# Abstracts

## Nathan Andersen
### Repetition and Reenactment: Collingwood on the Relation between History and the Philosophy of Science

R.G. Collingwood concludes *The Idea of Nature* -- his attempt to articulate the conception of nature that is implied by the historical development of natural science -- with an assertion that, if true, would be extremely relevant to contemporary discussions of the relation between the history of science and the philosophy of science. He claims that "no one can understand natural science unless he understands history." On Collingwood's view, "natural science" is not and cannot be a fully autonomous form of human activity. Rather, it depends upon the distinct methods of investigation that he takes to be characteristic of historical research. His arguments for this view are not, however, fully developed in the context of *The Idea of Nature* itself, which remained unfinished at the time of Collingwood's death in 1943. In this work, in fact, his claim appears to amount to the trivial one that insofar as scientific research depends upon the accumulation of facts it demands that the scientist, like the historian, consult and interpret historical documents that report observations. It is argued in the present essay that the significance of Collingwood's view of the relation between history and the philosophy of science can only be appreciated when the concluding passages of *The Idea of Nature* are read in the context of his other works on the nature of historical investigation.

Historical thinking, for Collingwood, involves more than the accumulation of facts on the basis of testimony. To engage in historical thinking requires that one *reenact* the thinking of the historical figure under investigation. It requires, as Collingwood writes in *The Idea of History*, that the historian "re-enact in his own mind the thought he is studying, envisaging the problem from which it started and reconstructing the steps by which its solution was attempted." Although the relevance of this form of investigation to the actual practice of natural science may not be immediately evident, this essay shows that Collingwood's thesis is illustrated and confirmed by recent historical and philosophical studies of the nature of experiment. In particular, David Gooding's book *Experiment and the Making of Meaning* - that focuses on the experiments of Biot, Faraday and others on electromagnetic phenomena - shows that experimental results become significant only as a result of a complicated process, that involves *both* laboratory practices and social involvements. As a result of his own efforts to reproduce the results of experimental scientists, Gooding discovers that to uncover the *meaning* of an experimental result requires that one *reenact* and *relive* the practices and assumptions of the investigative community that produced them. If Gooding's conclusions are accurate, they indicate that experimental science does in fact rest upon the form of activity that Collingwood identified as characteristic of historical thinking. The essay concludes by indicating the relevance of Collingwood's thesis for contemporary discussions of the relation between the history of science and the philosophy of science.

## R. Lanier Anderson
### The Traditional Logic and Kant's Philosophy of Arithmetic

Kant famously asserts that mathematical knowledge is synthetic. The claim has been particularly controversial in the case of arithmetic, where the role of "construction in intuition" is less obvious than it is in geometry. Kant's view has also been criticized on grounds of clarity, as part of general attacks on the idea that there is any intelligible distinction at all between analytic and synthetic judgments. These two forms of objection intersect in complaints about the frustratingly thin character of Kant's reasoning in defense of the syntheticity of arithmetic. For example, his repeated insistence that “The concept of twelve is by no means already thought merely by my thinking of that unification of seven and five, and no matter how long I analyze my concept of such a possible sum, I will still not find twelve in it" (B 15; cf. A 164/B 205) seems less an actual argument that an exercise in table pounding. Moreover, such Kantian assertions clearly depend on his definition of analyticity as a matter of one concept's being “contained in" another (A 6/B 10), which many philosophers, following Quine, have dismissed as “merely metaphorical." I will show that, contrary to widespread current opinion, Kant deploys a perfectly clear and defensible notion of concept containment, which emerges in light of traditional, early modern logical ideas (and their appropriation in the metaphysics of C. Wolff). The notion of containment is then able to fund a clear distinction between analytic and synthetic judgments. Once we understand that distinction, it provides the resources for a compelling argument that arithmetic must be synthetic, sensu Kant. To anticipate, on my reading, Kant's denial that the concept<12> is contained in the sum concept <7+5> amounts to a claim that there is no concept hierarchy, conforming to the rules of traditional logical division, which establishes a containment relation between <12> and <7+5>.I will show that Kant is right that no such hierarchy can be constructed. It follows not only that arithmetic is synthetic, as Kant understood the term, but also that there are deep and principled limitations on the expressive power of a logical system of the sort appropriate as a framework for a Wolffian metaphysics. Kant's result thereby deals a fatal blow against the Wolffian program to reconstruct all genuine scientific knowledge in privileged logical form. Simultaneously, it illuminates the motivations of Kant's broader philosophy, because it raises a problem about how synthetic judgment is possible at all -- a problem Kant aimed to solve in the Aesthetic and Analytic sections of the Critique of Pure Reason, by offering a general theory of cognitive synthesis, of which the theory of mathematical construction in pure intuition is one prominent (and paradigmatic) example.

**Eric Audureau**

## On Poincaré’s Alleged Conventionalism

Les interprètes paraissent généralement s'accorder pour reconnaître une forme de dualisme dans les conceptions de Poincaré sur l'origine de la connaissance mathématique et physico-mathématique. Intuitionniste en arithmétique, Poincaré serait conventionnaliste en géométrie et en physique mathématique. Une telle absence d'unité, si elle était attestée, ramènerait l'intérêt de sa philosophie à quelques positions circonstancielles occasionnées par les différentes controverses qu'il a pu entretenir avec les savants et les philosophes de son temps. Cet éclectisme amoindrirait l'intérêt de sa doctrine, voire le priverait du titre de philosophe, si on admet que le propre de la philosophie est de faire dériver l'origine de nos connaissances d'un principe unique.

J'essayerai de montrer que le conventionnalisme géométrique de Poincaré, qu'il faudrait mieux appeler conventionnalisme géométrico-cinématique, n'est qu'une conséquence de son intuitionnisme. Cet intuitionnisme consiste à dénier toute forme de réalité à l'espace et au temps ("Ce n'est pas la Nature qui nous impose les idées d'espace et de temps mais c'est nous qui les imposons à la Nature") et donc également aux grandeurs cinématiques qui, par définition, en dépendent. Les questions portant sur la forme de l'espace (ou de l'espace-temps) physique (p. ex.: la courbure de l'espace est-elle positive, négative ou nulle?, combien l'espace a-t-il de dimensions?) sont donc privées de sens. En d'autres termes entre la notion *mathématique* d'espace, objet de la géométrie pure, et la notion *psycho-physiologique* d'espace, que Poincaré appelle "l'espace représentatif", il n'y a, dans sa doctrine, aucune place pour la notion d'espace *physique*. Cependant, puisqu'on ne peut faire de physique sans instruments de mesure, et que ceux-ci emploient constitutivement des règles et des horloges, nous sommes astreint à poser conventionnellement l'existence de grandeurs physiques associées à l'emploi de ces instruments pour interpréter toute expérience. Le conventionnalisme est donc une conséquence de l'intuitionnisme et de la nature de la physique, le constat du rôle des instruments de mesure ne traduisant en lui-même aucun engagement philosophique.

Je mettrai cette interprétation à l'épreuve en examinant un autre sujet controversé, et jusqu'ici expliqué de façon insatisfaisante, de l'histoire des sciences: comment se fait-il que Poincaré qui, entre autre, a montré que la notion de simultanéité était dépourvue de sens physique et qui a formulé, peu de temps avant Einstein, les lois de la dynamique relativiste, soit demeuré indifférent à la relativité restreinte en maintenant sa doctrine de l'espace (bien qu'il ait admis que ses conceptions aient été ébranlées par cette théorie)? J'avancerai que c'est par fidélité à son intuitionnisme, et non pas par manque de clairvoyance comme on a pu le soutenir, qu'il a campé sur ses positions. Il restera à voir si cette position est légitime

## Erik Banks

### Two Ex-Herbartians on Space: Ernst Mach and Bernhard Riemann

During their early careers, Ernst Mach and Bernhard Riemann were influenced by the German realist philosopher Johann Friedrich Herbart (1776-1843). One of Herbart's abiding interests was the construction of extended magnitudes from intensities, for example manifolds of color and tone in psychology and a construction of physical space from notions of quality and magnitude.

Ernst Mach undertook the study of Herbart in his early career and absorbed many of the German philosopher's insights into his own *Elementenlehre*. As a sense physiologist, Mach attempted his own constructions of the one-dimensional tone-row and the space of colors.

Riemann explicitly credited "certain philosophical investigations of Herbart" in the body of his famous 1854 *Habilitationsschrift* on geometry and used Herbartian philosophical terms in his own definition of extended magnitudes as the outcome of "a transition from one mode of determination of a manifold to another." Riemann's *Nachlass* notes to the *Habilitationsschrift* (in published fragments) reveal that Herbart's tone-space was a model for his treatment of one-dimensional manifolds. He also gave color-space as an example of a three-fold continuous manifold, as Herbart had done.

After discovering Riemann's work in 1867, Mach began to incorporate the mathematician's ideas into his own philosophy of space. Specific references to Riemann in Mach's Nachlass notebooks and lectures reveal that Mach sought a construction of physical space from qualities (his elements). Mach's notion of a universal "chemical manifold" of energies was as close as he ever got to the realization of this plan.

It thus appears probable that both Mach and Riemann accepted Herbart's fundamental idea that extended magnitudes ought to be constructed from aspatial qualities. This is consistent with Riemann's stated aim to reconstruct geometry from the ground up without assuming the notion of extension at the outset and it is consistent with Mach's construction of the world out of elemental intensities.

## Zvi Biener and Christopher Smeenk

### Does Gravity Feign? Newton, Cotes, and the Essential Properties of Matter

At the heart of Newton's achievement in the *Principia* lies an innovative conception of matter and matter's relation to gravitational attraction. Modern readers may be tempted to see this conception of matter as nothing more than the familiar idea of mass. In fact, Newton does use "quantity of matter" throughout the *Principia* as a measure of a body's response to impressed forces; in Definition III he asserts that the quantity of matter is proportional to the *vis insita* of a body, and thus measures the resistance of a body to a change in its state of motion. However, this "dynamical" conception exists alongside a different, "geometrical" conception of matter that is often ignored due to its apparently less important role in the *Principia* itself and neglect by 18th and 19th century developers of Newtonian theory. On this conception, introduced by Newton in *De Gravitatione* and Definition I of the *Principia*, the quantity of matter is to be measured by the amount of space filled by body rather than void. We argue that both dynamical and geometrical properties of matter are essential for understanding the metaphysical *and* mathematical underpinnings of the argument for Universal Gravitation .

The relation of these two types of properties of matter to Newtonian theory will be articulated through an analysis of the correspondence between Newton and Roger Cotes, editor of the *Principia*'s second edition, during the winter of 1711/12. Cotes shows in this exchange that Newton's use of the dynamical and geometrical properties in the opening argument of Book III is inappropriate since the two conception are in conflict with one. On the mathematical side, we examine how the two properties of matter lead to two different ways of quantifying the proportionality between matter and gravitational attraction and show how Newton must make strong and unjustified assumptions regarding their equivalence in order to salvage the argument for Universal Gravitation. On the philosophical side, we examine the relationship of the two conceptions and their supposed equivalence to the Third Rule of Philosophizing and the methodological role this rule plays in Newton's experimental philosophy. The central position of this rule, particularly with respect to its application in inductively ascertaining the essential properties of matter, is exposed as precarious, at best, given Cotes' penetrating criticisms. It seems that although Newton himself proclaimed that he feigns no hypotheses, the very nature of gravitation suggests that some hypotheses must be feigned. It seems, however, that Cotes' criticisms fell on deaf ears; Newton did not revise the *Principia* substantially in light of them. A reworking of Newtonian mechanics that took account of these difficulties had to await the work of luminaries such as Kant, on the philosophical side, and Boscovich, on the side of physics.

## John Blackmore
### Mach, Mauthner and Six Types of Skepticism

Fritz Mauthner who spent roughly equal times in Bohemia (1849-1876) dedicated to study, Berlin (1876-1905) to literature, and Meesberg on the sea of Constance (1907-1923) to philosophy is diversely understood as an admirer of Bismarck, Buddhism, atheism, Machism, linguistic philosophy, satire and skepticism. Our paper will investigate his type of skepticism and compare it to that of Mach and other types, while trying to understand various kinds in a context contrasted with the Hellenistic definition of dogmatism as 'any belief that we have absolute (unconditional) certainty about anything.' We then show how Kant's "critical philosophy" moved Mach and Mauthner to issues of skepticism and dogmatism.

The first type of skepticism denies that we can be unconditionally certain about the existence of a trans-conscious physical world. Both Mach and Mauthner were skeptics in this sense.

The second type denies that we can be unconditionally certain of sensations or immediate conscious experience. Mauthner was skeptical of this but not Ernst Mach, even though he did allow for human fallibility.

The third type is skepticism in the above two senses but allows for unconditional certainty about logic or mathematics. Both Mach and Mauthner were skeptical towards this position.

The fourth type is skeptical in the above three senses but allows for the unconditional certainty of intuition or feeling. Mauthner seems to have accepted this position, but not Mach.

The fifth type, while denying that we can be unconditionally certain of anything, also rejects truth as correspondence with reality in favor of one or more types of relative truth, such as the coherence theory, pragmatism or subjective criteria. Both Mach and Mauthner seem to have favored this position.

The sixth type of skepticism, which seems to have been favored by Arcesilaus, Carneades and the "Middle Platonic Academy," rejects all claims to unconditional certainty as well as all kinds of relative truth in favor of probabilistic theories of truth as correspondence with reality, based on apparent or actual weight of evidence. Both Mach and Mauthner rejected this approach, even though it has long been generally favored by many scientists. Today it is generally employed by most historians but rejected by many empiricists, Popperians and mathematically inclined philosophers and scientists.

The critical philosophy apparently helped turn both Mach and Mauthner into skeptics in most of the senses mentioned above and helped place them in opposition to the sixth type of skepticism, that is, to the probabilistic approach which is still very widespread today, especially among historians. On the other hand, when Mach and Mauthner were writing history (like Hume before them) they also tended to think in probabilistic terms and *even* behaved as if truth existed as correspondence with past reality.

## Peter Bokulich
### Bohr on Disturbance and Quantum Uncertainty

Several historians and philosophers of quantum theory claim that Niels Bohr held an unacceptable "disturbance" interpretation of quantum uncertainties. According to Harvey Brown and Michael Redhead, Bohr explains, for example, the uncertainty in the momentum of an electron that has passed through a slit in a diaphragm as resulting from the momentum uncertainty of the diaphragm. On their reading of Bohr, this uncertainty gets transferred to the electron when it interacts with the diaphragm. Although Mara Beller and Arthur Fine offer a somewhat different account of Bohr's idea of disturbance, they agree with Brown and Redhead that the appeal to disturbance is an essential part of Bohr's interpretation, and that this appeal is illegitimate. Beller and Fine further argue that the 1935 paper by Einstein, Podolsky, and Rosen forced Bohr to recognize the inadequacy of his view, and thus to abandon his disturbance interpretation of quantum uncertainty.

Here I argue that these accounts of Bohr's philosophy are mistaken. I offer a more accurate account of the role of disturbance in Bohr's interpretation of quantum theory, in part by investigating his 1933 discussion of the measurability of quantum fields. This important paper, written with Leon Rosenfeld, has been generally neglected or misunderstood by historians and philosophers of quantum mechanics. The account of field measurements given in this paper helps to clarify the role of "classical concepts" in Bohr's account of quantum measurement and reveals a distinction between two aspects of interactions between measuring apparatuses and the system under investigation. More specifically, Bohr distinguishes between a *classical* part of the interaction – which can be properly considered a "disturbance" – and a fundamentally *quantum* aspect, which he argues is not truly a disturbance, but is rather a manifestation of the fundamentally stochastic nature of quantum theory. I argue that, properly understood, Bohr's position does not appeal to any problematic account of disturbance to explain quantum uncertainties. This aspect of his interpretation is perfectly consistent and remained unchanged from 1927 onward.

## Michel Bourdeau
### Comte et le Naturalisme

Si Comte voit dans la naissance de la sociologie un événement sans précédent c'est que, les phénomènes proprement humains devenant enfin l'objet d'une étude positive, la distinction entre philosophie naturelle et philosophie morale, qui dominait l'histoire de la pensée depuis les grecs, devient caduque. Comte, qui avait fait de la diversité irréductible des phénomènes le fondement de sa philosophie des sciences, apparaît cette fois comme un moniste. De cette position que l'on peut encore qualifier de naturaliste, je ne retiendrai que deux aspects.

1. Sa pertinence dans les débats actuels sur les sciences cognitives. Ceux qui reprochent à Comte son refus de la psychologie oublient d'ordinaire de signaler qu'il n'a jamais refusé d'étudier les fonctions intellectuelles, affectives et morales : il leur assigne simplement une autre discipline, la physiologie (cf. la 45ième leçon du *Cours*). S'il est donc permis de voir dans l'auteur du Tableau cérébral un des précurseurs des neurosciences, il n'est par pour autant réductionniste et il a toujours condamné l'irrationnelle prétention des sciences inférieures à gouverner les supérieures, où il voyait l'essence du matérialisme.

2. La position comtienne met également en lumière la dimension éthique des débats actuels sur le naturalisme. Qu'il s'agisse de bioéthique ou de naturalisation de la morale, ce sont les rapports de la science et de la morale qui sont en jeu. Pour faire de la morale la septième science, l'auteur du *Système* a été obligé d'en proposer une définition très personnelle, qui tombe sous le coup de la critique de Poincaré : entre science et morale, il y a la même distance qu'entre l'indicatif et l'impératif.

## Jean-François Braunstein
### Comte et le "Style Français'' en Histoire des Sciences

Il est courant de parler d'une épistémologie française post-bachelardienne, qui se caractériserait par son "historicisme" et son "régionalisme scientifique", et réunirait des auteurs tels que Canguilhem, Foucault ou F. Dagognet. Nous voudrions montrer que ce "style" historique de pensée en philosophie des sciences trouve en fait son origine dans l'oeuvre d'Auguste Comte. Ces problématiques sont également présentes lors de la fondation en 1932, par Abel Rey, de l'Institut d'histoire des sciences et des techniques de l'Université de Paris. Cette approche se caractérise par d'autres aspects connexes, comme la critique explicite de toute idée de "méthode", ou de "philosophie de la connaissance": de Comte à Canguilhem, en passant par Abel Rey "la théorie de la connaissance n'est qu'une idéologie vague ou une dialectique verbale sans l'histoire philosophique de la science".

La philosophie des sciences de Comte jette également un jour nouveau sur les controverses actuelles sur la "désunité" des sciences, puisque Comte est le premier "antiréductionniste" résolu. Elle permet aussi d'éviter le débat — "ennuyeux et répétitif" selon H. Putnam — qui oppose l'histoire à la science: le même Putnam a souligné avec humour que "le premier positiviste, Auguste Comte, était un historiciste résolu".

## Anastasios Brenner
### Carnap's Critical Conventionalism

When Carnap came to express more fully his ideas in his first postdoctoral publication, "On the Task of Physics and the Principle of Simplicity", he put forward a conception that took inspiration from Poincaré and his continuator Dingler. He borrowed from the latter the expression "critical conventionalism" as a means of situating himself on the philosophical scene. This orientation seems to have characterized his research up until his major work, *The Logical Structure of the World*. Emphasis is usually laid on the adjective "critical". It is true that Dingler referred to Kant, and Carnap himself had begun his work in philosophy from a neo-Kantian viewpoint. Yet what is concerned here is first and foremost a certain form of conventionalism, and Carnap was already steering clear of Kant. The concept of synthetic *a priori* was no longer to be understood in Kant's sense. Commentators speak here of a relativized *a priori*. But at this stage of Carnap's philosophy another concept came to play a prominent part, that of convention. This concept enables us to characterize what goes beyond experience and makes it possible to construct science.

It is not enough to acknowledge conventions in science; one must make clear their nature and extent. Some conventions are more appropriate than others. Attention should be directed to the criteria of decision. Poincaré had invoked simplicity in favor of Euclidean geometry as the mathematical language of physics. But recent developments in physics made it necessary to take up the problem again. Here Carnap entered the scene and gave conventionalism a new turn. By simplicity different things may be understood. Should one prefer the simplicity of the mathematical part of physical theory or the simplicity of the whole body of science including the connections with perception? Calling on conventionalism, Carnap introduced here themes that announce *The Logical Structure of the World*. It remains to understand the relationship between Poincaré's doctrine and the next stage represented by the theory of constitution.

## Dorothy Coleman
### Baconian Probability and Hume's Theory of Testimony

Hume notoriously argued that no testimony is sufficient to justify belief in the occurrence of a miracle, defined as a violation of a law of nature, "unless the testimony be of such a kind, that its falsehood would be more miraculous, than the fact, which it endeavors to establish" (E, 116). His argument for this thesis relies on the premise that in determining the credibility of testimony to any extraordinary event-whether miraculous or merely anomalous-"the evidence, resulting from testimony, admits of a diminution, greater or less, in proportion as the fact is more or less unusual" (E, 113). Ironically, both advocates and critics of Hume's "diminution principle" have invoked a Bayesian model of conditional prob-abilities in evaluating his theory of testimony. While this fashionable approach is consistent with Hume's focus on epistemic probability, or probability relative to evidence, I prefer to side-step this debate because both sides of it assume without argument that all epistemic gradations of probability should be evaluated using a Pascalian model of probability, that is, probability based on the mathematical calculus of chance, of which Bayesianism is one form. I will defend Hume on his own terms by showing that criticisms based on the calculus of chances are irrelevant for assessing his account of testimony be-cause the model of probability on which he bases it is Baconian rather than Pascalian. The foremost advocate of Baconian probability, L. J. Cohen, has credited Hume for being the first to explicitly recognize "that there is an important kind of probability which does not fit into the framework afforded by the calculus of chance," a recognition he finds evident in Hume's distinction between "probabilities arising from analogy and probabilities arising from chance or cause." The purpose of this paper is to interpret Hume's account of testimony in light of this insight and to discuss its implications for assessing his argument against the believability of miracles.

## Darcy Cutler
### Logicism and Gödel’s Theorems

There is a tendency in the literature on mathematical logic to suppose that Godel's incompleteness theorem count as decisively against logicism as they do against formalism. I argue that they do not. The appearance that they do results from a failure on the part of vaious authors to attend to the differences between Frege's program in foundations of mathematics and Hilbert's.

Godel's incompleteness theorems point out the limited powers of formal systems of deduction to capture mathematical truth and of proof theory to guarantee the consistency of formalized theories. The notion of a formal system of deduction has a central place in the formulation and defense of both logicism and formalism. Frege attempted to show the autonomy of arithmetic from spatio-temporal intuition by presenting a set of deductions within a particular formal system. In contrast Hilbert attempted to reduce the notion of mathematical truth to deducibility within a particular formal system. The first incompleteness theorem shows, in effect, that for any consistent formal system of arithmetic there is a true statement of number theory that is not a theorem of the system. Godel's first incompleteness theorem bears decisively against the viability of the formalist's goal but not, I argue, against the logicist's.

## Todd Davis
### Science, Language, and the Reconstruction of Philosophy: Sellars’ Critique of Carnap in “Empiricism and Abstract Entities"

In “Empiricism and Abstract Entities," Wilfrid Sellars says that Carnap’s work in syntax and semantics provides for the first satisfying empiricist understanding of mind and knowledge. However, Sellars’ praise is given against the backdrop of a criticism of Carnap’s idea of the natures of pure and descriptive syntax and semantics and their relations. Underlying that criticism is a disagreement over the notions of “prescription" and “description". In interpreting these criticisms, it is important to note that

Carnap's ideas of pure and applied syntax and semantics are in the service of his conception of philosophy as a formal science and as a subdiscipline of "metascience". As such, these ideas concern both the way in which philosophy is to become "scientific" and the way in which reasoning in science can be rationally reconsructed and its rational content exhibited. Sellars' criticism of Carnap thus has significant import for how such reconstruction should proceed and what it can accomplish.

In this paper, I will examine this moment in the intertwined histories of philosophy of science and the philosophies of mind and language, focusing in particular on two aspects of Sellars' essay. First, I will examine Sellars' complaint against the analogy Carnap draws between pure and descriptive syntax and semantics, and pure and physical geometry. Second, I will examine Sellars' discussion of games and statements about rules in games that precedes and sets the stage for this complaint. "Normativity" and its connected concepts, like "prescription", are currently of great interest in philosophy, and I hope that my analysis of this episode between Sellars and Carnap helps shed some light on the history of those notions in mid-20$^{th}$ century analytic philosophy and thus, also, on how those notions have shaped our current debates.

## Robert J. Deltete

### Helm on Mach

Three passages from Georg Helm form the basis of my talk. The first comes from his history of energetics:

Mach has repeatedly and justifiably warned of the *mysticism* associated with the word "transform" that has sometimes tried to make its way into energetics. But it emerges clearly...that, judged by his manner of thinking, the *founder* of energetics [for Helm , Robert Mayer] does not need this warning....In the sense of its founder, energetics is a pure *system of relations* and is not out to place a new absolute [ie, energy] in the world. When changes occur, *this* definite mathematical relationship still subsists between them--that is the guiding formula of energetics, and certainly is also the only guiding formula of all true knowledge of nature. What goes beyond it is fiction.

Two others come from a memorial address that Helm gave shortly after Mach's death:

Mach has taught us that it is not the task of physics to `explain' natural phenomena by means of forces that work mysteriously behind the phenomena, and then come to be worshipped as their 'true' causes; rather, the task of physics to represent the relations between the facts of experience in a manner that they can be easily understood, and in a comprehensive way, so that they can be controlled.

Mach was rightly just as suspicious of any attempt to treat energy as a substance--as an essence standing behind the world of experience--as he was of atomism with its recourse to [substantial] forces. His efforts here, which have been very influential, seem to me to foretell the certain death not only of the currently comfortable concept of atoms, but of any concept insofar as it is an absolute, insofar as it is a substance, insofar as it tries to more than a summary of the relations given in experience.

In my talk, I shall argue that Helm fashioned his version of energetics as a quantitative relationalism of a sort that he attributed to Mach. In so doing, he adopted Mach's critical attitude toward substances and causes. I will sketch Helm's (unsuccessful) attempt to found all of physics and chemistry on what he called the "energy principle", and will argue that, despite some obvious affinities in outlook, Mach did not find Helm's approach congenial.

## Enzo de Pellegrin

### A lack of reverence: Schlick and Wittgenstein in 1926

The role of Ludwig Wittgenstein's thought in the discussions of the Vienna Circle in the years 1926-30 has long been obscured. His allegedly constitutive influence on the Circle in a previous stage of its development has either been praised or condemned wholesale. In either case, it has been customary - apart from noting the importance of the Tractatus-Logico Philosophicus - to focus on what sometimes is called an anti-scientific stance in Wittgenstein's writings. Hallmarks of an anti-scientific stance are frequently identified in his later work and are associated with the constructivist and anti-realist tendencies in his writings on the philosophy of mathematics.

The present paper aims at providing two negative results related to Wittgenstein's impact on the Circle through the mediation of Moritz Schlick in the seminal year of 1926. This was the year when Schlick eventually established a loose professional and personal relationship with Wittgenstein and when he offered a seminar on the philosophy of mathematics. [The previous term he had read on Bertrand Russell's Introduction to Mathematical Philosophy for the first time. It was the year before Rudolf Carnap took up regular teaching at the University of Vienna in the summer term of 1927.]

First, a brief survey of documentary records in Wittgenstein's Nachlass and of records related to his first interactions with members of the Vienna Circle serves to illustrate the central role of constructivist tendencies in his position in the mid-1920's. A lack of evidence for an anti-scientific stance and/or anti-realist position is noted.

Secondly, a detailed analysis of manuscripts and additional sources in the Nachlass of Moritz Schlick is given to assess the extent of exposure to Wittgenstein's thinking that core members and members at the periphery of the Circle experienced in the year immediately succeeding the first reading of the Tractatus in 1923-25. Schlick's role in a seminar will be examined at length. Most striking is his emphasis on an anti-constructivist stance in the philosophy of mathematics and his recurrent reference to structural realism. Schlick's position on this matter stands in stark contrast to Wittgenstein's emergent position, thereby illustrating the conceptual distance between the perspectives endorsed by the reclusive thinker and the leading figure of the Vienna Circle in 1926.

## Dennis Des Chene
### Life After Descartes

I examine various instances of opposition to Cartesian mechanism in the theory of living things. The primary distinction among the opponents is between those for whom the rejection of mechanism in biology followed from a general rejection of mechanism on metaphysical grounds, and those who came to reject it on the grounds that it was empirically insufficient. In the first group we find, for example, Cudworth and Leibniz, in the second Stahl and others. Although both groups tended to apply organic metaphors to their non-mechanical principles, only the second, which accepts mechanism in the inorganic world, can properly be said to be vitalist; the other is rather panpsychic or panbiotic.

## Maria-Filomena de Sousa
### Knowledge, Rules and Tradition

Hayek's 'Scientism' essay is usually pointed out as his most consistent philosophical piece of work, his most significant contribution regarding the epistemology of social science and a milestone regarding his methodological thought. Although I don't wish to dismiss such claims I want to argue that it is appropriate to regard the 'Scientism' essay and the related article 'The Facts of the Social Sciences' as a mere stage in the evolution of Hayek's thinking, as his understanding of the spontaneous order and the consequent assessment of the possibilities for social science undergoes a significant transformation from the 1960s onwards. Although I don't dismiss the significance of the three-part 'Scientism' essay, I believe that later work such as 'Law, Legislation, and Liberty' and 'The Fatal Conceit' offer a much more complete and sustained perspective on the question of the ontology of the spontaneous order and the related theory of cultural evolution. Moreover, we cannot evaluate Hayek's contribution to the epistemology of social science without taking into account this later work.

The specific path that Hayek was to follow in later work emerged for the first time in the article 'Rules, Perception and Intelligibility' (1962). This new path represents a foreseeable evolution rather than a radical transformation as it is the result of a more sytematized development of Hayek's previous insights and earlier work, however, the link between ontology and epistemology remained an important topic of his research. But from the 1960s onwards it is studied in the context of new ontological presuppositions, that is, in the context of the spontaneous order and of the theory of natural selection.

In this talk I will summarize Hayek's well known arguments regarding the spontaneous order of cooperation and the characteristics of the rules that allow for such an order to emerge. My ultimate goal consists in sorting out the possibilities for social science in the light of the theory of spontaneous order.

## Karen Detlefsen
### The Relation Between Advances in Microscopy and Malebranche's Conception of Nature

One way of reading the relation between a thinker's metaphysical commitments and his philosophy of nature is to investigate the way in which the former shapes the latter. This direction of influence is implicit in Descartes' "tree of philosophy" which suggests that medicine-one of the special sciences in the "branches" of the tree-is an outgrowth of and dependent upon the metaphysics found in the "roots" of that tree. I investigate a problem in natural philosophy that Malebranche deals with–the problem of organic generation–with an eye to the opposite direction of influence: what can Malebranche's use of advances made in the life sciences due to the advent of the microscope tell us about certain aspects of his metaphysics? Malebranche was well aware of the microscopic discoveries made by some of his contemporaries such as Swammerdam, Malpighi, and Leeuwenhoek. He uses these discoveries to argue in at least two different ways for the theory of generation by preformation, the theory that God pre-formed all living creatures at the creation of the universe. But Malebranche's theory of generation, and the use he makes of the empirical data, are interesting not just for what they tell us about the interplay between method and theory in Malebranche. These are also interesting because they tell us something significant about Malebranche's deeper metaphysical commitments, most especially what he must maintain regarding the nature of causation in the natural world.

## Robert DiSalle

### Theory and interpretation in the development of 20th-century physics

Einstein's special and general theories of relativity came into being accompanied by explicit philosophical principles, which seemed to provide not only motivation and warrant, but also the basis for the proper interpretation of the theories. In the development of quantum mechanics, similar principles were appealed to, at least by some of the founders of the theory, but no comparable consensus was reached regarding interpretation; indeed, the principles said to be inspired by Einstein were often regarded --even by Einstein himself-- as naive and simplistic. I argue that a proper understanding of the philosophical background and motivation for relativity will lead to a more subtle and sympathetic picture of the philosophical background to quantum mechanics, and will place the problem of interpretation in an illuminating historical context.

The historical conceptual shift from Newton's to Einstein's theories of space and time sheds some light on the problem of interpretation. One sort of "natural" interpretation of a spacetime theory emerges from an understanding of the way in which it identifies the conceptual limitations of earlier theories, while accommodating whatever genuine insight they express within an expanded conceptual framework. For example, special relativity (as Einstein and Minkowski presented it) identified the assumptions about simultaneity underlying Newtonian mechanics, and its seeming conflict with Maxwell's electrodynamics, and in doing so represented the Newtonian picture of space and time as a narrow local perspective on a more comprehensive spatio-temporal structure. General relativity, similarly, represented Newtonian gravitation theory as a kind of coordinate-dependent perspective that separates gravity from inertia, while in a more comprehensive picture their underlying unity is apparent.

For the spacetime theories of Einstein, however, the task of interpretation is simplified by the fact that, in both cases, the working-out of such an interpretation was identical with the development of the theory itself. In the case of quantum mechanics, by contrast, the large number of competing interpretations suggests a degree of arbitrariness concerning exactly where the crucial departure from the classical theory lies– that is, an arbitrariness regarding which classical assumptions we choose to regard as at fault. In order to arrive at an interpretation for quantum mechanics that has some of the plausibility of standard interpretations of spacetime theories–one that expresses, not merely the consequences of choosing to preserve some particular classical assumption, but an insight into the intrinsic structure of the theory– we would have to be able to show that an analogous process of conceptual criticism motivates some particular departure from the classical view. Heisenberg attempted to motivate his 1925 "quantum mechanics" by such an analysis; this element of his work, and of the development of quantum mechanics generally, tends to be under-appreciated, because the philosophical aspects of this development are usually seen (if not dismissed) as mere applications of broad-philosophical motives such as "positivism" or "operationalism." I suggest that Heisenberg's arguments, if not finally satisfying, nonetheless reveal a more subtle understanding of the philosophical foundations of relativity than he has been given credit for, and place the problem of the interpretation of quantum mechanics in an unusually clear perspective.

## Mary Domski

### Newton’s Philosophy of Geometry

Commentators such as Peter Dear and A. G. Molland have attributed to Newton a “constructivist" and “mechanical" philosophy of geometry similar to that espoused by his 17th Century contemporaries, Descartes and Barrow. In particular, both Dear and Molland grant Newton an interpretation of Greek geometry whereby “geometrical" constructions by straight edge and compass are endowed a loftier epistemological status than “mechanical" curves that require more complicated motions for their construction. While this reading of Newton is supported, to some degree, by the association made between geometry and mechanics in the Preface to the Second Edition of the *Principia*, a thorough examination of Newton’s unpublished *Geometria* (ca. 1692) yields a picture of his philosophy of geometry that I believe separates him from his “constructivist" contemporaries. In this present paper I will outline the arguments made in the *Geometria*, paying close attention to Newton’s appreciation of the power of Greek geometry to treat problems that extend beyond straight edge and compass constructions. Based on this discussion, I hope we can gain a firmer understanding of the remarks offered at outset of the *Principia* regarding the geometry of the ancients as well as Newton’s own “ancient" philosophy of geometry.

## Lisa Downing
### Newton and Thinking Matter

As John Yolton has documented, the debate over attraction in the eighteenth century was often connected to the issue of thinking matter. The thought was that if matter were capable of a manifestly active quality such as the ability to attract a distant body and thus produce new motion in it, the next step would be to attribute to matter the paradigmatically active power of thought itself. A prominent example is provided by Bernard le Bovier de Fontenelle, who, in his 1752 *Théorie des Tourbillons* invokes the specter of thinking matter against attractionism, stating that, for the Newtonians, "God could give thought to matter just as well as attraction."This connection gave Cartesians such as Fontenelle one more reason to cling to the vortices. It also posed both a problem and an opportunity for British natural theologians such as Samuel Clarke and Richard Bentley. Clarke and Bentley saw in Newton's theory of gravity a valuable confutation of materialist ambitions to reduce the world to the effects of matter and motion, that is, to mechanically explain all the workings of the universe. On the other hand, they could not allow it to subvert mechanism itself, that is, to undermine the mechanist view that matter possesses only size, shape, motion, and solidity. For if Newton's results argued that matter might have powers undreamt of by the mechanists, those powers might be held to include thought.Interestingly, in the one text where Newton really considers metaphysical questions, including the relation between mind and body, he seems to endorse the view that bodies may think. The text is the manuscript (never published by Newton) known as *De Gravitatione et Equipondio Fluidorum*. In this manuscript, Newton proposes a radical ontology that revises basic metaphysical categories and thus is not easily compared to traditional materialism or dualism.The paper attempts to elucidate the implications of Newton's *De Grav.* ontology for the question of thinking matter. I argue that Newton sees himself as endorsing a sort of dualism, in the sense of the separability of mind and body, while allowing for true mind-body union, which he thinks Descartes cannot accommodate. I examine how Newton might try to answer the questions which attend this balancing act. I also address the question of whether Newton's view of the mind-body relation has the sorts of implications that Clarke and Bentley regarded as the unacceptable consequences of thinking matter.

## Uljana Feest
### Of Rats and Psychologists: A Conceptual and Historical Analysis of E. C. Tolman's Operationism

While operationism is commonly associated with radical empiricism or verificationist theories of meaning, and usually believed to have been discarded, the position thrives amidst debate in contemporary psychology, and psychologists agree that historical analyses of operationism's origins, attending to its role in scientific investigation, are needed.  This paper analyzes E. C. Tolman's operationism of the 1930s.  For Tolman, operationism was a method for identifying “intervening variables" (regarded as causally efficacious components of a behavioral system) by way of controlled experimentation.  This position was the result of a view Tolman developed in the 1920s which held that rat behavior had to be accounted for in terms of “demands" and “cognitive postulations" (both later referred to as “intervening variables"), and that cognitive postulations represent objects in the environment in terms of how they can be used as tools for satisfying demands.  The former point raised the question of how to empirically distinguish between the two kinds of variables, and the latter point was central to Tolman's characterization of knowledge acquisition in rats.  Moreover, Tolman's model of scientific knowledge acquisition resembled his model of knowledge acquisition in rats: in both cases, the environment is represented via postulated outcomes of hypothetical operations.

I will discuss the influences that may have contributed to Tolman's views, including:  the New Realism of Perry and Holt, McDougall's theory of instincts, Yerkes's work on animal problem solving, Watsonian behaviorism, Gestalt psychology, the pragmatist epistemologies of Lewis and Pepper, Brunswik's probabilistic functionalism and, finally, the Unity of Science movement.

## Saul Fisher
### Mechanism and Atomism in Gassendi's Account of Plant and Animal Generation

As Dennis Des Chene notes in his *Spirits and Clocks*, Descartes offers a strongly mechanist model of animal generation in the *Description du Corps Humain*.  The mechanism is so strong, it turns out, that the generative aspect of the account is somewhat mysterious.  The new organism's development is explained by reference to the seed's current and native mechanist properties.  What is missing is an account of inheritance.  When the fetus develops in the womb, there is, in Des Chene's phrase, “…no ancestral memory, nor anticipation of fruits." (156)

By contrast, Gassendi's account of generation (*De Generatione Plantis* and *De Generatione Animalium*) offers an explanation, in mechanist terms, of how organisms create offspring to whom they pass on their traits. Development of the new organism is directed by a material 'soul' or *animula* bearing ontogenetic information. Where reproduction is sexual-as is most likely in animal generation-two sets of seminal matter and corresponding *animulae* meet and jointly determine the division, differentiation, and development of matter in the new organism. The determination of inherited traits requires a means of combining or choosing among each parent's contributions, and towards this end, Gassendi sketches the nature of competition and dominance among the *animulae*.

Unlike Descartes, Gassendi can offer a mechanist account of inheritance because he proposes a material vehicle for ontogenetic transmission, the *animula*. This proposal in turn relies on his atomist hypothesis, in that the uniform nature of atoms allows *animulae* to operate equivalently across different modes of generation-'pre-organized' or spontaneous. Further, his molecular model of atomic structures yields a material means of storing ontogenetic information received from the souls of parent organisms.

## Melanie Frappier
### The influence of Hilbert on Heisenberg's closed theories

In an interview with T.S. Kuhn, Heisenberg claims that, under Hilbert's influence, many physicists came to believe "that we may be forced to describe nature by means of an axiomatic system which was thoroughly different from the old classical physics." As I will show, Hilbert's ideas on axiomatization pervades Heisenberg's thoughts on "closed" theories (which he sees as complete systems of axioms, laws, and definitions giving us a knowledge of the laws of nature valid for all time). This is not surprising as both men share the belief that, in physics, understanding consists essentially in the knowledge of the relations existing between the different concepts of a theoretical framework rather than in the direct knowledge of the things to which those concepts are associated. Consequently, for both of them, completeness and internal consistency are essential components of any physical theories. However, I will demonstrate that, because Heisenberg thinks that theoretical change implies the development of a completely different conceptual apparatus, he cannot accept the reductionist program Hilbert attaches to his axiomatization of physical theories. Heisenberg's closed theories refer to different regions of reality and can simply not be reduced to one another. I conclude that, although avoiding some of the pitfalls of Hilbert's axiomatics, Heisenberg's approach remains unsatisfying as it does not give a precise enough characterization of closed theories, internal consistency, and the relations between the different theories. It can therefore not explain how Heisenberg can believe that theories that cannot be reduced to one another, like electromagnetism and quantum mechanics, can, one day, be unified into a single physical theory.

## Michelle Friend
### What a Proof Guarantees for Frege

For Frege, a gapless proof was meant to be truth preserving. That is, if we have a gapless proof, which starts with basic logical laws, then the conclusion is also true. This is very close to our modern definition of the validity of an argument, namely: if the premises of the argument are true, then so is the conclusion.

We all know this. What we seem to have forgotten, is that Frege's notion of proof was quite rich. His proofs in logic were meant to preserve both analyticity and universality from the axioms to the conclusion.

In Frege, 'analyticity' is given both a positive and negative definition. The positive definition is that a truth is analytic if and only if it follows from basic logical laws and definitions by means of a gapless proof. The negative definition is that to know an analytic truth we need not make appeal to sense experience or to Kantian spatial or temporal intuition. 'Universality' was defined in terms of applying to the universal domain; that is, the domain of all things. This characterisation of universality, together with Basic Law V, led to contradiction. Both definitions are problematic, but can be salvaged.

There are three questions I wish to address concerning the philosophical richness of Frege's proofs. One is: 'what feature, or set of features, of gapless proofs is meant to guarantee preservation of analyticity and universality?' The second question is: 'could we recognise these features in an arbitrarily chosen formal system of proof?' The third question is whether or not Frege's formal system (as presented in Begriffsschrift, and Grundgestze (minus Basic Law V)) plausibly achieve these goals.

Part of the answer to the third, less ambitious question, lies in Frege's choice of axioms. Part of the answer has to do with Frege's conception of logic as providing ultimate justification. Arguably, the concepts of preserving universality and analyticity can be identified with some other formal systems of proof. Some examples will illustrate this.

## Mathias Frisch

### Lorentz's Cautious Realism and the Electromagnetic World Picture

The project of finding an electromagnetic world picture arguably constituted the first big revolution in physics in the twentieth century. In contrast to many younger physicists, such as Max Abraham or Wilhelm Wien, Hendrik A. Lorentz, whose theory of the electron more than anyone else's contributions fueled the hopes for finding an electromagnetic foundation for all of physics, was rather guarded in his support for the project. Lorentz's cautious attitude towards the project is striking, since he indubitably was attracted to the kind of unified and conceptually simple account of physical phenomena the electromagnetic world picture promised.

In this paper I explore certain methodological or *meta*--physical views of Lorentz that may help explain why Lorentz was less unequivocal in his support of the project than, for example, Wien or Abraham. Lorentz's methodological views have thus far received very little attention in the philosophy and history of science literature. This lack of attention is unfortunate, since besides shedding light on his attitude towards the electromagnetic world picture these views are independently philosophically interesting and in several ways prefigure Albert Einstein's philosophical views. Even though Lorentz discussed philosophical questions only rarely in his writings and nowhere presented a fully developed methodology of science, there are enough *meta*-physical remarks interspersed in Lorentz's published works to suggest a substantive and interesting 'philosophy of science'.

Lorentz, I argue, was to some extent influenced by Heinrich Hertz's philosophical views. I explore the relations between Lorentz's views and Hertz's 'picture theory' of theories and argue that it is a mistake to attribute a straightforward scientific realism to Lorentz. The two views of Lorentz on which I focus in particular are, as I argue, his deep commitment to theoretical and methodological pluralism and his belief that our confidence that our best scientific theories in some sense correctly represent certain features of the natural world is ultimately based on a non-rational trust (a view that prefigures the "motivational realism" Arthur Fine has attributed to Einstein). Both these views suggest that one ought to approach universalizing theories in physics, such as the project of an electromagnetic world picture, with a certain amount of caution.

## Frédéric Fruteau de Laclos

### Le néo-comtisme d'Emile Meyerson

Sartre dans son article sur l'intentionnalité husserlienne déclare que toute la philosophie française est épistémologie, et épistémologie spiritualiste. Trois représentants de cette philosophie sont nommés : Brunschvicg, Lalande, Meyerson. Kuhn qualifie Brunschvicg et Meyerson de néo-kantiens. Deleuze à son tour voit dans les œuvres de Lalande et de Meyerson une façon de satisfaire à un certain kantisme. Si la qualification vaut à la rigueur pour Brunschvicg et Lalande (à la condition toutefois de déterminer en quoi consiste leur « kantisme » respectif), elle est fautive pour Meyerson. Emile Meyerson n'est pas néo-kantien, il est « néo-comtien ». Toujours chez lui l'épistémologie se dépasse en théorie de la connaissance, et la théorie de la connaissance à son tour vise à établir une philosophie de l'intellect. Le fameux schéma d'identification est une loi de l'esprit humain au sens de la loi comtienne des trois états, bien plus qu'il n'est une catégorie transcendantale. La question n'est pas celle des conditions de possibilité d'une expérience - question *de juris* -, mais celle des voies par lesquelles la raison a *de fait* cheminé à travers l'histoire de ses découvertes scientifiques. Un tel comtisme, plus profond encore qu'un simple emprunt de méthode, ne contredit nullement le rejet meyersonien du « positivisme » de Comte. Le rôle attribué par Meyerson au métaphysique (à l'ontologie), au légal (ou positif), enfin au théologique (à travers les causalités théologique et efficiente) montre en effet qu'en s'opposant au système de Comte il se place sur le même terrain que lui. Ce terrain devra nous permettre de mettre au jour au sein de la philosophie française une lignée toute différente de la lignée kantienne, celle qui court jusqu'à l'anthropologie des sciences d'I. Stengers ou de B. Latour.

## Michael Futch

### Temporal and Causal Asymmetries in Leibniz's Philosophy of Science

Within the past few decades, Leibniz's philosophy of time has been recognized as a precursor to causal theories of time. This means that for Leibniz, as for some of his more contemporary counterparts, temporal facts are identified with or reduced to more analytically basic causal facts. Consistent with his attempt to analyze time in terms of causation, Leibniz further believes that the direction of causation is given independently of the direction of time, and that temporal asymmetry is partially grounded on causal asymmetry. In advancing this thesis, Leibniz directly confronts one of the most formidable objections attending causal theories of time, for on many accounts of causation, causal asymmetry is grounded on temporal asymmetry in such a way that precludes analyzing temporal facts in terms of causal facts. As Lawrence Sklar has written,

> we can't "independently" establish causal priorities without first already knowing the temporal priorities . . . If Hume is correct, or if an analysis anything like this is correct, then at least a major component of the meaning of any assertion about the causal relationship holding among events will be a component describing the spatiotemporal relations holding among the events (*Space, Time, and Spacetime* 340-341).

If Sklar is correct in his characterization of causal priorities, then any purported causal theory of time, Leibniz's included, is doomed to failure.

It is clear from Leibniz's many writings that he categorically disavows a Humean analysis of causation according to which "the cause and effect must be contiguous in space and time . . . [and] the cause must be prior to the effect" (*A Treatise of Human Nature*, 1.3.15). Yet if Leibniz's disavowal of Humean conceptions of causation is apparent enough, much less obvious is how he explains the asymmetry by which an effect follows from its cause. In this paper, I explore Leibniz's many explanations of causal asymmetry, giving special attention to how he seeks to provide such an explanation without implicitly or explicitly invoking temporal asymmetry. I will conclude that, Leibniz's best efforts notwithstanding, he is less than completely successful in offering an account of causal asymmetry that does not presuppose a preexistent account of temporal asymmetry.

## Justin Garson
### The revival of emergentism in philosophy of science in the late 1960's

Following the publication of C. D. Broad's *The Mind and its Place in Nature* (1925), emergentism and emergentist themes rapidly disappeared from Anglo-American philosophy of science; with the exception of sporadically published and overwhelmingly critical articles, emergentism by and large remained absent in that literature until the later 1960's. The gradual revival of emergentism was initiated in the Anglo-American context by philosophically-oriented scientists such as Michael Polanyi, Roger Sperry, and Paul Weiss, rather than philosophers of science. This revival marked a significant shift away from the earlier British emergentist themes of cosmological novelty and unpredictability of macro-level phenomena toward themes that centered upon the macro-level or systemic control of micro-level phenomena, such as "macro-determination", "dual control", and "downward control". In the paper, I will elaborate some of these developments by focussing on the works of two scientists, Polanyi and Sperry, as well as some of the philosophers of science that responded to them, namely, Karl Popper, J. J. C. Smart, Robert Causey, Robert Klee, and William Wimsatt.

The development of emergentism from the late 1960's to the early 1980's is of interest to the history of the philosophy of science for at least four reasons. The first is that it was taken up by philosophers of science throughout the 1970's, and the conceptual articulation of the concepts of micro-level or systemic control by philosophers of science was crucial for the transformation of these themes into a conceptually coherent epistemological position. Moreover, this window of time provided a unique opportunity for philosophers of science to engage directly with philosophically-oriented scientists in journal articles. Secondly, the themes of macro-level or systemic control of micro-level phenomena established the conceptual landscape for the later notion of "downward causation", which was introduced into the philosophy of biology in 1974 but did not become a widespread topic of philosophical discussion until the early 1990's when it was appropriated as an ontological position in the philosophy of mind. Thirdly, the extent to which the newer emergentist themes of macro-level control were interwoven with explicit appeals to social and political value systems reveals an important social dimension of science and the philosophy of science. Fourthly, it marked a movement away from the predominantly reductionistic paradigm that had dominated the philosophy of science until that time.

## Yvon Gauthier
### La notion d'hypothèse chez Riemann

On a peu étudié la signification de la notion d'hypothèse chez Riemann d'un point de vue fondationnel, i.e. en tenant compte de ses ramifications tant mathématiques que philosophiques. À l'examen de ses textes mathématiques et de ses rares remarques philosophiques, on se rend compte que Riemann défendait l'idée d'hypothèse comme énoncé conditionnel (contrefactuel) pour définir une conception axiomatique où hypothèses, axiomes ou lois faisaient office de « *Thatsachen* », c'est-à-dire de faits établis internes à une théorie scientifique (1), le réel empirique étant limité aux phénomènes « *Erscheinungen* » au sens kantien. Bien au-delà de l'influence du philosophe Herbart ou de l'inspiration kantienne, Riemann adopte dans ses travaux mathématiques surtout une attitude plus constructiviste qu'empiriste, si bien que malgré ses préoccupations physicalistes, Riemann pourrait être perçu comme un philosophe des sciences moderne dans la mesure où il exprime des vues qui anticipent sur le positivisme logique, et mieux, comme un contemporain de Peirce dont le concept d'abduction se rapproche singulièrement de la notion d'hypothèse au sens de Riemann.

Je veux montrer en particulier que la genèse du concept d'élément linéaire $ds = \quad dx^{2}$ (métrique sur une variété différentielle avec structure pseudo-riemannienne ) obéit à la logique de la notion d'hypothèse dans son acception riemannienne. Les successeurs de Riemann , Helmholtz et Lie, ne s'y sont pas trompés qui prolongeront ses travaux arithmético-géométriques dans le même esprit et Hermann Weyl ne manquera pas de marquer la continuité des travaux du mathématicien Riemann avec les préoccupations fondationnelles qu'il a lui-même défendues tant du côté de la philosophie que du côté des mathématiques et de la physique.

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## Norma Goethe

### Frege's Account of a Legitimate Inferential Procedure and the issue of Proofs by Contradiction

According to Frege, a proof does not only serve to convince us of the truth of what is proved, but it also serves to reveal the logical relations between truths. Thus, he insisted that logical inferences must proceed from true grounds to consequences. Accordingly, he consistently rejected the legitimacy of deriving a consequence from a mere supposition. As Dummett points out, Frege's insistence on proceeding from truths to truths determined the axiomatic developments of logic. But, perhaps more importantly, Frege's account of a legitimate inferential procedure seems to exclude indirect proofs or proofs by contradiction, a fact which would make him join a long epistemological tradition in the theory of demonstration which values insight into the network of inferences or the *grounds* for the acceptance of a truth over the certainty afforded by a proof.

Breaking with this tradition, Kant sought to characterize the difference between mathematics and philosophy by the difference in the methods of proof they employ and, in order to prevent the antimonies of pure reason, excluded proofs by contradiction from the latter. Kant argued that their real home was in mathematics.

In contrast, according to Frege, if one counts logic as part of philosophy, the history of these sciences teaches us that there is a close bond between mathematics and philosophy. Also in mathematics there is a risk that the law of contradiction may fail, as the set theoretic antinomies show.

Frege argues that we make far too much of the peculiarity of indirect proofs vis-à-vis direct proofs, for the difference between them is 'not at all important', once we see that there are some necessary preconditions for the application of the excluded middle and proofs by contradiction.

The paper addresses the issue of Frege's reduction of such types of proof to direct proofs as well as some of its philosophical consequences.

## Ravi Gomatam

### Einstein's Critique of Quantum Theory - A Reassessment

Einstein is well known for questioning whether quantum theory (QT) provided a complete description of the individual system. This has led in turn to the widespread notion that Einstein envisioned completing QT from within by adding to its state description. Perhaps in a clear recognition that the rhetoric of completeness had been infelicitous, Einstein himself wrote as late as in 1949: "the testable relations which are contained in it, are, within the natural limits fixed by the indeterminacy-relation, *complete*." [Einstein's emphasis]

Taking three of Einstein's arguments, all involving thought experiments - the time of decay of a single radioactive atom, the "ink mark on the paper" argument and the EPR argument - we shall propose that Einstein's overall charge against QT viewed as a theory of the individual system is better seen as *inconsistency,* rather than incompleteness. That is to say, if taken as providing a description of the 'real' state of the individual system, QT is inconsistent. For example, QT permits the idea of a definite time of detection (ToD) of a particle (that is emitted as a result of the decay of an atom) while ruling out an idea presupposed by ToD (namely, a definite time of decay of the atom). In Einstein's view, even the consequences of nonlocality and inseparability are only due to relating the psi function to the individual system.

To avoid the inconsistencies, the options Einstein considered were *not* completeness versus incompleteness of description of the individual system, but complete of description of an *individual* system versus complete description of an *ensemble* of systems. Based on the latter view, Einstein did in fact provide in 1936, a 'holist' interpretation of QT that, he claimed, adequately disposed of the EPR argument, a point not sufficiently recognized in the literature thus far. The key idea behind his interpretation is that the psi function represents neither the absolute state of an individual system nor an average state of an ensemble of systems, but a state of the ensemble treated as a *single epistemic whole*. He endeavored to show how this state conception has proved to be predictively complete.

A truly complete theory in physics, however, must also provide a conception of the measurement-independent state of the *individual system*. Einstein based this stance on his view of scientific realism, wherein he proposed the need for developing a new "object conception" in everyday thinking that would be appropriate to guide quantum physical thinking. Thus, a complete theory (describing the individual system) need not necessarily feature locality and/or separability (since it would involve altogether new object concepts) as much as it would supply a consistent description. If we are right, Einstein's critique may yet have some useful insights for the ongoing efforts to ascertain the realist content of quantum theory.

## William Goodwin

### Intuition and Reductio Proofs in Kant's Philosophy of Geometry

The nature of Kant's appeal to pure intuition in geometry is a much debated aspect of his philosophy of mathematics; however, one consequence of this appeal which has been generally accepted is that the constructability of a concept in intuition is a necessary condition for one to have synthetic knowledge involving that concept. The idea that constructability-in-intuition is a necessary feature of geometrical concepts which figure in legitimate knowledge claims is appealing for several reasons. First, this constraint on geometrical knowledge seems to be a natural specification of Kant's Principle of Significance; that is, the claim that all concepts which figure in objectively valid judgments must relate to empirical intuitions. Furthermore, the constructability of mathematical concepts plays an essential role in Kant's explanation of the success of the mathematical method, for instance he says, "mathematical knowledge is knowledge gained by reason from the construction of concepts" (A 713, B 741). Lastly, if Kant's notion of constructability-in-intuition is assimilated to the Euclidean notion of the constructability of geometrical objects, then Kant's use of constructability as a constraint on knowledge harmonizes with the Euclidean emphasis on the constructability of geometrical figures.

Another feature of Kant's philosophy of mathematics that has received less attention than his appeal to intuition is his endorsement of reductio reasoning in mathematical proofs. Because reasoning in reductio contexts seems to require inferences from inconsistent sets of premises, it is not clear how, or if, judgments entertained in such contexts can be parsed such that their subject concepts are consistent. In Euclidean reductio proofs, one is often required to infer that non-constructible figures have certain properties that turn out to be incompatible (see, for instance, Euclid I.6). The most natural reading of these Euclidean proofs would be that they require one to make synthetic judgments whose subject concepts are not constructible (in the Euclidean sense). Thus, if Kant's constructability-in-intuition requirement is assimilated to Euclidean construction, that is, if a concept is constructible in intuition only if an instance of it can be constructed by ruler and compass, then Kant would be ruling out a form of geometrical reasoning which he seems to endorse.

In this paper, I will explore several options for reconciling the apparent conflict between these aspects of Kant's Philosophy of Geometry.

## Geoff Gorham

### The Metaphysical Roots of Cartesian Physics: The Law of Rectilinear Motion

According to Descartes' famous tree metaphor, metaphysics is to physics as roots are to trunk. In this paper, I attempt to uncover the metaphysical roots of Descartes' second law of motion ('all motion is in itself rectilinear'). Descartes says that the reason for the second law is just the same as the reason for the others: God continuously preserves the world, along with all its motions and transfers of motion, by the identical operation as when he first created it. In outline, his argument from the immutability of divine preservation to rectilinear motion is as follows. God preserves motion 'in the exact form in which it is occurring at the very instant he preserves it, without taking account of any earlier motion.' At any instant, God can only preserve a tendency to move along a straight line. Hence, an immutable God preserves rectilinear motion over time. (AT VIIIA 63-4, AT XI 44-5) What remains for modern commentators to explain is why God cannot instantanesouly preserve a tendency to move along a curved (e.g., circular) path. I critically analyze two significant recent interpretations of the argument: those of Dennis Des Chene and Daniel Garber. Des Chene suggests that rectilinear motion follows from the simplicity of divine operation, since rectilinear motion is the only kind of motion that can be specified by a single direction. Hence, “Descartes’ requirement of simplicity appears to be a transposition into his physics of the Aristotelian criterion of unity." (Des Chene, *Physiologia*, 285) But although this general requirement of simplicity would secure rectilinear motion, there is much more to Descartes’ argument. What Descartes emphasizes is that God is constrained to preserve motion strictly as it is at each instant, without regard to any others. But the requirement of simplicity would compel God to preserve rectilinear motion even if he took full account of earlier motions, and indeed even if he determined motion only at the beginning of the world rather than by continuous preservation. Garber argues that if we conceive of God’s preservation of motion as a continuous ‘divine shove’, then rectilinear motion should be expected since “at any instant the shove that produces the motion in time can only be a shove in one determinate direction." (Garber, *Descartes’ Metaphysical Physics*, 286) Assuming God’s immutability prevents him from shoving successively in different directions, this interpretation implies rectilinear motion. Unfortunately, Garber does not explain why God’s instantaneous shove could only give bodies a tendency to move in one direction, and not, for example, a tendency to follow a circular path. In the latter scenario, immutability would not be sacrificed, so long as God sticks to the same ‘curvilinear shove’ over time. I think the solution to this problem depends on fundamental principles of Cartesian metaphysics. For Descartes, the reason the world and its motions must be preserved ‘at each instant’ is because ‘the separate divisions of time do not depend on one another’. I argue that this doctrine depends in turn on the assumption that causes are necessarily simultaneous with their effects. These principles help us to understand the evidently crucial, but otherwise puzzling, role of time in Descartes’ justification of the second law. In particular, I think they can explain why Descartes emphasizes that God cannot ‘take account’ of any earlier motions when he preserves motion, and why it is so important to Descartes’ case against circular motion that ‘everything required to produce it [rectilinear motion] is present at each instant. . . whereas not everything required to produce circular motion is present.’ (AT XI 45)

## Godfrey Guillaumin

### Demonstration and Experience in Philosophical Magnetism during the Seventeenth Century

‘Demonstration’ was a central epistemological notion during the development of modern science in the seventeenth century. Even though ‘mathematical demonstration’ has been widely studied among several historians of science, ‘experimental demonstration’ has received no equivalent attention despite of it brought about several intriguing epistemological issues. During seventeenth century, there were different controversies among Gilbert’s followers and some anti-Copernican Jesuits in order to determine the role and importance of magnetical phenomena for the Copernican world system. Whereas pro-Copernican natural philosophers like Gilbert, Kepler, among others, defended the idea that there was magnetical evidence for the mobility of Earth, Jesuits as Kircher, defended the idea that there was experimental-magnetical evidence to demonstrate contrary conclusion: the Earth is not in motion. Although this episode in the history of magnetical science could be correctly considered as an excellent case for Duhem’s thesis on subdetermination, what I want to stress here is that magnetical controversies during 1600 and 1660 illustrate different epistemological characteristics of the early development of the notion of ‘physical demonstration’ in experimental philosophy. Unlike other cases in experimental philosophy developed during the seventeenth century, in magnetical experiments the same experimental devices (performed by William Gilbert) were used to defend contrary conclusions about the mobility of the earth and, even more interesting, both groups thought that they were entirely demonstrating their conclusions. This was not just a case of a problem of experimental reproducibility as much as an episode to set up the meaning of physical demonstration, its epistemological bounds, and the limits of experimental knowledge.

## Gary L. Hardcastle

### People, Machines, and Science: The Harvard Psycho-Acoustic Laboratory in the 1940s

The Harvard Psycho-Acoustic Laboratory (PAL) was, as James Capshew has noted, the "largest university-based program of... psychological research" in operation during WWII. Under the psychophysicist S. S. Stevens, the PAL boasted an interdisciplinary staff of fifty (including approximately twenty psychology PhDs) and produced hundreds of research reports on human communication in combat. More significantly, the PAL trained some of the most prominent experimental psychologists of next decades, including George Miller, J. C. R. Licklider, Eugene Galanter, Wendell Garner, Leo Postman, Karl Pribram, and Walter Rosenblith. I will consider the significance of the PAL from the perspective of the history of philosophical accounts of science, taking up Peter Galison's suggestion that in the PAL (and elsewhere) were forged connections between heretofore disparate disciplines, which in turn established robust "hybrid fields" and suggested to philosophers and scientists of the time not scientific fragmentation but a novel conception of unified science. We thus find in the PAL, Galison suggests, a new conception of science itself. Galison's understanding the PAL is not incorrect, but incomplete. My approach to the PAL seeks to reconcile Galison's account with competing views by understanding the ways PAL participants themselves understood the PAL and their work in it. I argue that we must recognize overlapping and incompatible visions of the PAL among PAL researchers, and, further, that many of the PAL's features, including its laudable ones, depended upon these different visions.

## Gary Hatfield

### The New Psychology and the Mind-Body Problem

During the latter third of the 19th century, experimental psychology sought self-consciously to establish itself as a natural science. In the course of these discussions, the status of psychological laws, and their relation to physical laws, was discussed by Helmholtz, Wundt, Mach, James, and Russell, among others. The mind-body problem framed but did not determine these discussions, since many considered the availability of (consciously experienced) psychological phenomena, and psychological laws covering them, to be better established than any solution to the mind-body problem. Although Helmholtz doubted the existence of psychological laws, Mach, Wundt, James, and Russell did not. Two different proposals were made about the relation between the mind-body question and the laws of psychology and physics. Wundt argued for a parallelism which recognized the autonomy of psychological causation from physical causation. Mach offered an epistemologically modest position that all that is known are the elements of sensation, which enter into both physical and psychological laws. James, and Russell in 1918, adopted a neutral monism, modeled after Mach's position but apparently embracing metaphysical monism.

I will examine the conceptions of psychological laws held by Wundt, Mach, and James, their appeal to actually established psychological laws (if any) to support their talk of psychological laws, and their attitudes toward psychological data and the mind-body problem. The aim is to gain a deeper understanding of Wundt, Mach, James, and later Russell's respective commitments to autonomous psychological laws, and to see what they felt were the problems, if any, with this notion.

## Sophie Hutin

### Le holisme et l'histoire des sciences: Un aperçu des holismes de Duhem et de Quine

Cette communication a pour but d'examiner l'influence des holismes de Duhem et Quine sur l'histoire des sciences. A cette fin, après avoir défini ce que nous entendons par ces holismes, nous mobiliserons les contextes théoriques de Pierre Duhem (1861-1916) et de Willard Van Orman Quine (1908-2000). Enfin, nous tenterons de conclure sur l'impact de la thèse holiste considérée isolément sur l'histoire des sciences.

En particulier, nous verrons en quoi et pourquoi l'histoire des sciences a chez Quine un rôle anecdotique alors même qu'il met au centre de ses réflexions le caractère évolutif de notre schème conceptuel et de notre langage. A l'inverse, pourquoi l'histoire des sciences et son enseignement sont-ils des préoccupations majeures de Duhem, alors qu'il a remis explicitement en question la conception d'une histoire de la physique comme accumulation de savoir ? Il apparaîtra que, chez Quine, l'histoire est toujours celle du particulier. Elle joue sans doute un rôle dans la clarification de notre schème conceptuel ; mais ce rôle demeure minime en tant que l'histoire n'aide pas à fournir les grands principes expliquant la genèse de notre théorie du monde. Ces derniers sont issus de l'observation des comportements verbaux, et non des histoires particulières menant à ces comportements. Néanmoins, il n'existe pas à proprement parler de réflexion de Quine sur l'histoire des sciences, *a fortiori* sur son enseignement. Au contraire, pour Duhem, un enseignement en histoire de la physique est nécessaire : il procède de l'impossibilité pratique pour l'étudiant d'apprendre la théorie physique comme un tout : l'apprentissage est toujours parcellaire. En sus, un tel enseignement a également la vertu de montrer en quoi la théorie physique devient progressivement une classification naturelle, c'est-à-dire comment l'ordre logique de la théorie devient peu à peu le reflet de l'ordre ontologique des phénomènes physiques.

This paper aims at appraise the influence of Duhem's and Quine's holisms on the consideration of history of science. For that purpose, after defining what we mean by holism, we will mobilize the theoretical background of Pierre Duhem (1861-1916) and Willard Von Orman Quine (1908-2000). Lastly, we will try to conclude on the impact of the Duhem-Quine thesis, considered in isolation from Duhem and Quine, on the conception of history of science. Particularly, we will see why history of science is anecdotic according to Quine, even though he puts in the center of his work the evolving character of our conceptual scheme. On the contrary, why history of science and its teaching are major concerns for Duhem, even when he questioned the conception of history of science as an accumulation of knowledge ? It will appear that, for Quine, history is always history of a specific thing. No doubt that it plays a role in the clarification of our conceptual scheme. Nevertheless that role is minimized because history is no help for providing big principles which explain the genesis of our system of the world. Those principles are isolated thanks to observation of verbal behaviours, but without knowing the particular stories leading to those behaviours. Nonetheless, properly speaking there is no reflexion from Quine on history of science, *a fortiori* on its teaching. Conversely, for Duhem, a teaching in history of Physics is necessary. It proceeds from the practical impossibility for the student to learn the physical theory as a whole : learning is always fragmentary. Moreover, such a teaching has got also the virtue of showing how the physical theory becomes slowly a natural classification, that is to say how the logical order of phenomenons becomes gradually the reflect of the ontological order in correspondance with physical phenomenons.

## David Hyder

### Foucault, Husserl and Historical Epistemology

French philosophy, including philosophically inclined history of science, is characterised in the post-war period by an anti-phenomenological turn. In a word, the thesis that history and epistemology are concerned with the reconstruction of conscious intentional events was rejected by the new generation of philosophers. Since this thesis was at best tacitly shared by members of the Vienna circle, it becomes increasingly difficult to link the concerns of writers such as Canguilhem and Foucault to their analytic counterparts, who by then were largely settled in America. In my contribution, I show how Foucault's rejection of phenomenology and his development of a so-called "archaeology of the sciences" can be related to concerns in what came to be English-language philosophy of science. I do so by showing how Foucault, in his *Archaelogy of Knowledge*, systematically undermines the theories of scientific meaning propounded by Merleau-Ponty and by Husserl in his late work, *The Crisis of the European Sciences*. Like Husserl, Foucault aimed at a history of science with epistemological import, but against the phenomenologists (and with Bachelard), Foucault's "historical epistemology" denies the intentional human subject a central position. Reading Foucault this way emphasizes how fundamentally such a project is opposed to the notion of a "social construction"-for one cannot be a social constructivist while denying the centrality of human experience and agency. At the same time, Foucault can be seen as going a road which analytic researchers only later followed. For the results that 1) the history of concepts is both epistemologically of interest, and that 2) this history is not simply a history of theories, but also one of technologies and experimentation, are indeed typical of much contemporary work, even if the latter draws its arguments more from Kuhn and Goodman than from Foucault.

## James Justus

### The Emergence and Fate of Cognitive Significance

The discovery of specific technical problems, the waning popularity of logical empiricism, and the rise in allegiance to scientific realism in the late 1950s and early 1960s led many philosophers of science to desert the project of formulating a criterion of cognitive significance. The criterion was intended to delineate the meaningful from the meaningless, the scientific from the metaphysical or merely poetic. This paper traces the history of the project from its inception in the 1930s to its abandonment in the 1960s, and assesses the reasons the project is thought to have failed. First, I briefly describe the emergence and problematic fate of the early criteria of cognitive significance based on the verifiability requirement for meaningfulness. These simplistic criterion proposals, for instance Ayer (1936, 1946) and Schlick (1936), were conclusively demonstrated to be inadequate by Lazerowitz (1937), Berlin (1939), and Church (1949), among others. Second, I describe the more sophisticated proposals of Achinstein (1963-4), Carnap (1936, 1937, 1956, 1961), Hempel (1950, 1951, 1965), and Reichenbach (1959). These later proposals were also judged to be inadequate following critical reviews. For instance, Carnap's (1956) proposal was thought unfeasible following criticisms by Kaplan (1959) and Rozeboom (1960). Significant problems confront these sophisticated strategies, but it is unclear they are plagued by formal problems of the same caliber as those facing earlier simplistic proposals. Thus, although Carnap accepted Kaplan's (1959) criticism (Kaplan 1971), he remained optimistic about the successful formulation of a criterion of cognitive significance based on his 1956 work until his death (Carnap 1963). One reason for his continued commitment to the project is that the technical problems raised by Kaplan and Rozeboom do not seem to be definitive.

### Berna Kilinç

### Kant's Notion of Objective Probability

One of the earliest distinctions between objective and subjective senses of probability can be found in the teaching of Immanuel Kant. In his lectures on logic in the second half of the eighteenth century, Kant used the qualifications objective and subjective in order to distinguish between the attitudes involved in the core examples of classical probability, such as the ones involving dice, and the appraisal of hypotheses that did not involve such dynamical set-ups, for example the probability of life in other planets. That Kant could countenance objective probabilities is surprising in view of his deterministic construal of the idea of nature. I claim that the surprise can be lessened when one attends to Kant's views on evidence in connection with the concept of objectivity he was the author of. In my talk, this claim is supported by an analysis and contextualisation of Kant's position within the juncture of two intellectual histories: history of probability and history of objectivity.

Comparing Kant's treatment of probability with that of his predecessors, for instance with George Friedrich Meier's *Auszug aus der Vernunftlehre*, leaves no doubt that Kant was familiar with the quantitative notion of probability that was developed by mathematicians such as Jakob Bernoulli. While the prevailing commitment to what can be termed a theocentric view of knowledge in these circles led them view probabilistic evaluations as subjective expressions of the limits on human cognition, Kant had no scruples rendering the same probability assessments objective. This shift can be understood in part as deriving from Kant's anthropocentric view of knowledge, together with one of the most original concepts he appropriated for its elaboration, viz., objectivity. Yet, this epistemological stance does not explain some aspects of Kant's philosophy of probability. The latter differed from the classical philosophy of probability despite the fact that an awareness of the boundaries of human reason was a main thrust of Kant's critical philosophy. That awareness did not lead Kant to adopt a probabilistic epistemology. When the collaboration of understanding and intuition did not licence objective cognition, reason could help itself with regulative principles of various sorts, but not conventional choices of hypothetical frameworks based on probability considerations. Since Kant predicated neither being objective nor being subjective in a wholesale fashion to all probabilistic evaluations, his discrimination between the two senses of probability involved a finer distinction than those to be found in global assessments of the human epistemic condition. Consistent with his philosophy of nature, that distinction does not appear at the level of understanding– probability is not a category or a concept derived thereof. Probability has no representative function like the latter, nor a regulative function like those provided by ideas. Probability pertains to another level of epistemic awareness, arising when the "grounds for belief" are analysed. I present in my talk this little explored topic of Kant scholarship by examining Kant's early writings on natural philosophy and the several compilations of his lectures in logic, as well as his critical works, especially the *First Critique*.

### Meinard Kuhlmann

### The Significance of Operationalist Arguments in Alternative Approaches to Quantum Field Theory, 1947 - 1975 and Today

Due to a considerable dissatisfaction with standard Quantum Field Theory (QFT) attempts to establish alternative approaches to QFT flourished particularly between 1947 and 1975. I will focus on axiomatic reformulations of QFT which were meant to rebuild QFT on conceptually and mathematically lucid foundations. My aim is to survey and evaluate the arguments which were stated against standard QFT and in favour of axiomatic approaches to QFT. Operationalist arguments are pivotal in this development and show the impact of philosophy of science on physics in periods where new theories or formulations are sought. Further kinds of arguments which display an influence of philosophical considerations refer to the ideal structure of theories, the hierarchy of physical entities, the definition of concepts and the significance of approximations and mathematical rigor.

I. E. Segal's programmatic 1947 paper on the "Postulates for general quantum mechanics" (*Ann. of Math.* **48**) can be seen as the starting point for this period since it introduces various ideas that were to play an important role in the ensuing discussions. 1975 marks a certain end to this first period with the comprehensive volume "Introduction to Axiomatic Quantum Field Theory" by N. N. Bogolubov, A. A. Logunov and I. T. Todorov. Within the period 1947-1975 Algebraic Quantum Field Theory (AQFT) is arguably the most successful attempt to reformulate QFT in an axiomatic manner. It originated in the late fifties by the work of R. Haag and quickly advanced in collaboration with H. Araki (*Comm. in Math. Phys.* 4, 1967), and D. Kastler (*J. of Math. Phys.* 5, 1964**)**. Other prominent attempts to axiomatise QFT were A. Wightman's field axiomatics (using quantum fields smeared out with test functions), the S-Matrix-approach by Bogolubov and others and the later Euclidean QFT. A common feature of all these formulations is the attempt to place observable entities at the base of the theory.

The period I am discussing was followed by about a decade of slow development and the feeling of crisis which was ended by a number of younger scholars with fresh ideas. I will conclude my survey with a comparison of the initial arguments and expectations with those given (partly by the same authors) in the last decade, i. e. about 30 years later. One result is that one can observe a change in the relative emphasis that is given to different arguments. While operationalist arguments got into the background general arguments about the structure of scientific theories have gained weight. A final point of my comparison will be how the relation of operationalism and scientific realism was seen by different physicists.

## Elaine Landry
### Structure in Mathematics and Science

The aim of this paper is the investigation of the historical development and current use of the notion of structure in both mathematics and science. I will argue that even if mathematical structure represents physical structure, unless we assume that structure itself cuts nature at its joints, we cannot claim that the semantic view of theories frames a structural realist interpretation of science.

The focus of the first section will be the claim that a category provides the schema for our talk about mathematical structure. I begin first with Corry's [1996] historical investigation of the development of the mathematical notion of structure. The objective here will be to distinguish the set-theoretic path of the Bourbaki notion of structure from the algebraic path of the category-theoretic notion. Two observations will then be made. The first, that the Bourbaki notion implicitly assumes an ontology out of which structures are made. The second, that this assumption leads to a reification of structure, i.e., leads to interpreting structures as independently existing things. In contrast to such readings, I will offer a schematic, category-theoretic, interpretation of structure.

In the second section of this paper I will consider what it means to say that category theory is a framework for mathematical structuralism though not a foundation for mathematics. The first step in this investigation will be to distinguish between mathematics *qua* science and mathematics *qua* discourse. The essential claim will be that mathematics is not about objects *qua* independently existing things (and, hence, is not a science in the ordinary sense of the term). Rather, mathematics is a discourse: it allow us to talk about objects *qua* positions in structured systems by way of their shared (or same) structure. The conclusion here being that if mathematics is about anything it is about the structure of various mathematical systems; that if mathematics talks about objects, it does so only by construing them as positions in structured systems.

The purpose of this last section is to provide a comparison of the above category-theoretically framed interpretation of mathematical structuralism with both the semantic view of scientific theories and scientific structural realism. The semantic view of scientific theories takes theories as collection of models, where models are taken as non-linguistic entities, i.e., the constituents of models are "the things" which the theory purportedly *is about*. In following this view of theories too rigidity, philosophers have neglected to note that in mathematics models are linguistic entities -they tell us what a structured system *talks about*. Viewed in this light, I turn to consider how a category-theoretically framed interpretation of mathematical structuralism bears up against current arguments for scientific structural realism. What I show is that, unless we assume that structure itself cuts nature at its joints, Ladyman's claim that "taking structure [as opposed to taking "facts" or "objects"] to be primitive and ontologically subsistent" (Ladyman, [1998], p. 420) cannot explain why "the semantic approach to scientific theories offers a natural framework for [a metaphysical interpretation of structural realism]" (Ibid., p. 411).

## Bruno LeClercq
### Husserl and Hilbert: Theory of Formal Systems

Le souci constamment affiché par le fondateur de la phénoménologie de développer une théorie de la constitution des idéalités mathématiques dans les vécus de conscience, mais aussi l'importance accordée dans l'oeuvre husserlienne à l'intuition catégoriale, ont souvent incité les commentateurs à rapprocher la philosophie des mathématiques de Husserl de l'intuitionnisme brouwerien. Certains des plus proches disciples de Brouwer, tels Hermann Weyl ou Arend Heyting, se sont d'ailleurs explicitement revendiqués des analyses de Husserl pour compléter ou renforcer leurs propres positions intuitionnistes. Réciproquement, on sait quel intérêt le phénoménologue Oskar Becker a marqué pour les travaux de l'école intuitionniste. Identifier les philosophies intuitionniste et phénoménologique des mathématiques serait cependant occulter tout ce qui, dans les travaux de Husserl, le rapproche bien davantage de Hilbert que de Brouwer.

Héritier sur ce point de Bolzano davantage que de Kant, Husserl est en effet en mathématiques un penseur de l'analytique plus que du synthétique, des rapports déductifs entre énoncés plus que des constructions des concepts. Comme chez l'auteur de la *Wissenschaftslehre*, ce sont les systèmes formels qui constituent la principale préoccupation de Husserl, et ce dès la *Philosophie de l'arithmétique* - dont la thèse fondamentale est précisément l'impossibilité de produire l'ensemble de l'arithmétique par construction - et jusqu'aux recherches génétiques des années 1920-1930 - qui continuent de distinguer explicitement la question de la genèse des concepts de celle de leur validation objective, qu'elle soit empirique ou, comme en mathématiques, purement formelle.

On sait combien la proximité de Husserl et Hilbert à Göttingen aux alentours du changement de siècle fut stimulante pour la réflexion des deux mathématiciens sur la fondation de leur discipline, et notamment pour l'investigation de certaines notions métamathématiques comme celles de consistance, de catégoricité et de complétude. Si c'est au seul génie de Hilbert que revient d'avoir développé de manière rigoureuse le projet d'une fondation formaliste des mathématiques, Husserl, dont les préoccupations prirent quant à elles une tournure philosophique plus générale, ne manqua cependant pas de se référer à ce même projet à chaque fois qu'il revint sur la question plus spécifique des objets mathématiques.

## Jean Leroux
### Bachelard and Logical Empiricism

The relation between Gaston Bachelard and the Vienna Circle is one of (almost) total mutual neglect. Bachelard did write in 1935 a series of reviews discussing Popper's *Logik der Forschung*, Reichenbach's *Wahrscheinlichkeitslehre*, and Hans Hahn's Logik, *Mathematik und Naturerkennen*, which had just been translated in French as *Logique, mathématiques et connaissance de la réalité*. But Bachelard's *New Scientific Spirit* (1934) was never reviewed in *Erkenntnis*, and in the following years his entire epistemological work was to be totally ignored by the Logical Empiricism movement. This situation was perpetuated even after of the demise of Logical Empiricism, with historically minded authors such as Kuhn and Feyerabend coming to the fore and ignoring altogether Bachelard's substantial work towards a historically-based epistemology of science.

I shall first briefly comment on Bachelard's early reviews of the Viennese group. I shall then present some aspects of Bachelard's epistemology that show affinities with the Vienna Circle Movement, while underlining specific grounds for their mutual neglect. The affinities relate to their respective philosophical agenda and also to common influences, such as Poincaré's structural objectivism and Mach's endeavor towards an epistemological historiography of science, while the neglect stems from these influences obviously going in opposite directions and involving divergent views concerning conventionalism, axiomatic method, and logic. Further divergences concern general philosophical questions such as anti-psychologism and the so-called linguistic turn. Finally, a brief comparative examination of Carnap's and Bachelard's brand of constructivism will serve to illustrate how different ways of worldmaking can be worlds apart.

## Eric Lewis
### The Concept of Body in the Hellenistic Period

One of the most important and fundamental physical concepts is that of body. Yet suprisingly the history of theorising about the nature of body as such has yet to be written. Precritically we think of bodies as being distinct from other existent yet noncorporeal entities in virtue of some property like resistance or impenetrability. Yet it is by no means clear that such notions entered into the earliest Western theories concerning body. After a brief survey of early Greek theories of body, I will turn to the Hellenistic period, in particular the Stoics and Epicureans, both of whom offer explicit, and contrasting, theories of body. I will demonstrate that their theories of corporeality, and the motivations behind them, are not what they seem to be. In particular, I will argue that the Stoics take causal efficacy as characteristic of the corporeal, and so do not characterize the corporeal as necessarily resistant. The Epicureans, on the other hand, do so characterize the corporeal, but for reasons that are far from obvious. They need not only to distinguish body from the void, but (like the Stoics) from mathematical body, which shares with physical body the characteristic of being three dimensionally extended. Along the way I will demonstrate that many texts which have been claimed to be presenting Stoic theories of body do not, but are in fact reports of concerning the Epicurean tradition. It is this Epicurean legacy of the corporeal as being "hard" (or perfectly hard) that returns with Gassendi into the early modern period, and is ultimately responsible for the incompatibility between Newtonian mechanics and atomism, an incompatibility not finally resolved until the advent of quantum theories of matter.

## Mathieu Marion and Paolo Mancosu

### Wittgenstein's constructivization of Euler's proof of the infinity of primes

In an unpublished paper, 'Zur Frage der Konstruktivität von Beweisen' (1930), Heinrich Behmann presented a flawed proof of a conjecture by Felix Kaufmann, in *Das Unendliche in der Mathematik und seine Ausschaltung* (1930, p. 66), according to which proofs of existence claims which do not depend on the axiom of choice implicitly rely on the exhibition of an instance satisfying the existence claim. As part of the proof, Behmann provided a method for transforming indirect proofs into direct ones. In the last part of his paper, he presented, as an example, a constructivization of the well-known proof by Euler of the infinity of the prime numbers. In a footnote, Behmann acknowledged that his general strategy and his constructivization of Euler's proof had been influenced by a constructivization of Euler's proof given by Ludwig Wittgenstein. The constructivization in question differs from the standard one given by Leopold Kronecker in *Vorlesungen Über Zahlentheorie* (1901, pp. 270f.) We shall give textual evidence that Behmann learned about Wittgenstein's proof through Friedrich Waismann and Kaufmann. We shall then present the constructive proof and show how it sheds light on Wittgenstein's remarks on Euler's theorem that are published in *Philosophische Grammatik*. Wittgenstein refers to Euler's proof as a "proof by circumstantial evidence" and adds that such proofs should "absolutely never be permitted" in mathematics. In these and surrounding passages he criticizes existence proofs and claims that philosophical clarity will "prune mathematics". Wittgenstein's constructivization of the proof is evidence of the depth of his thinking on these issues and a clear indication of his constructivist stance in the early 1930s.

## Jean-Pierre Marquis

### A Brief History of the Foundational Role of Category Theory

When Eilenberg & Mac Lane published their first paper on category theory in 1945, they saw that the theory could reveal the "fundamental concepts" of a field, but they did not claim that category theory could provide a foundational framework for mathematics. It was only in the sixties that Lawvere explicitly made that claim, but in different ways. First, in his doctoral dissertation, he set the stage for the development of categorical logic and then he presented the category of categories as a potential foundation for mathematics. Unfortunately, the latter proposal was soon showed to be technically flawed. Not long afterwards, elementary toposes were discovered, again by Lawvere but this time in collaboration with Myles Tierney, and *toposes* were considered to be an appropriate foundational framework for "ordinary" mathematics. Then, Mac Lane, Lambek, Bell and others suggested various ways of looking at toposes as appropriate foundational frameworks. But these are not only technically different, but they rest on different philosophical principles. Finally, the recent development of higher-dimensional category theory seems to allow a return to Lawvere's original idea, albeit in a different conceptual and mathematical dressing. We are therefore facing a zoo of proposals and it seems necessary to clarify and organize these various claims. In this paper, we propose to present the different views underlying these various proposals and try to sketch a history of these philosophical standpoints.

## Dan McArthur

### Why Bachelard is not a Scientific Realist

Analytic philosophers have largely neglected Gaston Bachelard's philosophy of science. This oversight is unfortunate since Bachelard's philosophy shares some revealing similarities with much analytic work of later decades. Nevertheless, despite years of relative neglect, some commentators in the analytic tradition, like Garry Gutting and Mary Tjiattas, have seen the possibility of constructing novel defences for scientific realism in Bachelard's work. These thinkers have found in Bachelard's work an account of experiments that shares similarities with the experimental realism of Hacking while at the same time giving due account to the internal-realism of Putnam and the historical views of Kuhn. This paper shows that this reading of Bachelard's views on experimental practice misunderstands some important features of his philosophy of science. Its also demonstrates that no defence for scientific realism can be derived from Bachelard's philosophy of science. This paper, then, provides an historical clarification of the nature of Bachelard's position. However, it also has a bearing on the debate over scientific realism because if my conclusions are sound, then one approach to defending scientific realism is ruled out.

## Patrick McDonald

### Helmholtz, Bernard, and the Epistemology of Experiment

Hermann von Helmholtz and Claude Bernard contributed significantly to experimental physiology and to experimental methodology. Bernard's *An Introduction to the*

*Study of Experimental Medicine* formulates his views on experiment systematically. Since Helmholtz produced no such treatise, one might think that he had no view to match Bernard's in power and scope. However, Helmholtz not only developed an epistemology of experiment, but he also presented what one might call an "experimental epistemology", in which the concept of experiment functions as a central component. In order to make an effective comparison possible, I will reconstruct Helmholtz's epistemology of experiment across a number of his epistemological writings.

Helmholtz and Bernard developed parallel themes, yet the former more clearly shows how experiment has an autonomous epistemic and constructive function in scientific practice. His clarity rests in part upon the fact that he builds a comprehensive epistemology around the power of experimental interaction to establish knowledge. Both authors clearly distinguished observation from experiment, arguing that controlled experiments effectively isolate causal connections, and both discussed the elimination of experimental error. Each argued that guiding ideas are necessary for fruitful experiments but need not compromise the soundness of experimental findings. However, Bernard provides only a sketch of an epistemological framework to justify such distinctions. Helmholtz's explication of the central role of experimental interaction explores in detail the dynamic relationship of theory and experiment. Bernard recognizes such dynamism, but does not explore the implications for scientific epistemology in general. I will conclude my discussion by showing how these different philosophies of experimentation were adapted by later writers with a more philosophical agenda, such as Mach and Canguilhem.

## Emily Michael

### John Wyclif's Atomism

John Wyclif (1320 - 1384), a prominent, if controversial, Oxford master was an innovative thinker and prolific writer, who is identified by his biographer (1926), H. B. Workman, and by such contemporary commentators as Kenny and Spade, as the evening star of scholasticism and the morning star of the Reformation. He is best known for his controversial account of the Eucharist, his attack against papal authority, and his opposition to ecclesiastical property. It was especially these views that, after his death, drew the attention and produced the dramatic action of the Council of Constance (1414-1418), much of which was devoted to the condemnation of Wyclif and his followers, Jan Hus and Jerome of Prague. That Wyclif was a bold thinker is reflected in his philosophical system and in his theological and political views. My interest here is in Wyclif's now little known natural philosophy. What I wish to examine is whether he can, with any justice, be dubbed the morning star of a reformation in science as well as religion, for the fact is that his distinctive contribution, anticipates, in some respects, developments of early modern natural philosophy.

In this paper, I will focus on what was perhaps Wyclif's most important scientific development, his distinctive atomism. I will examine Wyclif's arguments against the views of his scholastic predecessors, his arguments in support of atomism, the fundamental principles of his atomism, the nature of his atoms (perhaps better named point particles), and his account of molecules and of the formation of compound bodies. I will conclude with a brief consideration of the philosophical framework of Wyclif's atomism. I will compare this with the philosophical framework of two seventeenth-century atomists, Daniel Sennert and David Derodon, to suggest a significant similarity between these views of two eras.

## Pierluigi Miraglia

### Truth-Aptness and Logical Potential

I argue that there is a peculiar Fregean notion of truth-aptness that can be brought out by leveraging the analysis of what W. Taschek has called the 'logical potential' of assertions. It seems to me that the resulting interpretation of truth-aptness in Frege promises at least a couple of advantages in interpretation. It illuminates the significance and scope of the rather obscure Fregean conception of logic as encompassing the 'laws of truth'. It also explains the special status of the laws and rules of logic in Frege. Furthermore, I argue that we can use this Fregean notion of truth-aptness as a prism through which to cast a new, sharper, look at several long-standing philosophical issues. In particular, I shall use Frege's notion of truth-apness to look at the Geach-Frege problem.

## Gregory B. Moynahan

### Thinking the Infinitesimal: Hermann Cohen's Philosophy of Science and the Origins of Modern German Cultural Critique

In 1883, the philosopher Herman Cohen published a 'popularization' of his complex neo-Kantian philosophy entitled **"***Das Prinzip der Infinitesimalmethode und seine Geschichte*." Following on the success of his friend and teacher Frederick A. Lange's best-selling *Geschichte des Materialismus* - one of the most widely read philosophy books of the late nineteenth century - Cohen's work was to crystallize the philosophy of the Marurg school of neo-Kantianism even as it increased its accessibility. Cohen's works were in fact to have an immense influence on figures ranging from the philosophers Ernst Cassirer and Martin Heidegger, to the cultural critics Walter Benjamin and Aby Warburg. Yet ironically Cohen's popularization in the *Infinistesimalmethode* has remained cryptic both in intent and in meaning. Accepted solely as a pure philosophy of science by later philosophers (most notably Martin Heidegger), Cohen's works - and particularly the infinitesimal book -- were derided by later philosophers of science as obscure at best, mystical at worst. In this talk, I will outline one reading of the text and argue that an understanding of Cohen's work can indeed be greatly aided by his reading of the history of the infinitesimal. Ultimately, the reason for this is that the infinitesimal problem was for Cohen at the root of both cultural and natural science. The infinitesimal reveals a common link between thought, perception, and history that provided an important basis for the rise of the modern concept of cultural critique in both Cohen's own work on the philosophy of culture and those who read him. Once understood properly, it can be seen that Cohen's work not only presaged the later theory of paradigm shifts in thinkers such as Thomas Kuhn (a point that has been argued elsewhere), but proved equally fruitful for fields such as art history and intellectual history. Revisiting Cohen's work is of contemporary relevance since his study of the development of calculus and the infinitesimal provides an historical methodology that focuses solely on evolving practices in order to understand broad intellectual and cultural trends. Moreover, Cohen's work allows us to see that the modern fields of 'culture' (cultural history, cultural anthropology, cultural studies, etc.) may owe far more to the history and philosophy of science than they are aware. By suggesting one reading of the *Infinitesimalmethode*, I hope then to place in proper perspective the interrelated value of both Cohen's philosophy of science and of his philosophy of culture.

## Staffan Müller-Wille

### Boris Hessen's Philosophy of Science

The challenge that historically and sociologically oriented "science studies" pose against a methodological and normative philosophy of science has a clear origin: On July 4th 1931, a delegation of out-standing Soviet administrators, philosophers, and scientists appeared in a special session at the Second International Congress of the History of Science and Technology in London, to present their views on science to an international audience. The lecture with the strongest impact, both instantaneous and in the long run, was certainly that of Boris Hessen on *The Social and Economic Roots of Newton's 'Principia'*. In a forceful rhetoric - almost every paragraph consisting of a single, factual statement, each beating in the message and opening up whole bundles of promising research strains - Hessen confronted his audience with "a radically different conception of Newton and his work", in which "practice" was not "to be explained by reference to ideas, but on the contrary the formation of ideas [...] by reference to material practice", and which aimed at "understanding Newton, his work and his world outlook as the product of [his] period". Disciplinary historians and epistemologists alike suddenly saw their pristine object of curiosity - "pure science" in form of methodologies and theories - related to the mundane, extra-scientific realm of economic interest and religious prejudice, and, even worse, positioned amidst the ongoing "class struggles" of their time. Since then, though Hessen himself is rarely directly referred to, the problem of clarifying the relation of "pure" science to its "earthy core" (as Hessen called it) has remained the vital ferment to science studies, Thomas S. Kuhn's *The Structure of Scientific Revolutions* 1962 being the most prominent attempt to reconcile their results with the philosophy of science.

Though there have been studies clarifying the background that Hessen's contribution had in on-going debates within the Soviet Union about the status of quantum mechanics and relativity theory (e. g. Graham 1985), the philosophical argument he raised in his essay has not yet received a detailed, text-based analysis and re-evaluation. In my paper I will explore some of the subtleties that lurk behind his seemingly crude externalist explanation of Newton's achievements and short-comings. Rather than maintaining a simple determination of theory formation by extant technologies and ideologies, Hessen portrayed science as an activity that effectively transcended such constraints in an unforeseeable and, by consequence, undirectable manner. His essay may thus be seen as an early argument for the under-determination of scientific theory and against contemporary attempts to exert political control over science as a productive force.

## David K. Nartonis

### Idealist Philosophy of Science at Harvard, 1723-1859

When Louis Agassiz arrived in Boston, in 1846, he brought with him an idealist approach to the natural world. Agassiz and his philosophical realism were enthusiastically welcomed by the Harvard community, despite the fact that the official philosophy at the College was the nominalism of Dugald Stewart and his student, Thomas Brown. Historians have explained Agassiz's welcome by pointing to the growing influence at Harvard of European Romantic writers such as Coleridge and Novalis during the previous two or three decades. Here I will explore another factor in Agassiz's enthusiastic reception: books promoting an ideal view of nature and science had been a constant presence at Harvard from at least 1723. At least two of these books enjoyed a high level of interest among Harvard students in the late 18th century and interest peaked again in the early 19th century when faculty and students were first encountering the European Romantics. In this paper I will examine the place at Harvard of four of these books - Plato's *Timaeus*, Philo's *De Opificio Mundi*, Ralph Cudworth's *The True Intellectual System of the World*, and John Norris's *An Essay towards the Theory of the Ideal or Intellectual World*. In the process, I will show that a long term Harvard interest in an idealist philosophy of science (1) moderated acceptance of Locke and the Scottish philosophers, (2) prepared faculty and students to accept the European Romantic writers, and (3) long anticipated and then buttressed the idealist philosophy of science professed by insider, Benjamin Pierce, and then by new-comer, Louis Agassiz.

## Stephen Nazaran

### Tragedy and History: Émile Meyerson's a priori

In this paper I will examine Émile Meyerson's *a priori* and the method by which he establishes it. In the first section, I will explain his notion of the *a priori*; next I will examine in more detail his argument for it.

Émile Meyerson (1859-1933) was a French philosopher and historian of science during the rise of "History and Philosophy of Science" as a discipline in early twentieth-century France. Meyerson and many of his friends (such as Brunschvicg, Metzger, and Koyré) formed the nucleus of an anti-postivitic movement at this time. Despite Kuhn's acknowledgement in the preface to *Structures of Scientific Revolutions* of the influence of Meyerson's work upon his own, Meyerson's thought remains almost unknown.

Meyerson held that all cognition is dominated by an *a priori* tendency to "identify." This tendency is manifested when one attempts to explain phenomena by positing an identity of cause and effect (for example, the principle of the conservation of matter states that the quantity of matter before and after a change remains the same). However, explanation pushed to its limit produces a theoretical picture of the world in which both time and space have been eliminated, and the universe is a changeless undifferentiated singularity. To the extent that a phenomenon resists explanation by identification, it is "irrational." Meyerson claims that such irrationals exist in nature, can be specified *a posteriori* to some extent, and are absolute limits to knowledge. However, one cannot predict *a priori* the existence of specific irrationals. Thus, although the world can only be understood according to the schema of identification, the attempt to so understand it is continually, and in fact, inevitably frustrated by insurmountable irrationals.

I then argue that Meyerson replaces Kant's Transcendental Deduction of the *a priori* by a historical deduction. Meyerson argues that since introspective methods of understanding the progress of thought are unreliable (if not useless), we must resort to studies of the product of thought, such as the progress of scientific thought through history. Such a study reveals two things: first, the aprioristic causal tendency of thought, noted above. Second, historical study shows that this tendency is so strong that it often (if not always) drives us to accept principles that are neither completely *a priori* nor well supported by empirical evidence, but which nevertheless appear to explain phenomena by identification; Meyerson labels such principles as "plausible." Furthermore, both "successful" theories (e.g. chemical atomism), as well as "failed" theories (e.g. phlogiston) provide equally strong evidence for his theses. Thus, the "plausibility" of a principle does not guarantee that it is unassailable, and hence Meyerson can hold a robust doctrine of the *a priori* which should not be affected by scientific developments of the future. Finally, I consider the advantages of this historical method for the philosophy of science.

## Elisabeth Nemeth

### Socially enlightened science - Neurath on social science and visual education

First I want to show that some central features of Neurath's and Arntz's picture language are related to Neurath's position in the "Methodenstreit" (see Uebel 1996 and 2002). The "Viennese Method" of pictorial statistics reflects the combination of "individualism" and "holism" Neurath was advocating. The "Viennese Method" was an excellent intellectual instrument to develop Neurath's version of comparative economics further. Second: in the context of his reflections on visual education Neurath elaborated his views on how science could contribute to "social enlightenment". The aim of social enlightenment is not primarily to distribute scientific knowledge on social issues to the public, but to communicate and exercise a specific way of considering social phenomena. Visual education tries to transfer a specific "scientific attitude", "a quality not restricted to scholars only; there are laymen who have it and scientists who do not have it."(Neurath 1946) Third. The way Neurath characterized the "scientific attitude" he wanted to encourage by visual education, invites us to look at scientific practice from a unusual angle. Scientists and philosophers can learn something new about their own practice by looking carefully at what happens when social phenomena are represented in pictorial statistics.

## Alfred Nordmann

### The Power of Anecdote: Heinrich Hertz's Philosophical Appeal to the History of Science

Historians of the philosophy of science have contributed to the view that scientists themselves can and perhaps should be viewed as philosophers of science. Scientists articulate a particular conception of science implicitly through their practice (relating theory to experiment, choosing theories, adopting methodologies etc.) and explicitly through their declarations about proper scientific conduct, the significance of certain findings, etc. It has also been suggested that some scientists, at least, can be viewed as historians of science (e.g., Brush 1995). This paper begins with the suggestion that the way in which scientists relate to the history of science offers insight into their philosophical conception of science.

I propose to scrutinize the power of one particular appeal to anecdote. In his 1884 lectures on the constitution of matter Heinrich Hertz considers various general specifications of matter. He calls each of those specifications in question by adducing empirical evidence. The claims that matter is extended or that it is impenetrable are thus exposed as being merely a priori and supported only by prima facie evidence. Hertz departs from this procedure only in regard to the indestructibility of matter. The absence of direct countervening evidence does not lead him to endorse the conversation of matter as generally valid. Instead, he claims that in this case the history of science "proves" that the supposed indestructibility of matter results from a confluence of a priori and empirical considerations.

For his historical proof Hertz turns to the very recent past. "A couple of years ago ... [Paul] Schützenberger (Paris) brought to the attention of the chemical society" that the analyzed parts of hydrocarbons weigh more than the whole. "At roughly the same time [1881] the Englishman Thomas Carnelley caused a stir by claiming that he had succeeded in producing hot ice." Hertz points out that both of these empirical claims failed to shake the foundations of science. What interests Hertz is not that these claims were made but that the reaction of the scientific communtiy exposed the a priori character of the principle of the conservation of matter — in light of the supposed indestructibility of matter, Schützenberger and Carnelley "had to be wrong."

When scientists refer to historical precedent or contextualize their work within a research tradition, we might want to know what prompts this appeal to history, what is its supposed evidentiary or persuasive role, and what features of contemporary science are to be rendered salient through the established lineage. I will argue that the conditions under which Hertz appeals to the history of science underscore his decidedly ahistorical conception of science. My reconstruction of the scant documentary record regarding the claims by Schützenberger and Carnelley will show how Hertz's historical proof makes an epistemological point: Conservation laws and other a priori principles serve to organize certain phenomena but lack any evidence of truth beyond this ability to organize just those phenomena. This suggests what might be a more general pattern of scientists paying attention to the history of science as a means of advancing their philosophical views.

## Yoshinori Ogawa

### Idealization and Deduction

In a paper he wrote in 1928, Paul Bernays spoke of the need for a methodological discussion of the mathematical principles systematized in proof theory as a "philosophical supplementation" of proof theory and further remarked that such a discussion would provide those principles with a kind of "deduction." What is puzzling, however, is that Bernays's "deduction" does not seem to fulfil the obligations of a true Kantian transcendental deduction nor is it ever meant to: according to him, the principles (or assumptions) upon which mathematics is built cannot be recognized as TRUTHS (in the philosophical sense), and we should be content if we succeed, in proof theory, in establishing them to constitute a CONSISTENT system of thought or belief [Glaube]. The questions naturally arise then what precisely Bernays means by deduction in this context and what he thinks is achieved by it or, to put it more pointedly, what he thinks is the purpose of engaging such a "philosophical" investigation OVER AND ABOVE proof-theoretical ones. We begin to understand his meaning when we realize two things: first, Bernays considers the primary task of the deduction to consist in a clarification of the epistemological, methodological meaning of those principles and, more specifically, in a clarification of the methods of IDEALIZATION employed in mathematics; second, in his view, only with this, are we able to provide a satisfactory answer to his philosophical teacher at Goettingen, Leonard Nelson's question "What could the norm for an idealization be if it does not lie in pure intuition?" In this paper, I will first try to explain Nelson's account ofmathematical idealization as found in his discussion of issues surrounding the foundations of geometry. With the results of this investigation in hand, I will then consider what sort of methodological role Nelson assigns to the theory of idealization within the general framework of his Friesian philosophy of mathematics. I will conclude the paper by relating all this to the interpretive questions regarding Bernays's remark about the need of providing a deduction for the principles of infinitary mathematics. In so doing, I will try to identify and describe the Problematik that guided Bernays's (and perhaps Hilbert's) thinking in the foundations of mathematics in the late 1920s before Goedel's incompleteness theorems.

## John Ongley

### Anti-Positivism and the Idea That There is No Logic of Discovery

At the last HOPOS conference, I provided much textual evidence to show that the immediate source for the common 20th c. idea that there is no logic of discovery was neo-Kantian anti-psychologism, but I did not give the rationale used by the neo-Kantians to justify that idea. In this talk, after a brief summary of the last talk, I will continue the history of the idea that there is no logic of discovery by showing that the rationale for this form of anti-psychologism that implies that there is no logic of discovery is a form of anti-inductivist anti-positivism that ran throughout 19th c. Kantian philosophy, back nearly to Kant himself. This tradition of Kantian anti-positivism argued, on the basis of Humean scepticism, that there is no logic of discovery, and in particular, that induction cannot be such a logic. It is this anti-positivism that neo-Kantians such as Wilhelm Windelband, Ernst Cassirer, and Oswald Külpe used to justify their form of anti-psychologism that implied that there is no logic of discovery. Hermann Lotze was the first to express this anti-positivism, and its accompanying idea that there is no logic of discovery, in the form of anti-psychologism found among the neo-Kantians. Remnants of this anti-positivism can be found in Reichenbach, Hempel, Einstein, and of course Popper, indicating that it is the origin of their idea that there is no logic of discovery. The tradition of 19th c. anti-positivism discussed in this essay is also the source of the anti-positivism that ran throughout, and in fact defined, most of 20th c. continental philosophy. Besides Carnap, Reichenbach, Hempel, Popper, Einstein, Windelband, Cassirer, Külpe, Lotze, Mach, Frege, and Husserl, this talk will include discussion of Whewell, Ampere, Oersted, Kant, Schelling, and Jacob Fries.

## Eric Palmer

### Pangloss Identified: Science and History to Ground an Account of Morals

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

## Annie Petit

### Auguste Comte promoteur de l’histoire des sciences

Comte n'a pas choisi l’ordre historique pour présenter les sciences dans son système de philosophie positive. Cependant, l’histoire des sciences est toujours présente. Mais que vaut-elle ? est-elle originale par rapport à celle des contemporains ? les analyses "historiques" ne souffrent-elles pas des visées "dogmatiques" ? apportent-elles des leçons profitables aux progrès ultérieurs, ou ne s'empêtrent-elles pas dans la récapitulation ? sont-elles continuistes ou discontinuistes ? Ces débats, déjà violents au XIXème siècle et souvent repris, mettent en cause la valeur du système comtien autant en son temps que pour le nôtre.

On dégagera d'abord les place et rôle assignés par Comte à l'histoire des sciences ; puis ses thèmes et thèses — sur l'origine des connaissances, leur évolutions scandées de multiples "révolutions". On précisera aussi comment Comte tire de l'histoire ses conceptions des processus d'élaboration et de précision des connaissances, de la manière de travailler à leurs progrès, ainsi que des impératifs sur les chemins à ne plus suivre ou/et les recherches à ne pas engager ; bref prescriptions et proscriptions, normes positives et interdits. On reprendra également la “loi des trois états", présentée souvent avec plus de raideur que Comte ne lui a donnée : en fait, peu de déterminations strictes, rien de mécanique dans les successions souvent ponctuées de va-et-vient ; elle combine une présentation systématique — attentive à la répétition des attitudes intellectuelles, aux débats repris et continués, et analogies de progression des connaissances dans les différents domaines — avec une lecture attentive aux originalités, aux conditions spécifiques des phénomènes, aux particularités circonstancielles, personnelles ou institutionnelles. D'où l'allure paradoxale de l'histoire comtienne des sciences, avec des continuités ponctuées de ruptures, et la réitération des innovations.

## Jessica Pfeifer

### Mill on Laws and Systematicity

Mill defined 'law' twice over. In some sections of *A System of Logic*, he characterizes laws as unconditional general truths; in other places, he describes law-statements as those generalizations from which all other true generalizations follow. These two accounts appear to be independent, and perhaps even inconsistent. The latter seems to fit Mill's purported Humeanism, and is often touted as the source of the recently popular account of laws defended by Ramsey, Lewis, Kitcher, and Earman. This account is often referred to as the Best Systems Account of laws. In contrast, the former definition seems to commit Mill to a modal account of laws. After all, what could unconditionality amount to, if not a claim about what would happen under possible, and perhaps non-actual, conditions? This paper focuses on the relationship between these two seemingly incompatible accounts of laws. I argue that, when properly understood, these accounts turn out to be equivalent. This equivalence results in part from Mill's views about the nature of inference and in part from a proper understanding of unconditionality. Once 'unconditionality' and 'derivation' are properly understood, it will become clear that unconditional general truths are all and only those truths from which all other true generalizations follow. This has important implications for our understanding of the nature of systematizing and the relationship between systematizing and laws. Mill draws an important distinction between two modes of systematizing. This distinction allows us to characterize more precisely the role that systematizing may play in gaining knowledge of laws.

## Gualtiero Piccinini

### Experimental Epistemology

It is often said that contemporary cognitive science has many roots in traditional philosophical concerns over the problem of knowledge. However, the history of how cognitive science came to be driven and motivated by the epistemological work of classical philosophers has not been investigated. This paper contributes to this investigation by focusing on Warren McCulloch.

McCulloch, a prolific neurophysiologist and psychiatrist, was trained in philosophy and mathematics. He was fascinated by work in the foundations of mathematics and wanted to explain human knowledge in terms of neural mechanisms. In the 1930s, he began thinking about the brain as a logic machine, where the relations of excitation and inhibition between neurons would perform logical operations upon electrical signals. With an appropriate structure, McCulloch thought, the whole brain could embody a logical system like the one in the *Principia Mathematica* of Whitehead and Russell, which would account for how humans perceive and think. Accordingly, McCulloch set out to discover the “logic of the nervous system," and worked on this project until his death. While doing so, he made fundamental contributions to neuroscience and computability theory.

McCulloch also belonged to an intellectual lineage in neurophysiology that goes from Lotze and Helmholtz to Magnus to Dusser de Barenne. These authors were all explicitly concerned with the physiological foundations of perception and knowledge, including the idea that Kant’s synthetic *a priori* knowledge is grounded in the anatomy and physiology of the brain. Dusser de Barenne consciously inherited from his mentor Magnus the quest for the physiological *a priori*, and he transmitted it to his collaborator McCulloch while McCulloch worked in Dusser de Barenne’s lab between 1934 and 1941. McCulloch saw himself as continuing the tradition from Kant to Dusser de Barenne, and would refer to his theory of the brain as solving the problem of the physiological *a priori*.

McCulloch deserves more credit in the history of cognitive science than he is usually given. In a paper written with Walter Pitts in 1943, he was the first to publish, in embryonic form, the view that brains are computers, which he elaborated and expressed in a number of presentations and publications. His work strongly impacted other cognitive science pioneers such as Turing, Wiener, von Neumann, and others. In the 1940s, McCulloch was a leader of the cybernetics movement. In the 1950s and 1960s, he was an established figure at MIT, where he influenced several generations of neurophysiologists and artificial intelligence researchers.

Among the pioneers in the history of cognitive science, McCulloch was perhaps the most acquainted with philosophy. In his papers and presentations, he referred regularly and insistently to the philosophical tradition and to the need to solve epistemological problems by studying the brain experimentally and building mechanical models of neural processes. He called his intellectual enterprise *experimental epistemology* in the 1950s, a sign reading "experimental epistemology" hung from his MIT lab's door. Thus, there is much evidence that McCulloch was a major contributor to the rooting of cognitive science in the concerns of epistemology.

## Mary Pickering

### The Status of the Intellect in the Last Works of Auguste Comte

In the last years of his life, Auguste Comte, the "founder" of positivism and sociology, celebrated the importance of the affections in such works as the *Système de politique positive* (1851-1854) and the *Synthèse subjective* (1856). To him, the cultivation of sociability, or what he dubbed "altruism," was the key to the health of both the individual and society. The purpose of this paper will be to investigate his changing attitude toward the intellect in these works. In setting up a new religion–the Religion of Humanity–did Comte completely abandon his earlier emphasis on ideas as the motor of history? What effect did his new interest in the sympathies, especially love, have on his celebrated hierarchy of the sciences? In seeking to answer such questions, this paper will shed light on Comte's growing dissatisfaction with reason–a dissatisfaction much in keeping with the direction of the century, which would end with the antipositivist philosophy of Nietzsche.

## Chris Pincock

### Carnap's Physical Construction System

In his 1928 *Logical Structure of the World* or *Aufbau* Carnap outlines a psychological construction system in which all genuine concepts are reconstructed in experiential terms. At several points in the book, however, Carnap suggests that a physical construction system is also possible. This construction system would take physical objects and relations as primitive and reconstruct all genuine concepts in physical terms. In this paper I investigate the details of this physical construction system based on Carnap's remarks in the *Aufbau*, his published writings prior to 1928 and his unpublished writings such as the 1924 *Topology of the Space-time World*. By comparing the physical construction system outlined in these writings with the psychological construction system of the *Aufbau* I hope to clarify exactly what construction systems were and what Carnap hoped they could accomplish.

Michael Friedman and Alan Richardson have quite successfully argued against the traditional interpretation of the *Aufbau* as a work firmly rooted in British empiricist concerns. One of their arguments relies on Carnap's acceptance of a physical construction system. If Carnap is willing to accept physical construction systems, they argue, then he is clearly not requiring that the basis of all construction systems be epistemically incorrigible. I review their arguments and conclude that a traditional empiricist interpretation of the *Aufbau* is unacceptable.

At the same time, though, the very neo-Kantian interpretation that Friedman and Richardson have advanced is ill-equipped to account for Carnap's physical construction system. Briefly, neo-Kantians were concerned with demonstrating the objectivity of our knowledge despite its origins in subjective experience. This issue simply does not arise for a physical construction system as its basis is composed of supposedly objective things and relations between these things. I argue that the physical construction system calls the adequacy of the neo-Kantian interpretation into question. While it may shed light on the particular construction system of the *Aufbau*, it does not help us to understand what construction systems more generally were meant to achieve.

I conclude by suggesting a new perspective on Carnap's work in this early period. This perspective emphasizes his claims of philosophical neutrality. I argue that only if we take these claims seriously can we come to understand what the purpose of his construction systems were. Construction systems were meant to reconstruct our knowledge in order to clarify and unify it. No more substantial philosophical purpose is consistent with Carnap's own claims of neutrality.

## Anya Plutynski

### R.A. Fisher and Sewall Wright: Philosophy of Science for Population Genetics

Theoretical population geneticists use mathematical models to describe and explain evolutionary change at the population level. Thus, the discipline stands at the intersection of these three major changes in evolutionary biology in the $20^{th}$ century: the establishment of genetics as a discipline, the mathematization of biology, and in part as a result of each of these, the synthesis of the new science of Mendelian genetics and evolution. This early synthesis in biology deserves more attention from historians of philosophy of science. Did the early population geneticists have a philosophy (or philosophies) of science? I will argue in this paper that they did, and will trace the origins of two of the major founders of population genetics differing ideals for a science of population genetics.

R. A. Fisher, I argue, comes from a "nomothetic" tradition - his object was to derive a rigorous mathematical theory of evolution akin to thermodynamics. For biology to take its place as a legitimate science, in Fisher's view, it needed to articulate the sort of mathematical laws that one finds in physical science. Fisher made both substantive and formal analogies between the theory of gases and the theory of genes in populations - and these analogies in part made possible his synthesis of biometrical views of evolution and Mendelian views of heredity. Populations of organisms were to be conceived as clouds of point masses - buffered by the forces of selection, mutation, migration and drift. The object of inquiry was changes in frequencies of genes. The patterns and processes of evolutionary change, the entire diversity of life, could be best represented by diffusion models of gene frequencies in populations. In short, Fisher's vision for a science of biology was strongly influenced by his training in physics by James Jeans as an undergraduate at Cambridge. My object in the first half of this paper will be to trace this influence and understand how it shaped Fisher's vision for biology.

Sewall Wright claimed contra Fisher, to have an "organismic" view of evolution. What did this mean? Wright's philosophy of science drew upon a tradition in physics and biology that took the object of inquiry to be large-scale patterns and processes; his method was holist. Wright remarks in the opening passage of his famous 1931 paper:

> . . . the evolutionary process is concerned, not with individuals, but with the species, an intricate network of living matter, physically continuous in space-time, and with nodes of response to external conditions with it appears can be related to the genetics of individuals only as a statistical consequence of the latter. From a still broader viewpoint (Lotka, 1925) the species itself is merely an element in a much more extensive evolving pattern… (p. 99)

For Wright, evolution is not simply change in gene frequencies. Wright here was shifting the object of explanation up from genes to species. Wright was concerned with the dynamics of whole evolving systems, or with large-scale patterns, such as population structure, and with the balance of both deterministic and stochastic forces that lead to greater or lesser genetic heterogeneity. He spoke of a population of organisms as having greater or lesser "plasticity," or "lability". Here he is drawing upon a 'holist' or 'organicist' tradition, and specifically upon Lotka's *Elements of Physical Biology.* Lotka believed that the distinctions between the organic and inorganic were merely quantitative, rather than qualitative, and that patterns and processes in the living world and in the inorganic realm ought to be studied using the same physical principles. A biological system ought to be analyzed in the same way as a physical chemist would analyze a chemical system (Kingsland, 1985). The same metaphors, tools, and concepts could be extrapolated from one realm to another - exchange of energy, steady state equilibria versus displacement. The second half of this paper will trace some organicist influences on Wright and how they shaped his vision for biology. My object will be to understand how these different views of a science of evolution evolved, and what their impact was on science.

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## Paul Pojman

### Mach's Biological Origin, Purpose, and Nature of Science

I wish here to explore, as fully as possible, all the ways in which Mach used biology within his Philosophy of Science; I will summarize them as follows:

A Biological Origins of Science

B Biological Purpose of Science

C Bio-psychological Nature of Scientific Change and Progress

A Biological Origins of Science. Mach puts science on a continuum with earlier human activity, in fact with earlier animal activity. He wishes, in a sense, to naturalize not only standard epistemology, but also science: "The adaptation of thoughts to facts, accordingly, is the aim of all scientific research. In this, science only deliberately and consciously pursues what in daily life goes on unnoticed and of its own accord." [AS: 316] Science has not only a deeply biological origin in being produced by evolution, but science was produced by evolution for a biological purpose.

B The Biological Purpose of Science. Mach's frequent statements that science has a biological purpose have sometimes been misunderstood as meaning simply that science aids us in our struggle for survival or personal fulfillment. But Mach has a deeper meaning here. Science doesn't just aid us in literal survival, but rather in the further adaptation of our cognitive structures to the world: "The biological task of science is to provide the fully developed human with as perfect a means of orientating himself as possible. No other scientific ideal can be realized, and any other must be meaningless." [AS: 37] This is not a normative statement, but descriptive of what is actually happening.

C The Nature of Scientific Change and Progress. Mach can be seen as the precursor of what is today one of the major epistemological analogies used to model scientific change. Mach offers a 'Darwinian' account of group-level change consisting of selection upon naturally produced variation, passed on through processes of heredity. As with Darwinian evolution, variation itself is not directed.

This selection process produces the same structure as the Darwinian tree of life, complete with branching and extinctions: "In thus dealing with the objects of his conceptual life, his ideas unfold and expand, like his nervous system, into a widely ramified and organically articulated tree, on which he may follow every limb to its farthermost branches, and, when occasion demands, return to the trunk from which he started." [PL: 231] An outcome of this is the skeptical observation that we can never be sure that we aren't out on a branch which will dry up in the future: "But we must never imagine, - and this physicists have learned from Faraday and J. R. Mayer,- that progress along paths once entered upon is the only means of reaching the truth." [PL: 217] Thus theories compete with each other, and a kind of survival of the fittest takes place - those theories which are better adapted to their environments (the facts and current theories) survive and become the next set of conceptions. I also look at aspects of Mach's thought which are non-Darwinian.

## Ofra Rechter

### Kant on Definitions in Arithmetic Across the Critical Turn

Kant's claim that there is more than mere "heuristic advantage" to the mathematical use of signs or symbols has been thoroughly explored by Charles Parsons and dominated the writings of Michael Young. In this paper I argue that the clarification of this claim can be enlightened and corroborated by analyzing its complex evolution across the Critical turn. I focus specifically on the role that definitions play in Kant's early and Critical explanations of our knowledge of numerical identities. A precursor of Kant's Critical conception of the symbolically constructive character of definitions emerges in the Prize Essay where arithmetical definitions are creative and serve the consideration of the universal under signs, in concreto. But in the Critique numerical equalities, for which proofs from definitions Kant has offered in the 1760's, are now conceived as indemonstrable and immediately certain.

Gottfried Martin had proposed that the recognition of the implicit use of associativity in such proofs as Leibniz's attempt to derive 2+2=4 from definitions by general logical means convinced Kant that arithmetical identities presuppose synthetic principles and are therefore synthetic.

Martin's proposal has been convincingly contested by Charles Parsons and indirectly challenged by Michael Friedman. I recover the insight implicit in Martin's proposal to address how Kant's pre-Critical views on the relation between definablity of the numerical concepts and the demonstrability of the numerical identities have evolved. In conceding Martin's claim "that Kant's dissatisfaction with the Leibnizian proof is due to his new awareness of operational rules irreducible to the logical principle of contradiction" but maintaining that for Kant they would not be axioms, Beatrice Longuenesse assumes that Kant's pre-critical view of the status of elementary numerical identities was Leibnizian. I argue that it is not. I argue that Kant's view from 1763 appeals to formal properties of the decimal system of Arabic numerals to show that in arithmetic the universal is considered in the single instance symbolically. From the Critical perspective, however, unless the operations with signs were proved to be sound, their "secure inferences" are insufficient for establishing that the formalism can serve as a model for the numbers.

In his notes from Kant's lectures of 1762-4 (Ak. 29:1) Herder produces examples of arithmetical definitions that illustrate Kant's Prize Essay discussion of them. For instance, "4=3+1" is a definition that appears in the setting of a proof of 4+8=12. I consider in detail Kant's treatment of 8+4=12 against the background of the lectures and the Prize Essay discussion and contrast it with Kant's critical analysis of 7+5=12 in B14-6, A163-5/B204-6 and the 1788 letter to Schultz (Ak. 10:555-6). On this basis I argue that both views present a constructive attempt to establish "what is said of number in arithmetic, that one could increase it, always and without end, by the appending of units or numbers" (Ak. 20:240).

### Laura Rediehs

### Redefinitions of Objectivity in the 20th Century

In this paper, I trace the histories of two discussions about objectivity in the 20th century. My paper begins with a review of the so-called "philosophical writings" of Niels Bohr. In these philosophical reflections on the epistemological crisis brought about by quantum physics and relativity theory, Bohr initially claims that the new findings in physics reveal to us the "subjective character of all physical phenomena" (1929). Later, he changes his language. Instead of suggesting that physics must incorporate subjectivity, he changes his strategy to one of expanding the definition of objectivity. Observations need to be reported in ways that include a description of the experimental arrangement, and objectivity is redefined by 1954 as "unambiguous communication," securing its unambiguousness by using mathematical symbols and avoiding reference to conscious subjects. Bohr's discussions over time then show a shift from an initial belief that the new physics represents a departure from objectivity to the claim that the new developments in physics have brought about a refinement and expansion of the very notion of objectivity.

Challenges to standard understandings of objectivity are raised again in a different but not completely unrelated context towards the latter part of the 20th century. Thomas Kuhn's book *The Structure of Scientific Revolutions* (1962 and 1970) raised important questions about the objectivity of scientific methodology and the objectivity of scientific theories or paradigms. While some philosophers of science, such as Lorraine Code (1993), respond by suggesting that incorporating subjectivity into science might in fact be helpful rather than harmful, others employ a strategy similar to that of Bohr sketched above: they try to redefine objectivity. I examine especially the redefinitions of objectivity proposed by Evelyn Fox Keller (1985), Helen Longino (1990) and Sandra Harding (1993).

Objectivity is already a concept that has undergone important changes over a wider span of time, as described in papers by Lorraine Daston, and by Daston and Peter Galison (1992). The questions to be raised in my paper are (1) whether the kinds of redefinitions of objectivity offered by Bohr, Keller, Longino, and Harding are related to each other or are fundamentally different in important ways, and (2) whether these redefinitions represent progressive shifts, as their proponents try to argue, or whether in fact they indicate that there remain important unresolved problems in the systems of thought that these versions of objectivity were invented to defend. That is, rhetorically, the term "objectivity" functions to defend the rigor of a system of thought, whether that system of thought be scientific or philosophical. But how far can a concept become stretched before it loses its most important meanings and no longer adequately plays its originally intended role within language? Are the 20th century changes in the notion of objectivity helpful either to physics or to philosophy of science, or do they reveal epistemological crises as yet unresolved?

### George Reisch

### To the Icy Slopes of Logic: Logical Empiricism, the Unity of Science Movement, and the Cold War

This paper documents the political vitality of logical empiricism and Otto Neurath's unity of science movement after its emigration to the United States. It examines the cooperative social and intellectual relations between the leaders of the unity-movement (Neurath, Rudolf Carnap, Charles Morris and Philipp Frank) and the so-called New York Intellectuals (including Ernest Nagel, Sidney Hook, Horace Kallen and others) and their largely shared political agenda. During and after World War II, however, these relations become strained. Hook and Kallen, in particular, became highly anticommunistic and antitotalitarian and they attacked the unity of science movement as totalitarian and soft on communism. Along with additional evidence that Neurath and his movement were perceived by intellectuals and anticommunists (including the FBI) as extremely leftist, these circumstances help explain the demise of the unity of science movement after 1945 and the increasingly apolitical and technical character of professional philosophy of science in the U.S. Possibilities for (still more) revisions of Kuhn's role in the development of science studies are then outlined.

### Alan Richardson

### The Pragmatic and the Empirical A Priori: Pragmatism's Resources for Relativizing the A Priori

Ever since Quine, in his rejection of Carnap's and C.I. Lewis's philosophies, neatly bundled the naturalistic, the pragmatic, and the a posteriori, it has been difficult to recover the actual historical relations among these notions within the American philosophical context. As it happens, however, in the 1920s and 1930s, just as Quine was receiving his philosophical training, American pragmatism issued its most detailed and compelling accounts of the a priori element in knowledge. Given the stress that has been placed on the relativized a priori in recent accounts of early logical empiricism, it is of particular interest that the leading pragmatist accounts of the a priori being developed at the same time also endorsed relativized notions of the a priori. This paper scouts the reasons for and accounts of the relativized a priori in work in the 1920s and 1930s by Lewis, Dewey, and Morris. It argues that one can find a common theme in the various accounts on offer: The a priori in pragmatism was ever to be thought of as a sense-conferring commitment to constraints on inquiry--a position in its philosophical motivation not a million miles away from either Carnap's notion of the analytic or van Fraassen's a priori empiricism. The paper ends by sketching an alternative to Friedman's recent elaboration of a relativized a priori for our times.

## Jason Scott Robert

### Revisiting Kant and Blumenbach on the *Bildungstrieb*

J.-F. Blumenbach, in his 1781 treatise, *Über den Bildungstrieb und das Zeugungsgeschäfte,* accounted for organic structure by invoking a new, pseudo-Newtonian, specifically biological force - the "development drive" or *Bildungstrieb*. According to Blumenbach, the *Bildungstrieb* could not be reduced to chemical particles, and he portrayed it in plainly teleological terms. But it was not an *ante res* force acting from without, or somehow imposed on matter. In this regard, Blumenbach was not a standard-issue vitalist: rather, for Blumenbach the *Bildungstrieb* had no existence apart from its material constituents. But it was emergent from these constituents and not reducible to them.

In his 1790 *Critique of Judgement*, Kant helped to systematize Blumenbach's views, and to formalize a consensus that had been building up between a number of biologists during the latter half of the eighteenth century. According to most interpretations, the Blumenbach-Kant position reconciles a version of preformationism with a version of epigenesis: what is preformed is a material development drive that emerges from and yet also guides the epigenetic development of the individual organism. The organism is thus both cause and effect of itself - the embryo embodying and fulfilling "the law of its own being", to borrow a phrase from E.S. Russell. The position avoids the usual charges against vitalism by insisting that the vital organizing force is not an independent entity, but rather an emergent property materially and lawfully dependent on the composition, order, and arrangement of the parts of the ontologically prior whole organism.

Pinto-Correia (1997, 305) represents a near-consensus view in arguing that "Kant's and Blumenbach's last conciliatory concept ..., in which epigenesis is directed by a set of preprogrammed instructions, is not, in its essence, all that far removed from our current views in developmental biology". But Richards (2000) has recently argued that the historical situation is considerably more complicated than most historians admit, and that Kant and Blumenbach never reached a deep and abiding common understanding (Richards 2000). In this paper, I explore salient historical and philosophical aspects of the Kant-Blumenbach position, its interpretation by historians, and also its putative verisimilitude to modern developmental biology.

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## Laszlo Ropolyi

### The "Hungarian" Lakatos

Lakatos emigrated from Hungary as a young people (in 1956 when he was 34 years old), and became well known as philosopher of science in England. When he lived in Hungary he wanted to follow a double purpose: to go forward in both political and academic fields and from time to time he was able to combine somehow these purposes in his personal practice. When he moved to England he already wanted to devote himself to the pure academic work. However, his philosophy of science embodied his earlier double purpose: it is a common theoretical representation of scientific and political practice. To explain the formation of his ideas we have to turn to Lakatos' life and political and philosophical activity of the 1940-50 years in Hungary.

Lakatos' extraordinary life in Hungary caught the public's attention in the last few years, unlike his political and philosophical writings and practice in his early years. We intend to concentrate on the latter aspects of Lakatos' activity.

The young Lakatos published (in Hungarian) about ten short political and philosophical papers and completed his doctoral dissertation in Hungary. His political writings contributed to party policy and were considered significant in those times. As a clever and committed ideological fighter he took part in numerous political and ideological debates and practical actions. His dissertation and his philosophical papers were written under the influence of Marx and Georg Lukács (especially his History and Class Consciousness). His dissertation was mysteriously lost, however, its content can be reconstructed from his published papers. Based on his Hungarian papers we try to characterize the young Lakatos' philosophy of science and compare the ideas of the “Hungarian" and the “world famous" Lakatos.

## Thomas Ryckman

### The Failure of Anti-Apriorism in Philosophy of Physics

The first panellist argues that one moral to be drawn ever more clearly nowadays lies ready to hand already in an analysis of the work of early 20th century philosophers of physics like Reichenbach on one side and Eddington and Weyl on the other. Neglect of attention to substantive a priori determinations leads to the dilemma of naturalism: either one is forced to argue for realism or for some form of instrumentalism, both of which are highly unsatisfactory. This paper begins a comparative case study of Reichenbach and Weyl with their logical reconstruction of the general theory of relativity and extends the analysis to their treatment of quantum physics. The failure of Reichenbach’s approach on both accounts points to the failure of anti-aprioricism as such.

## Rose-Mary Sargent

### Francis Bacon’s Experimental Activity

It has long been the received view that Bacon did not perform experiments himself and thus his methodological advice could be dismissed as empty rhetoric irrelevant to the historical development of experimental practices. In this paper I counter that prevailing opinion by providing an analysis and evaluation of Bacon’s experimental activity as he described it in his later works. Although he showed some familiarity with experimentation in the twenty-seven Prerogative Instances included in Book II of his *New Organon*, Bacon was overly optimistic in his assessment of how all other experiments could be as easily performed and used as the ones he there described. After his retirement from public life, however, he had more time to devote to natural investigations and his advice on experimental practice became more complex and sophisticated as his familiarity with the performance of experiments became deeper. In such works as *Natural and Experimental History for the Foundation of Philosophy* and *Sylva Sylvarum*, he acknowledged both the need for the use of “imperfect axioms" in the design and interpretation of experiments and the numerous practical difficulties that could arise in the attempt to perform them. He went on to insist that full details surrounding all of these aspects of methodological design, performance, and interpretation must be faithfully reported when composing natural and experimental histories in order to advance learning about natural processes and to improve the techniques employed in experimental practice. This account of Bacon’s experimental activity not only does justice to his works by countering the belief that he advocated a simplistic empiricist and inductivist methodology devoted primarily to fact gathering and the mechanical discovery of law-like regularities. It also serves to explain how it was that the subsequent generation of English natural philosophers could credit Bacon as their primary influence.

## Sahotra Sarkar

### Methodological Solipsism and Phenomenological Reduction: A Husserlian Technique at the Center of Carnap’s Aufbau

Fodor has claimed that Carnap’s *Aufbau* is the source of the position that he dubbed “methodological solipsism." However, a careful examination of Carnap’s use of “solipsism" in the *Aufbau* reveals a much richer doctrine which has both methodological and ontological aspects. Moreover, the method has strong similarities to Husserl’s technique of phenomenological reduction as deployed in the early sections of *Ideas*. While Carnap alludes to Husserl’s technique in some sections of the *Aufbau*, the similarities are stronger than he indicates. Carnap’s reluctance to identify his method more strongly with Husserl’s presumably arises because of his explicit desire to maintain tolerance for alternative bases (autopsychological, physical, *etc*.) that can potentially serve as a foundation for the logical construction of the world even though the autopsychological base is supposed to have epistemic primacy over the others. (Husserl, in contrast, has no such option: only one phenomenological base can serve such a foundational role.) Thus, this earliest manifestation of Carnap’s characteristic conventionalism already begins to create a tension between his project and those of his predecessors. Noting the Husserlian roots of Carnap’s use of an autopsychological base clarifies the sense in which the details of Carnap’s construction deviate from Russell’s external program even though that program, in a very general sense, motivates Carnap's project in the *Aufbau*.

This paper is the second of a set of four papers that explore the influence of Husserl on Carnap's early work. Together these papers develop two themes: (i) Carnap's participation in the Vienna Circle resulted in a constriction of his philosophical interests which, in turn, led to a much narrower conception of philosophy than what he initially started with; and (ii) the radical rejection of traditional philosophy, especially as it was formulated by Neurath, along with Heidegger's rejection of much of the epistemological claims of science, led to a divergence between scientific philosophy and phenomenology which was inimical to the early Husserl and Carnap.

## Jutta Schickore

### "…a contemplation of the whole of Science and its History" - William Whewell, the context distinction, and HPS

The "standard" distinction between the context of discovery and the context of justification is first of all a distinction between the processes which occur when new ideas a brought up and the arguments which exhibit and assess the degree in which those ideas are supported by evidential considerations. This distinction has been used to demarcate philosophy of science from empirical studies of the sciences, such as psychology, sociology, and, for my paper most important, history of science. In the debates about possible links between history and philosophy of science, the context distinction has been a focal point, and several historically-minded philosophers felt compelled to argue against it. It is thus remarkable that one of the earliest advocates of the context distinction, William Whewell, also begins for the first time to devote serious attention to the history of science. By reconstructing Whewell's position, we may gain further insights into what precisely is at stake in the discussions about HPS and the distinction.

My paper begins with a brief review of recent arguments regarding the context distinction and HPS. The main part of the paper reconstructs the specific version of the context distinction that Whewell advocated. To do so, I explore the apparent tension between his oft-quoted claim that "scientific discovery must ever depend upon some happy thought" and his conviction that "no discovery is the work of accident". I analyse how he combines his distinction with a detailed study of the history of science. Special attention will be given to the "fundamental ideas", which, in Whewell's project, are the crucial link between history and philosophy of science. In conclusion, I consider the lessons that can be learnt for the current debates about the role of history for philosophy of science.

## Warren Schmaus

### Did Kant transform Philosophy? The case of France

It is often held that subsequent to Kant's critical philosophy, it was no longer possible to pursue either the Cartesian rationalist or the Lockean empiricist program of providing a foundation for the sciences. For instance, Paul Guyer argues that Kant's transcendental deduction of the categories, in showing that self-knowledge as well as knowledge of external objects involves judgment, undermined any attempt to provide a foundation for knowledge in our certainty about our internal states, independently of any knowledge of the external world. To be sure, Guyer limits his historical claims about Kant having transformed philosophy to German idealism, logical positivism, and contemporary linguistic philosophy. I will argue that this claim does not hold true for much of French philosophy in the Nineteenth Century.

In France, far from Kant's philosophy having undermined Cartesian self-introspection, it was joined to it. In particular, Kant's transcendental apperception, through which he said in the transcendental deduction we are conscious of the unity of our experience, was assimilated to Descartes's *cogito*. Beginning with Victor Cousin, French philosophers denied Kant's distinction between transcendental and empirical apperception, rejecting his notion of a pure apperception, unmediated by either categories or empirical intuitions, of the mind's activity in unifying our representations. From Pierre Maine de Biran, they took the argument that causality and the other categories could be derived from the apperception of the mind's activity. This gave rise to a foundationalist philosophy according to which our knowledge was grounded in an empirical apperception of the categories.

Contrary to the usual historical claims about the eclectic spiritualist philosophy of Cousin and Biran having died during the July Monarchy, this philosophy continued to exert an influence on higher education in the Third Republic through philosophers whom Cousin had trained, such as Paul Janet. Philosophy textbooks and student notes from as late as the 1880s reveal that students continued to be taught that the categories had their source in empirical apperception. It was in reaction to this philosophy that the Durkheimians proposed an alternative, sociological theory of knowledge in which the categories were derived from collective, cultural rather than individual, psychological experiences.

Hence, regardless of what one thinks of the relative strengths and weaknesses of the arguments of Kant, Cousin, Biran, and other philosophers, there were powerful institutional reasons for the persistence of a kind of philosophy that Kant's transcendental deduction of the categories was supposed to have ruled out. Thus, in giving an historical explanation of the subsequent influence of Kant's transcendental deduction, it does not suffice to provide a careful analysis of his arguments. It may not even be necessary. What matters is not only how these arguments were read at the time by various philosophers, but what role these philosophers played in educating the next generation of philosophers.

## Lisa Shabel

### The 'Axioms' of Geometry in the Early Modern Period

Prior to the nineteenth century, one typically thinks of the 'axioms' of geometry as comprising the five postulates of Euclid's *Elements,* or possibly the five postulates in addition to the five common notions. Our contemporary familiarity with these axioms derives chiefly from Heath's definitive translation of the text of Heiberg, as well as from his accompanying commentary. But, early modern editions of Euclid's *Elements* (particularly the texts with which Kant was familiar) organized and augmented these propositions in widely varying ways. Though Euclidean geometry was uniformly conceived to be a completed and foundational science in the early modern period, it was nevertheless not uniformly presented as a strict axiomatic science founded on a single set of first principles. Since knowledge of geometry provides a touchstone for the epistemologies of so many modern philosophers, including Kant, it will be valuable to explore the mathematical practices with which these philosophers were familiar.

In order ultimately to assess Kant's philosophy of geometry, I propose here to assess first the mathematical foundations of that science, as Kant himself understood it. I will describe the sorts of variations evident in geometry texts from the early modern period in order to determine what the modern geometer conceived as the basis for the science of geometry. I will show, in particular, that the axioms set out, though formulated differently in distinct texts, shared a 'self-evidence' that typically depended on the constructibility of spatial diagrams to illustrate them. I will suggest, further, that Kant's explanation for the self-evidence of these axioms depends on the *a priori* constructibility of space itself.

## Lisa Shapiro

### The Health of a Hydraulic Machine?: Nicholas La Framboisière and Descartes on the Regulation of the Passions

How is a mechanist like Descartes entitled to advert to the health of the body-machine? Insofar as it is a corporeal entity, a body-whether 'well' or 'unhealthy'-follows the laws of nature. On what basis then, can a mechanist make normative claims about the workings of the body? To address this question, I compare the accounts of regulation of the passions of Descartes and Nicholas Abraham de la Framboisière (1560-1636), professor of medicine at Reims. The regulation of the passions was, in the 17th century, very much a matter of maintaining health. While La Framboisière writes in the Galenic tradition, and Descartes would seem to want to be distanced from that Aristotelian tradition, there are similarities in their accounts. La Framboisière thinks regulating the passions requires manipulating the dynamics of bodily fluids. And Descartes maintains, in *The Passions of the Soul,* in keeping with his view that bodies of living things are complicated hydraulic machines, that regulating our passions involves regulating our bodies' workings. We need to temper the "excitation of the blood and spirits" by thinking thoughts that counter or dampen these movements and by cultivating in ourselves generosity-the passion associated with firm and constant movements of the animal spirits (PS a.160, AT XI 452). This comparison suggests first that Descartes, in his mechanism, still retains something of Galenic humour theory in modeling living bodies as hydraulic machines, not simply a machine in which various parts interlock and move together, but one in which something flows. Second, it highlights the difference between Galenic and mechanist sources of the norms of health. I suggest that for Descartes "the way in which the machine of our body is composed" (PS a. 7, AT XI 331) replaces the substantial forms that ground Galenic medicine. Though this 'composition' of the body seems to do the work of a formal cause, it still fits the mechanist conception of the physical world as governed by laws of nature alone.

## Bonnie Shulman

### The Value of Value-Free Mathematics

There have been many attempts throughout history to bring the precise, rigorous and exact methods of mathematics to bear on moral philosophy. In the 20th century, attempts to apply mathematical reasoning to ethical decision making even led to new mathematics. Karl Menger wanted to purge the study of ethics of subjectivity, and used mathematics to construct an abstract system to study the logic of relationships, which he purported to be value free. Oskar Morgenstern was impressed with this work, and used it as a model for his own attempts to apply mathematics to economics. He went on to co-author a book on Game Theory with John von Neumann. In a fascinating feedback loop, the new mathematical theory is now being used to model ethical situations. This historical episode (the Menger-Morgenstern-Math-Ethics connection) provides a case study of mathematics being developed for and alongside a particular field of inquiry (in this case, the social sciences, particularly economics), analogous (as Morgenstern and von Neumann themselves pointed out) with the development of the Calculus, alongside astronomy and mechanics. We have here an opportunity to explore the complicated interaction between methodologies and content in the process of knowledge-making. I use this episode to illustrate the permeability of the boundary between the context of discovery and the context of justification, and show the influence of values on the very content of mathematical knowledge.

## Kurt Smith

### The Place of Enumeration in Early Modern Physics: Making Possible the Mathematization of the Physical World.

The concept of the 'enumeration' and its place in a combinatorial theory of analysis and synthesis can be traced back to the Twelfth-century Spanish theologian Ramon Lull. Arguably, Descartes's first work, *Regulae ad Directionem Ingenii* (1628), is a work that is centered around the basic ideas in Lull's *Ars Parva*. One of Leibniz's first works, *Dissertatio De Arte Combinatoria* (1666), is centered around the basic ideas of Lull's *Ars Magna*. And, it would seem that Leibniz's purchase in 1670 of a copy of Descartes's *Regulae,* in conjunction with his interest in Lull's work, would culminate in the writing of a number of related works: *Of an Organum or Ars Magna of Thinking* (1679) and *Of Universal Synthesis and Analysis; or, Of the Art of Discovery and of Judgment* (1683). Although both Descartes and Leibniz disagreed with much of what Lull did in the *Ars Parva* and *Ars Magna*, they nevertheless found great power in the theory of enumeration.

Although Descartes does not say much about it, arguably his insight into the conceptual power of the enumeration is directly connected to his insight into the mathematization of the physics. Leibniz also makes this connection. According to him, *algebra* is based on the concept of the enumeration and the rules for combining its categories. Robert Boyle also makes the connection between the enumeration, a combinatorial theory of categories, and the construction of *algebraic* equations in physics in Chapter III of his *An Introduction to the History of Particular Qualities* (1671). In this paper, I show exactly how the concept of the enumeration and a combinatorial theory of categories is connected to what Descartes and Leibniz referred to as *Mathesis Universalis*, and how it led to the mathematization of the new physics. In light of this, the paper *briefly* visits the history behind enumerations, ratios, and the concept of proportional unity, connecting Pythagoras, Anaxagoras, Scotus of Erigena, Lull, Galileo, Descartes, Leibniz, Boyle, and Newton.

## Laura J. Snyder

### The Science of the 'Dismal Science': Debates on Political Economy in 19th-Century Britain

In his novel *Hard Times* (1854) Dickens, ridiculing utilitarianism under the name "Gadgrindism," wrote,

> It was a fundamental principle of the Gadgrind philosophy, that everything was to be paid for. Nobody was ever on any account to give anybody anything, or render anybody help without purchase. Gratitude was to be abolished, and the virtues springing from it were not to be. Every inch of existence of mankind, from birth to death, was to be a bargain across a counter. And if we didn't get to Heaven that way, it was not a politico-economic place, and we had no business being there. (Bk. III, ch. viii)

This passage is interesting for two reasons. First, it indicates that, in 19$^{th}$ century Britain, an interest in political economy pervaded even popular literary culture. Why was there such interest in political economy in popular culture? At least part of the explanation is that at this time the number of the urban and rural poor was on the rise, and so were the costs associated with poor relief. Attempts to curtail these expenses had led to a series of riots and strikes by the laboring poor both in the late 18-teens and in 1830. There was a general sense that something needed to be done. It thus became important to define the science of political economy, and to determine whether and to what extent the science could be applied to solve the problems faced by society. For this reason it is not surprising that many of the day's leading writers on logic and scientific method (including J.S. Mill, William Whewell, John Herschel, Archbishop Whately, Charles

Babbage and others) were interested in the "dismal science," as Carlyle famously dubbed political economy.

Another noteworthy point indicated by Dickens' sneer at political economy is that at this time the subject was often, in the general public, confounded with utilitarian moral philosophy. Further, it was a particular form of political economy that was confounded with utilitarianism: namely, Ricardian political economy, with its abstraction of the "economic man," who acts exclusively with a view towards attaining a maximum value with a minimum sacrifice. As I will argue in this paper, the equating of utilitarian moral philosophy with Ricardian economics had an important consequence for attempts to define the science of political economy in the 19th century. Certain writers who were against utilitarianism and the radical political program derived from it, such as William Whewell and Richard Jones, followed Malthus in developing an *inductive* political economy that they believed led to different political consequences. On the other hand J. S. Mill, who was a major proponent of utilitarian radicalism, accepted the deductive, Ricardian methodology because of the political program it supported. The case of Mill is especially striking because his interest in this political program apparently superseded his stated belief that method in both the natural and social sciences is inductive. In this paper I will explore the interplay between methodology, morality and politics that characterized this controversy.

## Tom Staley

### Trends in the Development of Associationism: A Comparison of the Philosophies of David Hume and Alexander Bain

Nineteenth century British empiricist philosophy focussed on a set of questions about the nature and role of sensation, which became increasingly formalized and intricate as the tradition developed. These questions - concerning how to situate sensation properly as a mediating factor between the material and mental regimes - arose within an intellectual community in which scientific, moral, and aesthetic concerns were typically addressed as an ensemble. Sensation served as a central problem for this community insofar as it provided an experiential limit for inquiry in each of these three areas. The philosophical issues generated in these discussions of the 1800's remain open and interesting today.

A number of interacting schools of thought were active in British philosophical circles in this period. The two primary points of reference for inquiry were the works of David Hume and Immanuel Kant, both of whom emphasized the analysis of sensory input in their systems of thought. From these sources, several generations of thinkers developed theoretical extensions that highlighted different aspects of sensation in the generation of knowledge, beliefs, and morals. One tradition - deriving from the work of Hume and known as Associationism - delved ever more deeply into physiological details in an attempt to clarify the characteristics of human nature. Another line of thought, often identified with Kant and referred to as the Common Sense tradition, highlighted features of logic and procedure in the acquisition of rational knowledge. Other workers incorporated ideas about language and moral 'sensibility' into their account of sensation, concentrating more on concepts of human interaction than on individual knowledge per se. However, these trends were far from independent as each drew on the others as part of an ongoing intellectual dialogue.

Keeping in mind these interactions, this paper will examine the development of the Associationist position by a comparison of the central works of David Hume and Alexander Bain. Bain, writing in the 1850's, forwarded a sophisticated extension of the basic position codified over a century earlier by Hume in his A Treatise of Human Nature. By examining parallels between Hume's Treatise and Bain's two major works - The Senses and the Intellect [1854] and The Emotions and the Will [1857] - I will show how Bain incorporated information from physiological and psychological investigations into Hume's framework, thereby adding new levels of detail and specificity to the concept of 'human nature'. In this way, Bain attempted to extend Hume's discussion of sensation and perception so as to provide a better account of the roots of our mental and emotional capacities.

## Sheldon Steed

### Congestions And Remedies: Understanding Neurath's Concept of *Ballungen* and His Critique of Scientific Method

This paper takes up Otto Neurath's concept of *Ballungen* from the Vienna Circle debates on method in the 1930s. *Ballungen* are, for Neurath, congestions-ever-present clusters of concepts within the language of science that resist precise explication. Nancy Cartwright (1992) suggests that this concept emerges in Neurath's third Boat Metaphor in 1931; she argues it marks a shift in his thought and determines his "attack on method".

We will revisit Cartwright's treatment of *Ballungen*, arguing the need to clarify what exactly Neurath's concern over method is. We will suggest that for Neurath the concept does not sustain an attack on method in general, but rather resists a particular view of the function of scientific method-that logic alone may unify scientific language. Cartwright asserts that *Ballungen* is a new concept introduced in 1931 that consequently evokes a new doctrine for Neurath: there are no logically determinate connections between data and theory. As a result, Neurath can no longer hope for a system model of science by which to unify scientific inquiry. The present paper suggests that Cartwright places too much emphasis on the time and effect of the term's introduction; and that she credits that term with the force of an analytic tool that restricts what may or may not be available to Neurath's philosophy of science. Neurath's "attack on method" was not so sweeping. Neither was it so determined upon an analytic tool like *Ballungen*.

We shall draw attention to a common theme in Neurath's body of thought, of which *Ballungen* appears to be a part. This theme may foster a slightly different view of the nature of Neurath's attack on method. His papers in the 1910s exhibit similar concerns to those in the 30s over the precision of statements in scientific hypotheses: that any system of hypotheses necessarily has fuzzy boundaries; and that purely logical or mathematical analysis generates scientific claims only within a precisely delineated field. Method alone is therefore insufficient to account for the complex web that makes up scientific theories. Thus, we argue that the introduction of *Ballungen* in 1931 does not dramatically alter Neurath's view of method. By recognizing the concept within a broader theme it loses force as a critical tool in the thirties. Indeed, let us suggest that *Ballungen* may be best not viewed as an analytic tool at all, but rather a description of the state and nature of concepts - a description held rather consistently by Neurath.

The point of this endeavor is not to split hairs or even challenge Cartwright's more general analysis. Rather, it aims at a clearer understanding of Neurath's criticisms of method. Indeed, it is hoped that this approach will afford us the means to forge an image of how the historical analysis of science might be reconciled with scientific method to provide the type of complete (though by no means final) picture of the scientific enterprise that Vienna Circle logical empiricists seemed to be working for.

**David Stump**

### Getting the Logic into Logical Empiricism: Nagel's Early Study of Formal Axiomatic Systems and the Creation of the Philosophy of Science

Who established mathematical logic and meta-logic as tools for philosophers of science in America? Before members of the Vienna Circle arrived on the scene, a fertile ground already had been established by the 'American Postulate Theorists'- Huntington, Veblen, and Young- mathematicians who disseminated Hilbert's ideas and raised the standards of mathematical discourse. In philosophy, Royce was well aware of developments in logic, however the key figures to be followed here are Ernest Nagel and his dissertation advisor Morris Cohen. Cohen and Nagel wrote some of the earliest philosophical discussions of formal mathematical systems. Nagel applied the idea of formal axiomatic systems to a standard topic in the philosophy of science, the conventionality of measurement and of simultaneity in relativity theory. Although he is now seen as embodying ahistorical, logical method in the philosophy of science, Nagel's early work belies his later image, for it includes significant historical studies of mathematics and logic and shows the strong influence of pragmatism (no doubt stemming from his friend Sidney Hook as well as his mentor Morris). Nagel's application of the idea of a formal axiomatic system to debates over the interpretation of Poincaré's conventionalism will be my case study for the application of logic and meta-logic to a standard topic in the philosophy of science.

Since the axioms of a formal system are assumed, rather than proven, they can be taken to be somewhat arbitrary or conventional (that is, of course, if any pretence to a priori knowledge or intuitive certainty is abandoned). Poincaré's geometric conventionalism can be seen as a special case of this general feature of formal axiomatic systems, in which alternative metric geometries are seen as indistinguishable models of a more general axiomatic system, group theory or topology. This interpretation seems to have begun in France, since it appears first in Louis Rougier's 1920 book, and then in Jean Nicod's of 1924, although the young Ernest Nagel seems to have hit upon the same idea independently in one of his first published articles (1929). Max Black later presented and extended Rougier's interpretation in 1942.

Since Poincaré's conventionalism had become a standard topic in the philosophy of science, it is not surprising that discussions of spacetime conventionalism drifted from Poincaré's original text. However, under this interpretation, conventionalism not only lost its connection to Poincaré, but also lost any connection to physics, and indeed lost its specificity altogether, being presented in a very general form that applies to all axiomatic systems in general, not only to the metric of space-time. Thus, instead of a philosophical interpretation of a specific physical theory, conventionalism became part of the standard conception of scientific theories that is generally associated with Carnap's viewpoint. Science was seen as consisting of a linguistic and/or logical part and an empirical part, so that pure and applied geometry could be clearly divided. One thread of the story of this transformation of conventionalism, involving an anachronistic but influential interpretation Poincaré's conventionalism will be highlighted here in order to shed light on the transformation of the philosophy of science as its center moved from Vienna to America.

### David Sullivan

### One of the Legacies of Philosophical Modernism

While most of the polemical parts of the analytic legacy have long since faded from view the sense of disciplinary coherence and methodological rigor remain intact. Not only is this sense internally cohesive, it is also untouched by any competing program or alternative approach. Indeed, most academic programs in philosophy (of whatever adherence) exhibit a peculiar agreement about both the nature and scope of their discipline. This agreement is founded in a shared acceptance of the view that philosophy is concerned with certain (philosophical) problems. This description, while obviously circular, is widely felt to be neither problematic nor viciously circular: rather, it is a badge of pride, regarded as a sign of the discipline's liberality and flexibility. Indeed, no one should presume to specify just what sorts of problems might come under the purview of philosophical investigation, particularly since these may be by-products of unanticipated advances in the sciences. Of course, this holds true despite the fact that certain problems are perennial (or, at least, persistent), having come down to us from the Greeks. If pushed, most would harmonize any seeming divergence here by claiming that, in all events, philosophical problems are of a relatively abstract nature (and obvious to skilled practitioners in the field). It is, hence, ultimately the level or the degree of abstraction that characterizes the problems that may accrue to philosophy.

In this paper, I hope only to successfully hypothesize as to why this view came to suggest itself so universally and so naturally to thinkers in the field. It is a commonplace that this self-conception is of (relatively) recent vintage: Hacking astutely labels it "the 1911 thesis" and notes its fundamental relation to the analytic program. (Given that so much seems to rest upon acceptance of this thesis, one wonders why it has not come under more scrutiny.) The account I wish to suggest, in broad outlines, is as follows: the late idealists continued with the orthodox Hegelian bent toward a peculiar kind of historicism -- one which was not genuinely historical but instead engaged in a kind of rational reconstruction regarding the emergence of certain key concepts in philosophy. (This is why, among other things, they insisted that the history of philosophy was not only genuinely but preeminently a philosophical task. Of course, here they were aided by an impulse toward both teleology and necessity ("reason in history") in constructing their narrative.) As the rational reconstruction of the products of reason itself, this approach gave little sway to the individual personalities (who, after all, simply functioned as convenient vessels for the workings of some larger, alien purpose). As this approach waned in its appeal (characterized as "one-sided"), some neo-Kantians proffered an alternative: while reason's proper study remains itself (per Kant), historicists failed to do justice to the contributions of individual personalities, whose very essence made reference to individual accidents of immediate circumstance. In contrast to the Hegelians, the context of discovery supplanted the context of justification (hence, the move was made from a rationalistic historicism to genetic psychologism). What became relevant was philosophy as a value-theoretic response to very particular circumstances by representative individuals: or, "worldviews."

Philosophical modernists rejected both historicism and this new-fangled psychologism: the story of philosophy concerned neither concepts nor worldviews but problems. It is the focus on problems made it possible to here (as elsewhere) "extrude thoughts from the mind" precisely because problems admitted of both a trans-historical and a trans-psychological treatment. But in this way, history of philosophy (in any form) became inessential to the project called philosophy.

### Iulian Toader

### Non-Propositional Aspects of Carnap's Quasi-analysis

In this paper I scrutinize Rudolf Carnap's formal method of *Quasianalysis* (*Q*), showing the inadequacies of two of its interpretations: Nelson Goodman's one as a symbolic method of manipulating elementary experiences and Thomas Mormann's one as a representational process that involves structure-preserving mappings between the flux of elementary experiences and the domain of constituted qualities. In order to surpass their limitations I propose a diagrammatic reconstruction, which takes advantage of the fact that the concept of *Lokalzeichen* (local sign) is no longer ignored. Carnap's *Q* is a formal procedure by which the subjective spatio-temporal-qualitative flux of experience is to be mathematized, i.e. logically organized, in order to be made objective, as an appropriate fundament for any scientific conceptual construction. *Q* is a synthesis presented in the linguistic form of an analysis. It stemmed from Whitehead's principle of abstraction, but also from set-theoretical topological developments of Felix Hausdorff's. Goodman interprets *Q* in a symbolic manner and fails to render similarity the meaning given by Carnap. He uses the quasianalytic frame in his theory of symbolic notation, where he explicitly avoids any diagrammatic representation, discussing only linear concatenation of symbols. Mormann thinks of *Q* as being framed by a general theory of representation, and uses it as a prototype for a theory of similarity structures, which are interpreted topologically. If *Q* is a structure-preserving mapping, one can continuously build qualities out of similarity structures. Mormann, too, overlooks the crucial detail that defines Carnap's similarity: spatial agreement of qualities. Here enters the stage the concept of "local sign", which gives the coordinates of a quality within the experiential flux. Close values of these coordinates make two qualities similar. This concept has a long history in the German psychology of the $19^{th}$ century. Carnap seems to have taken it from Wundt, but the notion was introduced by Hermann Lotze in his theory of local signs, as a way to localize visual impressions. I propose to consider the elements of *Q* as some sort of diagrams, graphically structured by local signs, and to view *Q* as a formal method for extracting information out of them. It is very much like a procedure of construction and retrieval that will allow the computer implementation of *Q* as a diagrammatic information system.

## Thomas Uebel

### *Einheitswissenschaft* as Wholesale *Ersatz* for Metaphysics?

The second panellist considers a different aspect of the naturalism problematic: Frank's persistent defense of the epistemological naturalism he shared with Otto Neurath against claims for the need for metaphysical propositions that cannot be supported by empirical evidence. The need for such propositions, Frank argued, can be obviated by due attention to the history and sociology of science and the psychology of scientific inquiry. This paper pursues the Frank-Neurath model through its changes into the 1950s and considers its compatibility with Carnap's approach and Frank's later debate with Feigl on realism. The paper includes and closes with a discussion of the question whether, throughout, Neurath's and Frank's anti-apriorism relied on a fatally flawed verificationist conception of meaning.

## Chuck Ward

### Emergence and Epigenesis

This paper examines the biological roots of the concept of 'emergence.' The concept of 'emergence' has been of central interest in the philosophy of science and the philosophy of mind for over a century. Historically philosophers and scientists that admitted the existence of emergent properties ('emergents') were in direct conflict with others that held the view that all properties of a system, including "higher-level" properties of complex entities such as organisms, are reducible to the properties of the fundamental parts of that system. Emergentism was opposed to mechanistic materialism. Mental states were often presented as the paradigm emergents (e.g. Broad 1925). But the concept derived from debates over the origin and development of biological form.

Emergentism was a fairly popular view within philosophy and some branches of the biological sciences in the first decades of the twentieth century (see, for example, Bertalanffy 1933, Driesch 1908, Morgan 1923). In that period literature on emergence and emergent evolution was being produced rapidly. But in mid-century emergentism waned. Recently the concept of emergence has regained some popularity (see, for example, Bechtel and Richardson 1983, Kauffman 1993). Ironically, in some (though by no means all) contexts the label 'emergent property' is used to denote precisely those properties of complex systems that are open to reductive explanation (e.g. Searle 1997). The fact that the concept is applied in varying, sometimes contradictory, ways is not new. Recent attempts (e.g. Cunningham 2001) to provide a taxonomy of different senses of 'emergence' have precedents in earlier efforts from the heyday of emergentism in the 1920s. In 1926, Authur O. Lovejoy addressed the International Congress of Philosophy, presenting what he described as a "prolegomena to any future discussion of 'emergence'." In that address he endeavored to provide an analysis and classification of different senses of 'emergence.' The fact that he built his analysis around the distinction between preformation and epigenesis is indicative of the biological origins of the concept. The present paper will examine those origins. It will focus particularly on (1) the important link between the concepts of 'emergence' and 'organization', and (2) the development of a species of emergentism as an alternative to mechanism and vitalism in the 1920s and 1930s.

## Michael White

### Deep Time and the Genres of History in Britain, 1815-1860

In this paper I will argue for the idea that early 19th-Century Britain can be productively addressed as an era of temporal disruption affecting both the genres of natural and civil history. Without invoking totalizing explanations such as the rise of historicism or epistemological breaks, I will explore the concept of deep time, as developed by Charles Lyell in *Principles of Geology* (1830-33), in terms of its status as a nature-culture hybrid and as implicated in the genres of the gothic and historical novel, civil history and popular science. Deep time, accepted as the temporal horizon of modernity, encompasses both the history of the civil and the natural worlds and as a consequence the implications of deep time implicate the representation of both realms. The attempts to mark this blank temporal horizon both for the history of the earth and for civil history were complex and controversial and thus provide a useful site for understanding the relations between narration, civil and natural history.

The focus of this paper will be the cultural impact of deep time on the writing of civil and natural history, largely to the exclusion, though not ignorance, of the considerations of professional geology, institution building and similar topics as treated in the work of Secord, Rudwick, Porter et al. In order to redress the characterization of Charles Lyell's *Principles* as culturally central yet professionally marginal, I will begin to analyze the interrelationships between civil and geological thinking. For example, Lyell, the geologist, cites the classicist Neibuhr as a methodological model and yet distances himself from civil history. Conversely, Thomas Carlyle's *French Revolution* (1837) uses Huttonian geology as the master-metaphor for his discussions of social change. Taking these transgeneric concerns seriously will help to show deep time as a problem of boundaries between natural and civil time which persists without adequate closure throughout the 19th century. The physicist Lord Kelvin, whose attacks Lyellian deep time in the 1850's and 60's focus as much on the heat death of the universe, the thermodynamic critique of a Lyell's supposedly steady-state earth as on the narrative and theological implications of the virtual eternity of deep time unmarked by meaningful instances of divine Providence or narratable events. For Kelvin, the macrocosm of natural time must be marked in order to mirror the microcosm of civil time.

Furthermore, civil history, even conjectural history, ultimately derived its authority from the citation of witnesses as the basis for veracity. Consequently, deep time, unwitnessable by most definitions, created problems for the genres of natural and civil history. The representation of past worlds had largely been the province of civil historians and the recent developments toward detailed representations of quotidian social realities and psychic interiority resulted in similar, and impossible to fulfil, expectations for the representation of the planetary past as well.

Thus, the problem of evidence and witness of the deep past could be elided through visual representation, as Rudwick's *Scenes from Deep Time* (1992) effectively argues. But both civil and natural historians and novelists invoked rhetoric concerning "resurrecting" and "recreating" the past as historical practice. The paleontologist and comparative anatomist, Cuvier, the self-described "magician of the charnel house," comprised his practice through reference to his ability to bring extinct creatures to life through detailed anatomical reconstructions while Charles Lyell, the popularizer of deep time and uniformitarian method cites the classicist Niebuhr as an analogous scholar with the "bliss of creation" as the aim of his geological writing. Historians describe the ideal of their historical practice as one of "resurrecting the dead," while novels like Mary Shelley's *Frankenstein* depicted the literal resurrection and amalgamation of the dead through the application of science. The debates on vitalism and the boundaries between life and death- literally and metaphorically- were blurred and embodied in the life of the variously extinct creatures of Cuvier, the historical dislocation of the *Vampyre* of Dr. Polidori, Frankenstein's Monster, and Charles Lyell's description of the eventual return of a global era of the deep past and Niebuhr's "revival" of ancient Rome to name only a few suggestive examples.

Lyell's formulation of deep time in 1830, can be understood on multiple levels as an important continuation of the problem of representing the relation of the human to the natural worlds.

## Lambert Williams

### Models, Simulation and Phenomenology in Physics: Some Remarks on Peter Galison

In his seminal work *How Experiments End*, historian of physics Peter Galison provides a clear historical argument against the common view of theory as more ‘fundamental’ than experiment. He shows that a theoretical overhaul need not imply any overhaul in the purportedly ‘lower level’ domain of experimentation, and suggests that it is more accurate to treat theory and experiment as distinct sub-cultures of physics with their own internal dynamics and a interrelationship too complex for the traditional ‘top-down’ picture to frame properly.

However, whilst on the one hand *How Experiments End* does away with the *global* hierarchy placing theory on a higher epistemological pedestal than experiment, there remains on the other hand a rather striking *local* hierarchy inside these two domains. Theory, for example, is constrained at the highest level by such lofty metaphysical concerns as unification, and only then at a lower level by gauge theories, with modelling and phenomenology sitting at the bottom of the constraint ladder.

This local hierarchy is substantially revised in Galison’s later work *Image and Logic*, where pidgins, creoles and the trading zone are put forward as more satisfactory analytical tools. This later work also sets out a condemnation of both positivism and antipositivism for aiming to establish a single narrative line for the relationship between theory, experiment, and instrumentation.

The paper I am proposing examines the neglected status of models, simulation and phenomenology, drawing heavily on case studies from twentieth century solid-state physics. Reflection on these case studies suggests that the local hierarchy of *How Experiments End* is untenable, and also that the revised picture of pidgins, creoles and the trading zone in *Image and Logic* must be given a very weak and heuristic interpretation if Galison wishes to avoid lapsing into exactly the ‘single narrative line’ problem, endemic to both positivism and antipositivism, that he intended to overcome.

## Vladimir Zeman

### On the Neo-Kantian search for invariance and Cohen's Infinitesimalmethode

Hermann Cohen is generally regarded as a founder of one of the two most important schools of German neo-Kantianism, the so-called Marburg School. There has lately been a marked growth of interest in his philosophy, primarily in Europe and with a focus on his social philosophy, ethics, and philosophy of religion. AIthough any attention to Cohen is better than none, I believe the current return to his position does not correspond to Cohen's original intention, his self-assessment, or to the development of his school, including its crucial role in German philosophy between the 1870s and 1914. I believe that the concept of transcendental method is doubly pivotal both to Cohen's interpretation of Kant's as well as to the development of his own philosophical system. I further believe that the proper understanding of this concept in its turn requires a careful analysis of at least two additional issues: the concept of possible experience and the role mathematics plays in constituting scientific knowledge. After 1883, Cohen identified the latter theme with infinitesimal calculus. My paper begins with a review of Cohen's original position, followed by an analysis of its "decline" because of external criticism as well as perceived internal difficulties. Here stress will be placed on recovering the position from what has been said about it.The second part of the paper will deal with Ernst Cassirer, Cohen's most famous disciple, as a way of assessing the fate of Cohen's own concern for a more accessible conception of invariance. The third and last part of the paper will consider salient differences in the way mathematicians and philosophers understand the idea of invariance. I will pay particular attention to Cohen's apparent disregard for hints about a proper approach in Kant's writings. The paper will end with some discussion about Cohen's inability to consider changing views of the relation between philosophy and science during the period he was interested in these questions.

## Gabor Zemplen

### Classification of Systems of Hypotheses - Otto Neurath on the History of Optics

The works of Otto Neurath (1882-1945) have received increasing interest in recent years. The paper attempts to contribute to our understanding of Neurath and the appreciation of his work by closely studying two of his less known works on the history of Optics, both written in 1914 (*Prinzipielles zur Geschichte der Optik, Zur Klassifikation von Hypothesensystemen*). The pieces followed his earliest works in Logic and preceded his critique on Spengler. These early works, while showing the influence of Whewell, Duhem, and others, also depart from the conventional views. They echo the views of the era - for example - on the status and importance of Newton’s *Opticks*, but on many instances differ significantly from contemporary accounts. On the one hand the paper tries to highlight these unconventional aspects of the early Neurath’s views, and to summarize Neurath's ideas on scientific theories, on the role and aim of the history of science. On the other hand, the paper contributes to the historiography of Optics, by comparing Neurath’s work to similar studies, highlighitng both the common features and the unique aspects, and by giving an evaluation of the works from our present day perspective. With this, I hope to answer Neurath’s call for a systematic study of the history of science, for a satisfactory classification of rival theories and his aim to find commonalities and thus better to see the differences between them.