in unobservable atoms and molecules. The conclusion to be derived from the synthesis of these developments is that Duhem's opposition to atomism was not due to any obsession with the observable realm, but to the precarious state of atomic theories in the beginning of the 20th century. In this paper I will (a) present the inadequacies of both the traditional and the new interpretation of Duhem's opposition to atomism and (b) provide a new framework for understanding the latter, that takes into account the historical development of Duhem's philosophy of physics as well as the wider intellectual, religious, and political context in which it was formed.The claims I make, fall into three major headings. Firstly, contrary to the revisionist interpretation of Duhem's epistemology, I argue that the origin and development of Duhem's philosophy of physics clearly shows that it was not compatible with belief in unobservable entities. Although Duhem believed that physical theory was a natural classification of experimental laws which offered an increasingly more accurate reflection of the underlying metaphysical order, the entities classified were abstract mathematical notions and not micro-entities. Even in its ideal ending point, natural classification (physical theory) was not to make any claims about (or contain any references to) atoms and molecules. Secondly, although I claim that Duhem's philosophy of physics was not compatible with belief in atoms and molecules, contrary to the traditional view, I argue that it was not conventionalist or instrumentalist. In order to understand the peculiar nature of Duhem's philosophical outlook, we have to regard his thought in its entirety and take into account the larger intellectual, cultural, and religious context in which it was formed and developed. Thirdly, and most importantly, I provide a new framework in which to understand Duhem's argument against atomism, especially during the late phase of his career. I claim that beginning in the late 1890's and continuing throughout the rest of his life, as the evidence for a discontinuous structure of matter was building up, Duhem appealed more and more to history of science in order to support both his ideal of science and his opposition to atomism. I claim that, during his late years, historical evidence played for Duhem the same role that scientifically based arguments had played in his early years.

Milena Ivanova, University of Bristol

"Poincaré's acceptance of the atom: against fundamentalism"

Poincaré's acceptance of the atom, in "The relation between Matter and Ether", can challenge significantly our understanding of his philosophy of science. If Poincaré defends structural realism, as most scholars suggest, his acceptance of the atom clashes with it. A structural realist is either agnostic about the existence of unobservable entities or eliminates them. In his paper, Poincaré argues for the reality of the atom, which does not seem compatible with the claim that we cannot know the 'nature' of unobservable entities but only their relational properties. This acceptance can be seen as a shift from structural to scientific realism. However, I argue that such an interpretation does not capture Poincaré's argument. I examine the argument closely and suggest that it is not an argument in favour of the existence of unobservable entities, but against fundamentalism. Poincaré argues that even if Perrin's experiments are taken to suggest that the atomic hypothesis is a successful empirical theory, we should not conclude we have reached the fundamental element of reality. The question with which Poincaré is concerned is not whether we should be realists about unobservable entities such as the atom. Since the atomic hypothesis is empirically testable, leads to more predictions and is more fruitful, there is no question that we should accept it. The question is whether we should think that we have found the ultimate element of reality. Poincaré claims that our starting point was believing that matter is composed of atoms – indivisible building blocks of matter. We discover that atoms are composed of other particles – electrons. But these particles are themselves composed of other particles and we expect, by induction, to find out they are also composed of other particles. What this argument shows is that we have inductive grounds to expect that we cannot find a fundamental level of nature. I compare Poincaré's argument with Schaffer's (2003) defence of antifundamentalism and argue they are both inductive arguments against our epistemic access to fundamental entities. I then turn to the question whether antifundamentalism is compatible with structural realism. Despite the fact some structural realist have argued that structuralism implies antifundamentalism (Ladyman and Ross (2007)), I argue that Poincaré's argument is much weaker. He does not propose a metaphysical argument against fundamentalism, which questions the premise that reality comes in levels and suggests that there is no fundamental level on which the rest supervene. Instead, his argument is purely epistemic. It allows for reality to be composed of levels and for there being a fundamental level on which all other levels supervene. The epistemic argument against fundamentalism is compatible with epistemic structural realism. That is, agnosticism towards whether there is a fundamental level to reality on which all other levels supervene, is compatible with agnosticism regarding whether there are unobservable entities or believing there are unobservable entities of which we have limited access. As a consequence, I argue that Poincaré's argument against fundamentalism is compatible with his structural realism.

Pablo Ruiz de Olano, University of Notre Dame
"Blas Cabrera's defense of relativity: Duhem's role in the debate on the foundations of relativity"

Blas Cabrera, the so-called father of Spanish physics, devoted many of his efforts during the 1920s and 1930s to popularizing and defending general relativity. His role in the reception of Einstein's theory is known to anglophone audiences mainly through the works of Thomas Glick and Jose Manuel Sanchez-Ron in the 1980s. In this paper, however, I argue that the philosophical significance of Cabrera's work has so far been neglected and that his defense of general relativity was based on a holistic form of conventionalism that was closely related to that of Pierre Duhem. Additionally, I make the claim that Cabrera's contribution is to be understood in the context of the debates on the foundations of general relativity that were taking place elsewhere in Europe, and that a clear understanding of Cabrera's position may shed light on the precise nature of those debates.

The paper comes divided into two different parts. In the first one I argue for a conventionalist interpretation of Cabrera's views on general relativity by looking at his analysis of the empirical evidence in favor of general relativity in his 1923 book "Principio de Relatividad". As I point out, Cabrera allowed for alternative ways of accounting for the results of Eddington's 1919 eclipse experiment, but he still held the view that general relativity was to be preferred owing to its greater simplicity. Cabrera's defense of general relativity, therefore, was conventionalist in that it made thechoice between Einstein's theory and those of his rivals a matter of convention, to be resolved by recourse to notions such as unity and simplicity. That Cabrera's type of conventionalism was closer to the holistic variant associated with Pierre Duhem rather than with that of Henri Poincare can be seen by turning to a series of conferences that he gave in the Fall of 1921 in the University of Madrid. Although his analysis of Eddington's experiment is essentially the same as in "Principio de Relatividad", the Duhemian roots of his position are more evident in this early articulation of the same point. In 1921, in fact, Cabrera explicitly admits that it is always possible to account for a single observation and still retain certain aspects of a given theory and suggests, in a Duhemian fashion, that this may be due to the presence of auxiliary hypotheses implicit in error treatment.

In the remaining half of the paper, I examine Cabrera's views on the empirical status of geometry, as expressed in a second talk given in San Sebastian also in 1921. Apart from strengthening the case for a conventionalist reading of Cabrera, his conference in San Sebastian is important because it closely follows Einstein's famous lecture on "Geometry and Experience", which he gave in the Prussian Academy of Sciences only a few months earlier. Given the fact that Einstein's lecture is understood to be part of a broader debate against neo-Kantian interpretations of general relativity, I take Cabrera's case to suggest that, by the early 1920s, holistic conventionalism constituted a well-developed position, different from those of neo-Kantians such as Weyl and Cassirer, but also from the atomistic verificationism of people such as Schlick and Reichenbach in the 1920s.

Session IX.3 Varia

Matteo Vagelli, Université Paris 1
"Some remarks on the role of conceptual flaws, errors and mistakes in the historiography of science"

In this presentation I will be dealing with some of the methodological aspects of the historiography of science. A much controversial but persistent opinion assigns to philosophy a peculiar strategic role in relation to science: whereas the latter concerns empirical matters of truth and falsehood, the former is seen as related to conceptual issues of sense and nonsense. This is relevant from a historiographical point of view, since this assumption seems to enable the historian to retrospectively read the path of science and detect *conceptual flaws* and mistakes in it. Furthermore, the way we read our past inevitably affects our present: namely, how are we to decide whether science is presently progressing or not? The answer to this question, besides relying on what conceptual progress is taken to be, bears also on the way one sees the constitution, the persistence, the modification and/or the fading away of scientific objects.

I will argue that two opposed historiographical approaches can be built upon this philosophical standpoint. To illustrate this, I will contrast the position advanced by Ian Hacking on the one hand and that developed by M. Bennett and P.M.S. Hacker on the other. Part of the novelty I will ascribe to the methodological suggestions provided by the former will be connected to Hacking's concept of "styles of scientific reasoning". This concept precisely aims to explain how new categories of propositions, new possible candidates for truth and falsity, emerge historically. In so doing it assumes as a primary necessity that of explaining new possible and 'technical' uses of words by scientists.

To better reveal the contrast I will briefly draw on some paradigmatic and highly contextualized cases in the history of science, such as the extinction from chemistry of an epistemic object (phlogiston) and the ascription by some neuroscientists of some mental faculties to the brain. Is it true, as Bennett and Hacker seem to maintain, that the attribution of negative weight to phlogiston by Stahl was literally *nonsense* on the very base of the scientific knowledge available at that time? Is it tenable that neuroscience is actually "conceptually flawed" since it allows for the production and the circulation of such propositions as "the brain *thinks, interprets, decides*…", which Hacker and Bennett take to be nonsensical since such propositions contradict our 'logical grammar'?

In the first and the second sections of the presentation I will sketch out the philosophical background of the opposition between the two historiographical positions, whereas in the third and conclusive part I will look closely at the specific case studies.

Alessandro Zir, Federal University of Santa Catarina

"The Indians who came from Ophyr: prophecy and natural history in early-modern Brazil"

This paper discusses how prophetical ideas concerning the origin of Indians affect the natural history written by Portuguese colonizers. The debate about the origin of the American Indians was prominent among early-Modern scholars concerned with the discovery of the New World. Gregorio García's *Origen de los indios del Nuevo Mundo e Indias Occidentales* (Spain, 1607) is a huge treatise covering the many theories connected to the issue.

The debate remains very alive throughout the first half of the seventeenth century. It is a touchy subject to Brazilian colonizers writing about the country in that period, because some of the theories in question connected the Indians to the lost tribes of Israel, and are entangled with Jewish prophetical speculations about the reconstruction of Salomon's temple and the returning of the messiah. In Brandão's *Diálogo das grandezas do Brasil* — the main work analyzed in this paper — these speculations influence the way the author describes not only the Indians (their "physiology" and language broadly speaking), but also Brazilian climate and Brazilian vegetation.

Right in the beginning of the book, Brandão describes a tree in his neighborhood. It is said to be growing together with a house. The trunk would have been initially part of the building's structure as a simple board, which germinated because of the extraordinary fertility of the Brazilian soil. The image makes sense in view of Brandão's speculations about the remote history of the land, and the origin of its inhabitants. He defends that the ancestors of the Indians were the same people sent by Salomon to fetch wood to the construction of his temple, in the celebrate land of Ophyr, referred by the Old Testament in the Bible.

Brandão's argument cannot be advanced straightforwardly, because part of the Brazilian territory described is traditionally situated in the so-called uninhabitable torrid zone. In order to face the difficulty, Brandão has to theorize about astrological and meteorological factors, such as the influence of the planet Saturn, and the remarkable impact of systems of cooling West winds coming from the Atlantic. In the long run, this would explain the many differences there are between Brazil and Guinea in Africa, including physiological characteristics of Brazilian Indians and Africans. The theory explains how Brazilian Indians could be descendent from the Jews. It eventually relates Brazil to Ophyr, and to the construction of the Salmon temple (in antiquity and in a near future as well).

The case analyzed here exemplifies how early-Modern natural history was not only inspired but effectively pursued with the support of very speculative ideas. For Brandão, history unfolds according to some providential plan to which man have no direct access, and which remains to a great extent and intrinsically opaque. In Modern times, perspectives like this tend to be more and more marginalized, surviving only in the works of peripheral authors such as Giambattista Vico.

Silvia Di Marco, Universidade de Lisboa

"From Hunter's Gravid Uterus to the Visible Human Project: Having 'interpreted images" really displaced 'metaphysical images'?"

Since Plato's attacks on mimesis, the epistemic status of image has been highly problematic and deeply entangled with rhetorical, aesthetic and moral issues. In two richly documented works, Lorraine Daston and Peter Galison give a convincingly account of the historicity of the concept of "objectivity" through the analysis of the rhetoric of image and authorship that governed the production of scientific atlases between the 18th and 20th centuries. They construct a chronology in which the rhetoric of the Genius, related to the *metaphysical* images of the 18th century, precedes the rhetoric of moral self-restraint, that defines the *mechanical* images (and mechanical objectivity) of the 19th century, which, in turns, comes before the rhetoric of the expert that accompanies the *interpreted* images of the 20th century.

According to Daston and Galison, the reasons for such profound representational displacements (that sound quite similar to the shift of incommensurable paradigms) are to be found in moral, social and institutional factors. It seems to me that, although very informative, this narrative is incomplete, because it ignores the cognitive and heuristic potential of images. First of all, as pointed out by Snyder, when discussing mechanical images Daston and Galison never take into consideration that many of them create and show phenomena that would otherwise be completely inaccessible to the human senses. Moreover, they strongly emphasize the differences among succeeding strategies of scientific representation, but one could alternatively read the history of scientific images in terms of overlaps and co-existences rather than displacements and substitutions, showing that *metaphysical*, *mechanical* and *interpreted* images are not so incommensurable as Daston and Galison claim. To make this point I will compare William Hunter's *Anatomy of the Human Gravid Uterus* (1774), which they use as one example of *metaphysical* image, with the *Visible Human Project of the U.S. National Library of Medicine* (ongoing since 1995), which chronologically belongs to the realm of *interpreted* images. On this basis I will suggest that we can think the shift from one representational strategy to the other in terms of relative epistemic efficacy of each form of visualization (with all the problems that the idea of efficacy entails), rather than just in terms of rhetorical conventions and moral struggles.

Jason Jordan, University of Oregon
"Causal necessity and the agreement between the ancients and the moderns"

One of the most common complaints propounded against Hume's critique of causation is his supposed "conflation" of causal necessitation with logical necessitation. This complaint is commonly extended to other moderns (e.g. Hobbes, Malebranche) and interpreted as a systemic error of early-modern philosophy. In this paper I defend Hume and the moderns against this charge on two fronts.

First, I argue (in support of Walter Ott) that the identification of causal with logical necessity was not the product of some modern conflation, but rather of a strong continuity extending from antiquity through the medieval and into the modern period. Either implicitly or explicitly, ancient philosophers accepted a "necessary connexion" between causes and their effects *without exception*; and this model was retained in medieval philosophy before reaching its critical dénouement in modern philosophy. Thus, the error lies not an erroneous identification of causal and logical necessity by the moderns, but in an anachronistic projection by contemporary philosophers onto the schools of antiquity in search of historical precedent and support for their own *erroneous* distinction between the two.

This leads to my second counter: I argue—with and in support of Hume—that the distinction between 'logical' and 'material', 'non-logical', or 'nomological necessity' appealed to by contemporary critics of Hume's critique is untenable insofar as the latter notions are metaphysically possible but epistemologically incoherent. My argument on this front is complex but its principle is very simple: the notion of non-logical necessity is predicated on the assumption that there is a modality between (logical) necessity and contingency. There is not: the relation between two terms is either one of implication and identity, or, barring that, one of brute regularity. A middle term might *exist*, be we are fundamentally incapable of comprehending it.

This claim is undoubtedly controversial and even cacodoxical at present, but it is my intention to defend it forcefully through a close examination of the reasoning of those figures in the history of philosophy that affirmed it and those that tried to avoid it.

Session IX.4 History of mathematics

Mario Santos-Sousa, University College London
"Berkeley on the mind-dependence of numbers"

Berkeley's arguments for the mind-dependence of numbers have not always been adequately discussed in the literature, either because they have been misconstrued as a mere corollary to his general idealism, or because, despite given serious consideration, their import has been overstated. As a result of the former approach, his views have often been dismissed without further analysis. As a result of the latter, their allegedly fatal conclusions have led to a rejection of Berkeley's most basic assumptions, even ones that he shared with his realist opponents.

The aim of this paper is to dispel these common misconceptions. In order to do so, I provide a careful analysis of three distinct but complementary lines of argument for the mind dependence of numbers found in the *Principles* and in the *New Theory of Vision*, which Berkeley himself took to be independent of his general idealism: (1) a negative argument that proceeds from considerations about the relativity of cardinal number ascriptions to the conclusion that numbers are not a mind-independent feature of the world; (2) an argument that takes the form of an inference to the best explanation, which Berkeley puts forward as the best way of accounting for the apparent variability of cardinality ascriptions; and (3) a 'straight' argument which builds on similar considerations about the variability of cardinality ascriptions in order to conclude that numbers are mentally constructed.

I examine each of these arguments in turn and offer some responses on behalf of the realist. Finally, I take issue with Douglas Jesseph's discussion of the relevant passages, which overstates the import of Berkeley's arguments. Once we understand Berkeley's arguments correctly, however, we realise that, ironically, the realist view ends up being reinforced.

Barbara Sattler, Yale University
"The labours of Zeno: A supertask?"

The so-called supertask debate – the dispute whether an infinite sequence of actions or operations carried out in a finite interval of time is physically or logically possible – started in the 1950s between Black, Taylor, Watling, and Thomson and was rekindled by Laraudogoitia and others in the 1990s and 2000s. But the real founder of this debate, it is normally claimed, was Zeno with his dichotomy paradox. This paradox famously poses the problem that a runner, trying to cover a certain finite distance, first has to cover half of the distance, then again the first half of the remaining distance, and so on *ad infinitum*. So it seems that the finite process of running a finite distance requires covering infinitely many spatial pieces, and thus performing infinitely many tasks.

In my paper I try to demonstrate that this is actually not a problem raised by Zeno's paradox, and that an account of the dichotomy paradox as a supertask (even though implicitly employed also by many ancient philosophers, like Barnes, Ferber, and Kirk, Raven, and Schofield) seriously misconstrues the problems Zeno raises. However, I also show that comparing Zeno's paradox with a paradigmatic supertask can nevertheless be instructive, since it forces us to make explicit the pre-conditions on which this debate rests and to examine whether these conditions are indeed given in the case of a run.

Within the supertask debate, the basic notion of a task is nowhere defined. But an analysis of paradigmatic supertasks, for instance as assumed of Black's marble transferring machine, shows that a task is a single, clearly defined action that has a definite beginning and end. It is clearly distinguished from the next task, usually by a change of the direction of movement and by a standstill in between two tasks. A supertask, accordingly, is a series of discrete states, normally an alternating series, in which no task can be arbitrarily chosen.

Movement as discussed in the dichotomy paradox, however, is not such a series in which each part is defined while the beginning and the end are undetermined. On the contrary, with a continuous run beginning and end are determined, but the parts can be chosen as one pleases – this last feature will be shown to be one of the main concerns of the dichotomy paradox.

Nevertheless, several people tried to model Zeno's dichotomy after a supertask. Most prominently, Grünbaum introduced a so-called staccato runner who pauses after the completion of each part of the run. But Grünbaum does not give any explanation why these single runs interrupted by pauses would still be considered as *one* run, rather than as a series of different runs. This discussion will help to show that while we might use the notion of a supertask for considering a motion in certain contexts, characterising something continuous like a run as a supertask means missing out on the specific unity required to conceptualise motion that is under discussion in Zeno's paradox.

Tobias Schöttler, Ruhr-Universität Bochum
"New adventures in old mathematics: The shift from causes to relations at the root of early modernity"

In the philosophy of mathematics during the early modern times, we can identify two justifications of mathematical certainty. The first strategy justifies the certainty of mathematics by means of the ontological status of their entities. Created by abstraction, they have the highest level of clarity and evidence. The second strategy deduces the mathematical certainty from their use of the so-called *demonstratio potissima*. Within the Aristotelian framework, the *demonstratio potissima* is regarded as the highest and most certain type of proof. Such proof consists of a syllogism that provides both the cause and the effect of an event. It uses a middle term which specifies the immediate cause of the effect in an unique way. The equation of mathematical proof and *demonstratio potissima* is the main point of the so-called *Quaestio de Certitudo Mathematicarum* initiated by Alessandro Piccolomini.

Piccolomini shows that proofs in mathematics, especially in Euclid's geometry, do not conform to any of the conditions above. While Piccolomini does indeed justify the certainty of mathematics by the nature of their entities, the destructive part of his argumentation was significantly more influential. Several scholars agreed with him (Simonius, Pererius, the jesuits of Coimbra, Smiglecius). Pererius radicalizes Piccolomini's theses and arguments by denying mathematics the status of a science. Other scholars either maintain that mathematical proofs are *demonstrationes potissimae* (Balduinus, Schegk) or try to prove that at least some of the mathematical proofs satisfy the conditions for being a *demonstratio potissima* (Barozzi, Viotti, Blancanus).

Such a level of detail in differences implies significant common ground. All participants of the debate recognize the Aristotelian scientific theory as the norm. In fact, the debate shows the inadequacy of assessing mathematical proofs using the criteria of the Aristotelian proof theory. From a present-day perspective, the debate is based on completely misguided assumptions. Yet even the traditionally Aristotelian answers take on a new meaning by virtue of a new context. This marks the birth of a generally new debate which has unwittingly left its Aristotelian roots behind.

The characteristics of geometrical proofs are only recognizable against the background of different kinds of proofs and proof theories. Critics like Piccolomini and Smiglecius are not satisfied with the mere statement that geometrical proofs do not meet the requirements of an Aristotelian proof. In addition, they seek to explain how geometrical proofs work, and in the process, they emphasize the role of internal relationships within geometry itself: Geometrical proofs are not based on a hierarchy of causes. Instead, one theorem can be proven by different premises. The proofs in geometry argue primarily based on the relationship between the different figures. One remarkable aspect of this lies in the rising influence of such concepts of coherence until centuries later, when non-Euclidean geometry has been discovered and different geometrical systems become recognized.

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"The role of analogy in mathematical reasoning: The case of Archimedes' De Circuli Dimensione and Bernoulli's *Art Conjectandi*"

I propose to examine the influence of Archimedes' *De Circuli Dimensione* on Jacob Bernoulli's strategy for proving his main theorem—sometimes called the first law of large numbers—in the *Ars Conjectandi* (1713). I will then briefly discuss what this breakthrough development in the history of mathematical probability reveals about the role of analogy in mathematical inquiry.

Jacob Bernoulli's famous theorem linked the observed (empirical or *a posteriori*) stable statistical frequencies of seemingly random events to the *a priori* probabilities of those events. Given the *a priori* probabilities of binary trials (e.g. coin tossing), Bernoulli developed a method to calculate in how many experimental trials one could estimate these probabilities *a posteriori* (empirically or statistically) to a desired level of precision. The proof strategy that Archimedes uses to find bounds for the value of π in *De Circuli Dimensione* provided Bernoulli with a plan of attack to demonstrate his theorem. I aim to present a careful investigation of the nature of Archimedes' reasoning in order to assess its heuristic impact on Bernoulli. Archimedes approximates the value of π in Proposition III of *De Circuli Dimensione* or *Measurement of the Circle*. Archimedes shows that the ratio of the circumference to the diameter of a circle is less than 3 1/7 but greater than 3 10/71.

Archimedes' solution consists of two parts, each concerned with finding one of the bounds. It is reasonable to claim, then, that this proposition was originally a problem to be resolved—namely, "approximate the value of π"—, that Archimedes reduced it to two problems—namely, "find a lower and an upper bound for π"—and that the formal statement of the proposition and its proof are only the formal skeleton of an involved analytical process. He finds the upper bound by inscribing a circle within a regular hexagon, then doubling recursively the number of sides of the regular polygon circumscribing the circle, up to a 96-side polygon—and showing successively that the perimeter of each polygon is less than and gradually approximates 3 1/7. Invoking relevant theorems from Euclidean geometry, he can deduce that π is also less than 3 1/7. Archimedes finds the lower bound in a similar fashion, deducing that $\pi > 3\ 10/71$. Thus, a series of carefully conceived and calculated inequalities lead to the approximation, which Bernoulli considers sufficient for practical use.

I will show how Bernoulli's reasoning in order to provide empirical bounds for a probability is analogous to Archimedes' reasoning for finding practical bounds for the value of π. My philosophical thesis will be that 'analogy' is an important heuristic technique in mathematics, where analogy is carefully defined. The mathematician's grasping of an analogy between two problems from different areas of mathematics, where one of the problems has already been solved, leads to the adoption of a successful strategy for solving the extant problem.

End of Conference

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